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EDITED BY

GEORGE E. DAY, F.R.C.P.,

CHANDOS PROFESSOR OF ANATOMY AND MEDICINE IN THE UNIVERSITY OF ST ANDREWS,

ALEXANDER FLEMING, M.D.,

PROFESSOR OF MATERIA MEDICA IN QUEEN'S COLLEGE, CORK,

W. T. GAIRDNER, M.D.,

PATHOLOGIST TO THE ROYAL INFIRMARY OF EDINBURGH.

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*Contributions to Pathology and Rational Medicine.* By John Hughes Bennett, M.D., F.R.S.E., &c.

No. XV.—Clinical and Histological Researches on Cancerous and Cancroid Growths.—(Continued).

Colloid Cancer.

The term colloid (from *colla*, glue) is applied to collections of gelatine which not unfrequently occur in the body. They resemble glue, calves' foot jelly, gum arabic, and similar glutinous compounds, of greater or less consistence, and are found in masses varying in size from a minute point to the size of the human fist, or even larger. In colour colloid matter may be greyish, of a light or dark yellow, brownish, reddish, and rarely green or black. It may be clear and transparent like amber, or semi-transparent and opaque, resembling honey. It may be found disseminated in a fibrous texture, giving it a pearly aspect, or it may constitute the contents of distinct cysts. It is one of the most common constituents of compound encysted growths of the ovary, and is not unfrequently seen in cysts of the kidney and follicular swellings of the skin.

Colloid matter, distributed throughout a fibrous structure, presents numerous loculi, filled with a grey, or amber-coloured, glutinous matter, sometimes transparent, at others opalescent or semi-opaque. On examination with a microscope, the matter is occasionally found quite structureless, or exhibits only a finely molecular appearance. Under these circumstances the term *colloid tissue* has been applied to it. At other times numerous nucleated cells, in various stages of development, are found in it as a blastema, and we observe that the

growth has a tendency to spread. This is *colloid cancer*—the *gum or gelatiniform cancer* of some authors—*carcinoma alveolare* (Muller) —*cancer areolaire pultacé* (Cruvelhier). It occurs most commonly in the peritoneum surrounding the stomach and large intestine, in the omentum and ovaries. It is occasionally found in the bones, and rarely in the uterus, liver, and kidneys. The following Observations illustrate the nature of this form of cancer:—

OBSERVATION XXIV.—*Colloid Cancer of Omentum, and the Peritoneum lining the various Abdominal Viscera—Compound Encysted Colloid Tumours of Ovaries.*

Mrs K., æt. twenty-eight, had no children, consulted Mr Woodhead in October 1845, considering herself to be in a pregnant state. She thought that she had been in the family way about seven months; during which time, however, the catamenia had appeared regularly, but were scanty. There was a visible enlargement of the abdomen, but no particular examination was made. In November she complained of pains in her back and abdomen, and passed very restless nights. These symptoms were treated by palliatives, and attributed to the spurious pains so common in advanced pregnancy. In the beginning of December an examination of the abdomen was made, which was considerably enlarged, so much so as to resemble a woman about the eighth or ninth month of utero-gestation. A distinct fluctuation was perceived, and an indurated tumour which remarkably resembled a fœtal head. About the commencement of January 1846 she was examined *per vaginam*, when the *os uteri* was felt unusually high, which led to the suspicion that she was not pregnant. At this time the peritoneal fluid was much increased in amount, and the abdomen was very tense. The respiration was considerably embarrassed, and the nights rendered restless from dyspnœa. About the middle of January Dr Simpson was consulted. The following day paracentesis was performed at the umbilicus, which was unusually prominent, and a wash-hand basinful and a half of fluid was drawn off. This fluid was of a reddish colour, and contained numerous flocculi and white nodules. The former were seen on a microscopic examination to be composed of several corpuscles, both nucleated and granular, similar to those hereafter to be described, entangled together in groups by numerous filaments. There were also many blood globules. The white nodules were entirely composed of filamentous tissue, containing several fusiform corpuscles.

The breathing was much relieved by the tapping, but from this time she could not carry on her usual occupation, which she had hitherto done. The pains in the back and abdomen continued: a febrile action was excited; the pulse rose to above 100; the nights continued restless: no vomiting; and a cachectic state made its appearance. The wound did not close, but continued to discharge a reddish fluid, which afterwards became dark, often mixed with flocculi, and latterly with a quantity of jelly-like substance. About the second week in February the tumour was felt hard, unequal on the surface, and apparently attached to the parietal walls of the abdomen anteriorly. On placing the ear over the abdomen a distinct to and fro friction murmur could be heard. Fluctuation could also be felt at the lower part of the abdomen. Two other openings now took place in the umbilicus, from which a continual discharge escaped. The other symptoms continued; the pains could only be relieved by morphia, and she sunk exhausted on the morning of March 3.

Sectio cadaveris, March 9, eight P.M.

The body was much emaciated; head not examined.

The thoracic organs were healthy.

Abdomen.—On cutting through the peritoneum, about a pint of dirty yellow purulent fluid escaped. The pelvis and abdomen inferiorly were occupied by an encysted tumour, which had burst anteriorly, exposing several cysts containing an amber-coloured jelly-like matter. The peritoneum superiorly and

interiorly was united to a firm tumour underneath by gelatinous adhesions, which were easily broken through. This tumour occupied the whole breadth of the abdomen, and extended from the ensiform cartilage to midway between the umbilicus and pubis. It varied in thickness from one to two inches, and was evidently a growth in the substance of the omentum, being attached superiorly to the stomach and transverse colon, lying over the intestines, and terminating inferiorly in a rounded edge. When removed from its attachments the tumour weighed three pounds thirteen ounces. To its anterior and posterior surface were attached numerous shreds and patches of chronic lymph. On cutting into it in various directions, it presented numerous cells, varying in size from a pin's head to that of a large pea, filled with a clear, glistening, gelatinous matter, surrounded by a firm fibrous substance or mesh-work. In some places it was of a greenish hue, in others of an ochrey or orange yellow. Here and there the meshes of the tissue were tinged of a greyish hue by black pigmentary deposits.

The peritoneal membrane lining the abdominal parietes was coated with a layer of the same colloid cancer about two lines in thickness, covered with patches and flocculi of lymph. This layer could in many places be stripped from the serous membrane below, which appeared to be healthy. Between the liver and diaphragm a mass existed half an inch thick. The spleen was surrounded by a similar layer. The lymphatic glands in the neighbourhood of the pancreas and stomach were much enlarged, and apparently quite converted into the colloid cancer. A mass surrounded the uterus more than an inch thick, circumscribing the cavity of the pelvis. The lumbar glands and *appendices epiploicæ* of the large intestines were greatly enlarged and similarly affected. On cutting into the liver, spleen, and kidneys, they were found healthy; the peritoneal membrane covering them being alone affected. The peritoneum surrounding the pyloric extremity of the stomach, and extending a third over the surface of that viscus, was upwards of an inch thick from deposit of colloid cancer. Its muscular and mucous coats were healthy. The small intestines were united together by bands of chronic lymph, of a bluish-black colour from pigmentary deposit. Anteriorly they were covered *en masse* by a layer of lymph, of greenish colour, about a line in thickness.

Both ovaries were the seat of a compound cystic tumour, that on the left side was in circumference about the size of the crown of a hat. When viewed anteriorly, it presented the appearance of a mass of trembling calves'-foot jelly, with numerous blood-vessels ramifying upon it. Shreds of the external sac were seen attached to it here and there, which had evidently sloughed, and in some places been adherent to the abdominal parietes. Posteriorly the fibrous sac was still entire, and presented numerous rounded elevations, marking out the forms of distinct cysts. On incising this mass it was found to consist of numerous cysts, the walls of which were very thin, but richly supplied with blood-vessels. They for the most part contained a trembling transparent, gelatinous substance, generally of an amber colour, but here and there of a brownish-red. Imbedded in some of this jelly, there existed opaque white masses, resembling blanc-mange, or thick cream. In other places the gelatinous matter might be observed semi-transparent, and of a light yellow colour.

The tumour of the right ovary was about the size of a small orange. In the fibrous capsule anteriorly was a round perforation the size of a fourpenny piece, from which a similar amber-coloured jelly to that which existed on the opposite side could be squeezed. On section it was found entirely full of cysts, all filled with the same gelatinous matter.

Microscopic Examination.—On making a thin section of the peculiar growth on the peritoneum, it every where presented a network of transparent filaments, forming loculi of various sizes; the filaments surrounding one opening often running in those forming others—Fig. 64. Some of these loculi contained nothing but a perfectly transparent fluid; others contained cells, generally of oval form, although a few were round or caudate. They varied in their

longest diameter from the 1-50th to the 1-30th of a millimetre in length. They all contained a nucleus of oval shape, varying in its longest diameter from the 1-200th to the 1-80th of a millimetre in length. The larger of these contained an oval nucleolus. These cells occurred in groups, and many of them floated loose in the fluid squeezed from the section—Fig. 64. Here and there numerous fat granules, and compound granular masses and corpuscles, existed in groups. On the addition of acetic acid the fibres became somewhat more transparent, the external walls of the cells were partly dissolved, while the nucleus was unaffected. The dirty yellow purulent matter was composed of broken down pus corpuscles, mixed with innumerable granules and molecules.

Fig. 64.

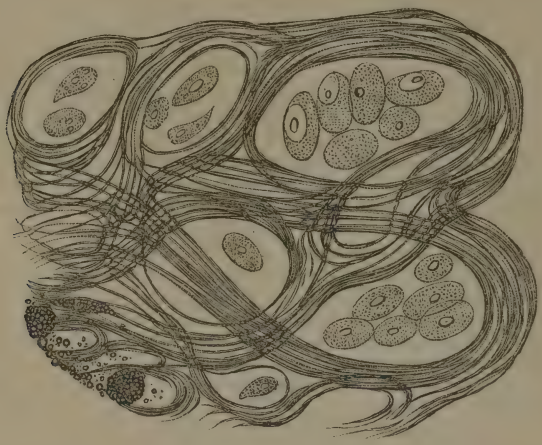

Fig. 64. *Appearance of a thin section of the colloid cancer of the peritoneum, described Obs. XXIV.*

The lymph attached to the omental tumour and parietal walls of the abdomen was composed of filamentous tissue, interspersed with the same broken down corpuscles and granules which have just been alluded to. It was also studded over with crystals of the triple phosphate.

The amber-coloured transparent jelly within the cysts of the ovaries, at first appeared to be structureless. On careful examination, however, with the rays of light directed obliquely, very faint cells could be distinguished of an oval form, varying in their longest diameter from the 1-80th to the 1-25th of a millimetre in diameter. Some of these were distinctly nucleated—Fig. 76. A few oily granules could also be seen isolated and in groups.

The white opaque creamy matter in the cysts was entirely composed of minute granules and molecules, united together by delicate filaments. These granules were of two distinct sizes. Those in one group being generally about the 1-500th, those in the other being about the 1-600th or 1-700th of a millimetre in diameter—Fig. 74. These granules were densely grouped together, and in some places constituted granular masses, similar to those seen in inflammatory exudation.

In the light yellow semi-transparent gelatinous matter delicate filaments could be seen crossing each other, together with numerous faint cells, round in shape, about the size of pus corpuscles, containing from one to six granules—Fig. 75.

The walls of the cysts themselves were composed of fibrous tissue, more or

less covered with numerous granules. In a few places only could granular cells be seen.[1]

Remarks.—This case presents the largest quantity of colloid cancer present in one individual, that, so far as I am aware, has ever been recorded. It is worthy of observation, that it was only found on the free surface of the peritoneum surrounding each organ, but leaving these untouched. Even the masses which covered the pyloric end of the stomach in no way produced constriction of the organ, or disease of its muscular coat. I am inclined to think, that the colloid masses in the ovary were in nature the same as those found in the fibrous growth of the peritoneum. Although not so much loaded with cells, it contained several in progress of development. A series of preparations illustrative of the morbid changes in this case, is preserved in my collection. The colloid matter of the ovary, which when recent was perfectly transparent, has been rendered opaque and white by the action of the spirit, while that on the peritoneum has remained unaltered.

Observation XXV. — *Colloid Cancer of Peritoneum, Stomach, and Ovaries.*—Marion Greenhill, æt. thirty-one, was admitted into the Royal Infirmary, Oct. 17, 1846, under the care of Dr Paterson. She dated the decline of her health from a difficult labour she had about three or four months previously. About a month subsequently she began to lose flesh, and became languid and jaundiced. At this time also, a small round swelling appeared in the left iliac region. She soon after felt a swelling at the epigastric region, accompanied by a burning heat shooting through the back. The abdomen also commenced to swell. For the last six weeks the urine has been scanty, with frequent desire to micturate, and for the last two weeks she has experienced excessive vomiting. On admission, no tumours could be felt, but there was considerable distension, with some tenderness of the abdomen. There was thirst, white tongue, weak pulse, scanty urine, and constipation. The treatment was palliative only. She died at the end of November, the emaciation and debility increasing, occasional vomiting, and latterly symptoms of pleurisy and pneumonia.

Sectio Cadaveris, Dec. 2.—Skin of yellow cachectic hue—extreme emaciation—abdomen distended by fluid.

Head was not examined.

Thorax.—Heart small—felt very dense—coronary veins very turgid. About four oz. of dark brown turbid serum, containing flocculi of lymph, were effused into the right pleura. The pleura, over the lower lobe of the same lung, was coated with patches of recent lymph. The inferior portion of this lobe was partly grey, partly red on section, from hepatization, and presented two indurated portions the size of a walnut, and of a plum colour, as if from recent extravasation of blood. There were about six oz. of serum in the left pleura; the inferior margin of the lower lobe of left lung also hepatized, but to a less extent than the right. A few patches of recent lymph also covered its pleural surface.

Abdomen.—The peritoneal cavity contained about a gallon and half of dark yellowish brown clear fluid. Under the right lobe of liver a pouch was formed by a band of membranous lymph, stretching from its fissure to the anterior abdominal wall, which contained about a pint of fluid similar in its character.

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[1] For figures illustrative of the structure of encysted tumours of the ovary, the writer must refer to a paper of his inserted in the *Edin. Med. and Surg. Journal* for January 1846. A few of these are given at the end of this communication.

The whole of the peritoneal surface was of a dark red or purple colour, mottled with round and square patches of a white colour, varying in size from a small pea and under, to three-fourths of an inch square. The dark colour of the peritoneum was owing to a covering of chronic lymph, deeply tinged with blood, which could be removed in layers from the surface of the intestines. The white patches consisted of the healthy serous membrane shining through the red exudation on its surface. Scattered over the peritoneum were numerous rounded semi-transparent masses of colloid cancer, resembling in appearance gum arabic, and varying in size from a small pea to that of a hazel nut. Over the transverse or descending colon, these masses were very common, and closely aggregated together. The peritoneal surface of the diaphragm was covered with similar masses, some of them as large as an almond, and its posterior portion adhered strongly to the liver, through the medium of the same deposit, a continuation of which accompanied the vena cava in its passage through the liver, and surrounded it to the thickness of a quarter of an inch. The spleen was united to the diaphragm and descending colon by firm bands of chronic lymph, and its surface was studded over with round semi-transparent gelatinous masses, the size of a pea. Internally the above organs were all healthy, the disease being confined to their peritoneal coverings. The pancreas and kidneys were also healthy.

The pyloric end of the stomach was felt to be hard and greatly enlarged, constituting a dense tumour, and the transverse arch of the colon was closely united to it, and drawn up towards the greater curvature by a mass of colloid cancer, extending between the parts. The coats of the stomach along the lesser curvature, and the pyloric extremity, were contracted and thickened half an inch by the deposition of the same cancerous matter.

Both ovaries were converted into solid tumours, about the size of a small orange, but deeply nodulated and irregular on their surfaces. On section, they seemed composed of a sarcomatous mass. In the left, this enclosed three cavities about the size of a bean, filled with a gelatinous matter, resembling in colour and consistence trembling calves'-feet jelly.

Fig. 65. Fig. 66. Fig. 68.

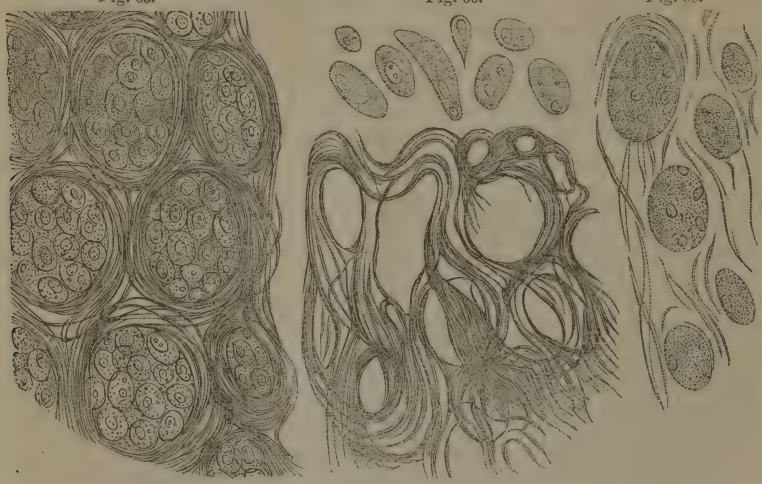

Fig. 67.

Fig. 65. *Section of the colloid cancer described Obs. XXV., showing the loculi in the fibrous structure and the contained cells.* Fig. 66. *Several cells isolated,* Fig. 67. *Fibrous stroma deprived of the cells by pressure and washing.* Fig. 68. *Section of the growth treated with acetic acid.*

Microscopic Examination.—On making a thin section of the morbid growth, it presented the same general appearance of a fibrous stroma, forming loculi, and containing cells, as in Observation XXIV. In some places, however, the loculi were completely crowded with cells, as seen in Fig. 65. The cells also presented exactly the same character as in the last case, but in some places were more numerous. Several are represented isolated, Fig 66. Granules and compound granular corpuscles were collected together here and there in groups in the substance of some sections. Firm pressure between glasses, and washing with water, got rid of the cells from a thin section altogether, so that the fibrous stroma appeared as represented Fig. 67. Here and there also, the morbid growth seemed to be wholly composed of colloid tissue, no cells being visible in the amber-coloured fluid. On adding acetic acid, the fibrous texture became more transparent, the cell-walls were partially dissolved, while their nuclei remained unchanged.—(Fig. 68.) In the ovaries the fibrous stroma was more dense, but presented here and there a cystic structure, as in the last case. The colloid matter also was rendered turbid by acetic acid.

Remarks.—In this Observation we observe the same disease as in the last, which, however, had not proceeded to the same extent as regards quantity, although the development of the corpuscles in some places was more exuberant. On comparing Fig. 65 with Fig. 1,[1] the resemblance of structure becomes striking. On comparing the two sections when treated with acetic acid, however—Fig. 68 and Fig. 2—a marked difference is exhibited in the fibrous element of the growth. In the former, the one now under consideration, no oval or elongated nuclei were present; in the latter, exhibiting ordinary cancer of the breast, the presence of these nuclei renders it probable that the fibrous element is developed in a different manner. As far as my observations have carried me, permanent nuclei are never present in colloid tissue.

For an opportunity of examining the excised breast in the following Observation, I am indebted to Professor Miller. An account of the case was kindly given to me by Dr Cornwall of Leith.

OBSERVATION XXVI.—*Colloid Cancer of the Mamma—Excision—Cure.*

Miss S———, æt. fifty-one, of firm stout make, robust, and of a ruddy healthy complexion, in March 1847 discovered, on trying on a pair of new stays, a tumour in her right breast. It becoming uneasy and painful, she came to Edinburgh and had medical advice; a prescription was given her for medicine to take inwardly, and for a lotion to be applied to the part. She was advised to return home to the country, and persevere in the use of these medicaments for several months.

The growth continued to increase in size. At the beginning of the present month it seemed to be about the size and shape of a walnut; its position was about an inch to the outer and upper side of the nipple, unconnected, *i. e.* not adherent, to the muscle beneath, or the skin over it; no trace of affection of the lymphatic system. In consistence it was firm to the feel, but not particularly hard; pressure caused pain; the surface was felt to be botryoidal or nodulated, as if peas were imbedded into it; poised in the hand it had not much weight; the breasts full and fat. Two kinds of pain were complained of, described as dull gnawing pains, and sharp shooting pains—the latter

[1] No. for October 1847.

sometimes extending to the shoulder and down the arm of the affected side; the former felt only in the swelling, not constantly, but coming on at intervals. The general health of the patient quite good. The catamenia ceased three years ago. She supposes that this breast may have got a blow in the year 1840, when she had a collar-bone broken, and was otherwise injured by the falling of a bridge which she was crossing in a carriage.

The mammary gland and tumour were removed by excision on the 15th of April by Mr Miller. The patient made a rapid recovery, and is now, June 10th, in excellent health.

Examination of the tumour.—Embedded in the fatty tissue of the gland was a firm growth the size of a hen's egg, which on section presented a bluish-gray surface. It was composed of numerous loculi or cysts, the fibrous partitions of which resembled, in some places, those on the surface of a cut orange. They were filled with a pearly, transparent, gum-like fluid, of the consistence of tolerably firm jelly, trembling on the knife. The individual loculi or cysts varied in size from a pin's head to a large pea. The margins of this growth were tolerably circumscribed, a distinct line of demarcation existing between it and the yellow fatty substance of the gland. Here and there a small nodule of the same colloid substance, about the size of a pea, was separated two or three lines from the chief mass, and embedded in fat.

Microscopic Examination.—A section of the morbid growth with a double-bladed knife, exhibited exactly the same fibrous structure, arranged so as to form oval and circular loculi as in the last case, and I have not thought it necessary to figure it again. The contained colloid substance was in some places colourless and structureless, in others loaded with very transparent delicate cells. On squeezing out, or scraping from the surface some of the gelatinous matter, it was seen to contain masses of cells grouped together, as in Fig. 69. The nuclei were generally very apparent, and seemed at first to be embedded in a quantity of granular matter. By careful management of the light, however, the nuclei could be observed to be surrounded with delicate walls compressed together. Some of these groups were loaded with fatty granules, evidently deposited between the nucleus and cell wall. Fig. 69 also represents some of the cells isolated, as in Fig. 66, but more transparent; a few are elongated, approaching the caudate form. Scattered among the transparent colloid matter were masses of fatty granules, often agglutinated together in rows. On the addition of acetic acid, the cells, both isolated and in groups, became more transparent—Fig. 70; and in some of the most transparent gelatinous matter, delicate fibres were formed by precipitation—Fig. 71.

Fig. 69. Fig. 70. Fig. 71.

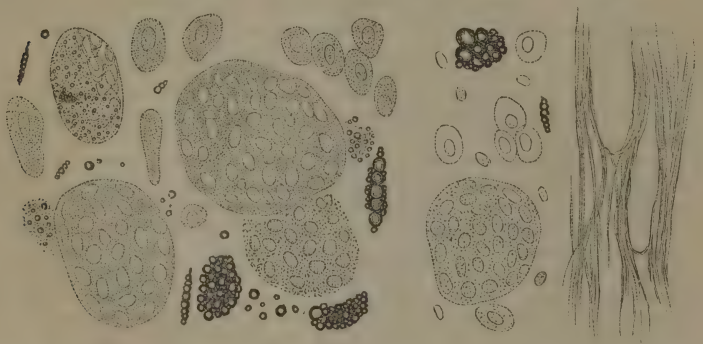

Fig. 69. *Cells, isolated and in groups, with granules of fatty matter, squeezed from the colloid cancer described Obs. XXVI. Fig.* 70. *The same after the addition of acetic acid. Fig.* 71. *Fine filaments precipitated in colloid matter on the addition of acetic acid.*

Remarks.—The colloid cancer in this Observation seems to me to have been in an earlier stage of development than in the two former ones. The groups of cells exactly resembled masses of young epithelium, which led me to examine whether any were attached to the inner surface of the cysts. After making numerous sections, however, with the utmost care, with a sharp double-bladed knife, I could not satisfy myself that the cells bore any relation to the inner surface of the fibrous structure, or that there existed any where an appearance of an epithelium layer. On the contrary, the nuclei were every where scattered throughout the substance of the colloid matter, surrounded by faint cells, and, as we have seen, were easily separated or turned out by pressure, and by washing. The formation of filaments in the colloid matter on the addition of acid, is a fact worthy of observation in this instance. It did not take place in the colloid cancer of the two former observations, but only in the colloid matter in the ovary in Obs. XXIV.

Observation XXVII.—*Colloid Cancer of the Mamma—Excision—Cure.*

In September 1847, I received from Mr Page of Carlisle a portion of a tumour he had excised from the breast. The following is the account he has been so good as to give me of the case :—"*Carlisle, 7th October* 1847.—My dear Sir,—The patient from whom the last tumour was removed is a female domestic servant, forty-seven years of age, still menstruating regularly, thin, but not of healthy aspect, but has seldom been the subject of indisposition. A year since she first accidentally discovered a hard tumour, the size of a bean, in the upper part of the left breast, which gradually increased in size, but was not attended with pain until about a month before its removal; during that period, however, she suffered much from occasional attacks of severe lancinating pain. The tumour (September 20th) was the size of a pigeon's egg, hard, and apparently quite distinct from the substance of the breast in which it was imbedded; but when removed it was found to be less circumscribed than had been supposed, as distinct prolongations of the tumour were seen extending chiefly in the direction of the nipple, which was not at all retracted. Of the general structure of the tumour, you can accurately judge from the portion I sent you. The wound healed rapidly; but there is now, less than three weeks from the operation, a very suspicious hardness of one of the axillary glands, at present not larger than half a pea, which is the seat of occasional 'pricking' pain.

From the throbbing pain in the former case, together with the rapid enlargement of the breast, it was for a considerable period looked upon as an abscess; at the time I first saw it, however, the woman suffered comparatively little pain, and that was generally more of a burning than of a lancinating character, although she spoke of occasional severe 'prickings through the breast.'—I am, dear Sir, yours very truly, W. B. Page."

In a letter from Mr Page, dated June 6, 1848, he says the cure has been complete up to that date, and she follows her usual occupation of a domestic servant without inconvenience.

Description of the Portions of Tumour received.—I received two portions of the tumour, each about an inch long and broad, and about half an inch deep. They were of a brownish purple colour, composed of a fibrous basis, containing here and there small cysts, the largest not above a line in diameter, filled with a dark brown, semi-transparent colloid matter. In one or two places there were a few gritty particles of calcareous matter.

Microscopic Examination.—A section of the morbid structure presented the same general characters as in the three last cases, namely, a fibrous net-work, forming loculi containing colloid matter. The fibrous element, however, was greater in quantity, and the cysts or open spaces consequently more separated from each other. No isolated cells could any where be discovered; but on squeezing the growth, the contents of the cysts could be removed entire, presenting round or oval masses, as in Observation XXVI. In masses so separated, no nuclei or cells could be seen, even with the most careful management of the light, nor was the existence of an investing membrane at all evident. The whole resembled a finely granular round or oval mass. One of these is figured alone on the left of Fig. 72. On adding acetic acid to a thin section of the tumour, the filaments became more transparent, and the contents of the cysts, while they retained their finely molecular appearance, permitted the existence of distinct nuclei to be observed—Fig. 72. These were of an oval or slightly flattened form, about the 100th of a millimetre in their longest diameter, scattered throughout the substance of the colloid matter. No cell wall was any where to be observed. The calcareous points were composed of irregular semi-crystalline masses, soluble in the mineral acids.

Fig. 72.

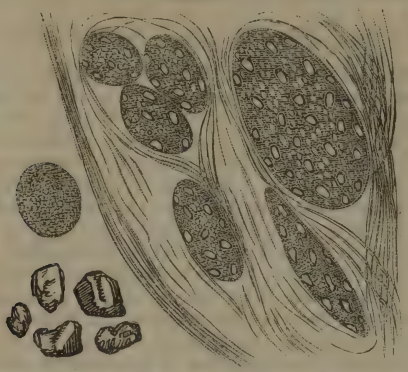

Fig. 72. Appearance of a section of the colloid cancer described Observation XXVII., after the addition of acetic acid. To the left of the figure is seen a round molecular looking mass squeezed from a cyst before acetic acid was added. Below, masses of earthy salts.

Remarks.—In this specimen of the disease, the cell formation evidently presented a less advanced condition than even in the last Observation, and we may fairly suppose that it furnishes us with an example of its earliest development. Nothing but isolated nuclei scattered through a finely molecular blastema was observable, and here, again, no connexion could be made out between these nuclei and the internal surface of the individual cysts.

The four specimens of colloid cancer we have now examined, point out to us that its fibrous element differs materially from that in other forms of cancer, and never presents any traces of having been formed from cell development, or through the agency of nuclei. It seems more probable that they are formed by precipitation, or the coagulation of the albuminous principle originally dissolved in a fluid blastema. The origin of the colloid matter is still shrouded in obscurity. Even for a knowledge of its chemical principles, we

must still look to future research, although it has been supposed to consist of some peculiar form of albumen or fibrin, probably the former. The facts detailed, however, show, that while under some circumstances acetic acid produces little or no effect upon it; at others it causes a copious precipitate, which assumes the form of filaments, which are identical in structure and properties with those forming the basis of the morbid growth. The corpuscles are evidently formed in the colloid matter as in a blastema; first by the formation of nuclei, around which cell walls are formed, which then present all the characters of cancer cells.

In colloid encysted tumours of the ovary, the fibrous structure presents two different arrangements. In one, cyst is formed within cyst; in the other, the whole growth is split up into divisions by partitions—(Fig. 73). In the former case, the cysts are often lined with a distinct epithelium; in the latter, I have not been able to observe this. The colloid matter itself presents the same general characters as those described in connexion with colloid cancer, sometimes forming filaments on the addition of acetic acid, at others not. Occasionally fatty granules may be observed of different sizes occurring in groups, in the very centre of colloid masses, as in Fig. 74; at others pale, round, and oval nuclei, as mentioned Obs. XXIV., more or less mingled with delicate filaments—Fig. 75. In some places there are seen faint nucleated bodies—(Fig. 76). In some compound cystic formations of the ovary, the epithelium separates, and the individual cells from endosmosis swell out, and

Fig. 73.

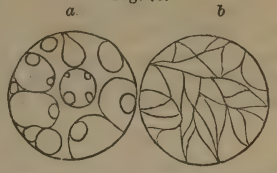

Fig. 73. Two diagrams, showing the arrangement of the fibrous element in ovarian tumours; a, cysts forming within cysts; b, cysts formed by partitions of the fibrous stroma.

Fig. 74.

Fig. 74. Groups of granules found in the centre of colloid masses in the ovary; a, granules of irregular shape; b, granules generally about 1-500th of a millimetre in diameter; c, others half the size.

Fig. 75.

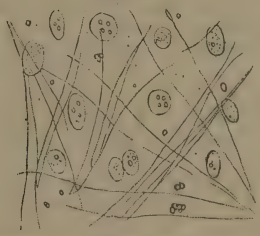

Fig. 76.

Fig. 75. Round and oval nuclei, with filaments in semi-transparent colloid matter of the ovary. Fig. 76. The same, with delicate oval corpuscles in an early stage of development.

present all the characters of a cancer cell—(Fig. 77). Sometimes these are rendered many-sided from pressure, when their analogy to epithelial cells becomes more apparent—Fig. 79. Careful observation also, will generally enable us to trace their origin to the internal lining of the cyst, as exhibited in Fig. 80.

Fig. 77. Fig. 78. Fig. 79. Fig. 80.

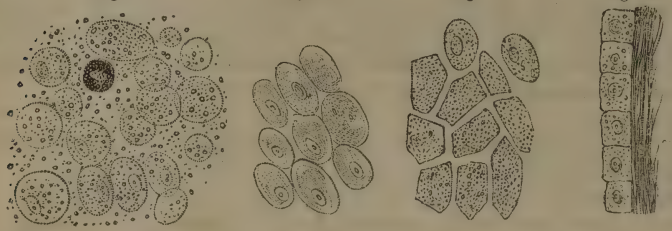

Fig. 77. Well developed cells in colloid matter of the ovary. Fig. 78. The same after the addition of acetic acid. Fig. 79. Similar cells many-sided from compression. Fig. 80. The same cells attached to the internal surface of the fibrous cyst.

ARTICLE II.—*Cases of Hysterocele; with Remarks.* By JOSEPH BELL, Member of the Faculty of Physicians and Surgeons of Glasgow, Lecturer on Botany, Anderson's University.

(*Read at a Meeting of the Glasgow Medical Society, March* 1848.)

HERNIA of the uterus is a very rare affection; few cases have been recorded. It seems to occur under three forms:—

1st, The protrusion taking place through the inguinal canal.

2d, The outlet being the crural opening.

3d, A separation or rent in the abdominal walls.

Two cases of the last kind of hysterocele having come under my notice, I have taken the present opportunity of bringing the subject before this meeting of the Society.

CASE I.—June 2, 1843. I was requested to visit Mrs P——, who had been eight days previously delivered of her fifth child. I found her much exhausted; pulse quick and small. She complained of intense pain of abdomen, on examining which the uterus was found protruding through a rent in the linea alba, extending from pubis to near ensiform cartilage. A portion of intestines was also protruded, but these were easily reduced; with the uterus, however, more difficulty was experienced. Compresses and bandages were applied, but had to be removed in consequence of the aggravation of pain which the pressure caused.

The patient died on the 4th instant. No post-mortem inspection was permitted. She was a tall slender person, of a very relaxed frame; so far as I could ascertain, the hernia had no existence previous to her last confinement.

CASE II.—On the 5th of May 1846, I attended Mrs M—— at her fourth confinement. At five o'clock A.M., after a tolerably severe labour of sixteen hours' duration, a fine female child was born. As soon as the cord was separated and the child handed to the nurse, in accordance with my usual practice I applied my hand to the abdomen, when, to my astonishment, I found the anterior

superior part of the uterus protruded through a rent in the linea alba, which was completely torn through from the ensiform cartilage to pubis. The uterus felt so large and firm, that little doubt existed of its containing another child.

On making a vaginal examination a second set of membranes was detected, on rupturing which the child was ascertained to present with nates to mother's abdomen. The uterus was pressed back into abdominal cavity, and firmly maintained in this position with both hands until the child (a male) was born; this occurred half an hour after the first birth.

The conjoined placentæ came away immediately—the uterus contracted firmly, and descended into the hypogastrium.

A compress was placed on each side of the rent along its whole extent, and a bandage firmly applied.

A smart attack of peritonitis occurred, but it was fortunately subdued by the ordinary treatment. Three months after delivery I carefully examined the abdomen, and could detect no trace of the rupture.

This woman is of small stature. The great distension to which the abdominal parietes were subjected from the uterus containing two very large children, must have stretched the fibres of the linea alba to their utmost extent, the contraction of the abdominal muscles during labour producing the complete separation of the tissues. The only circumstance which I noticed attending the rupture, was, that, almost immediately before birth of first child, the patient complained of a burning pain in the abdomen and a sensation of faintness; but as the child's head was at this time partly protruded through os externum, and the complaint being made during the pain by which the head was wholly expelled, no attention was given to her statement.

I find a case related by Boivin and Dugés very similar to that of Mrs M——.

A woman who had been in labour three days, experienced on a sudden violent pain, with a sense of laceration in the abdomen, followed by extreme weakness. J. L. Petit found, on examination, a ventral rent or rupture extending from the umbilicus as far as the pubis, and another from the umbilicus to the ensiform cartilage. The lower one was so considerable that the recti muscles were separated from each other by a distance of nine or ten inches, allowing the uterus to completely protrude.

There are recorded several cases in which the hysterocele has taken place through a ruptured cicatrix. For example, Ruysch met with a case of a woman who had been the subject of a large abscess followed by a cicatrix just above the pubis, having become pregnant the cicatrix gave way, allowing the uterus to protrude.

Rousset states, that he has seen the same occurrence after a first Cæsarian section.

Dr Ashwell, in his excellent work on the diseases of women, mentions a similar case.

Instances of inguinal and crural hysterocele are related by Sennert, Doringius, Lallemand, Chopart, and Murat; but not having observed any of these forms I shall not enter into details regarding them.

The treatment in cases of ventral hernia is very obvious,—replace-

ment of the uterus, compression, and bandaging, with confinement to the recumbent position for weeks. Indeed, in every form of hysterocele reduction ought to be practised if possible. In cases of pregnancy, such as that related by Dr Ashwell, the treatment which he and Dr Butler adopted was successful. The uterus was raised up, supported firmly with the hands, and pressed backwards into abdominal cavity. In a case of the inguinal variety, Saxtorph merely raised up the fundus uteri and labour was terminated favourably, but the uterus remained permanently protruded; no means, however, were taken to effect reduction after birth of child.

Boivin and Dugés recommend the Cæsarian section to be performed, if the raising of the fundus does not allow delivery to take place. They inform us, that this succeeded in the hands of Rousset in a case where the labour afterwards took place spontaneously, notwithstanding a considerable increase in the size of the hernia.

In the event of strangulation, Davis, Boivin, and Dugés recommend immediate recourse to the proper operation for the removal of the stricture.

ARTICLE III.—*Contributions to the Pathology of the Kidney.* By WILLIAM T. GAIRDNER, M.D.

PART I.—PATHOLOGICAL ANATOMY. (Continued from p. 865.)

II.—*Lesions affecting chiefly the Vascular System of the Kidney.*

IN passing to the consideration of the morbid changes which occur in the vascular system of the kidney, the conditions of sanguineous congestion and extravasation on the one hand, and anæmia on the other, would fall to be described first in order. But the simple hyperemic and anemic states of the organ have been noticed so fully in the anatomical introduction (April No. p. 716,) that little more remains to be said on this subject.

Congestion followed by permanent obliteration of the Capillaries of the Cortical Substance.—Under this head I have to describe a form of lesion in the kidneys which, although certainly of less frequent occurrence than those characterised by exudation, is of a pathological and practical interest in no way inferior to any other.

The appearances most characteristic to the naked eye of this form of lesion, are those so admirably figured and described by Rayer as the second form of his "néphrite albumineuse." The kidneys are generally increased in size, sometimes very remarkably so. Their consistence varies; they are sometimes more flaccid than in the natural condition, but always preserve considerable tenacity. The surface is either quite smooth, or more or less depressed and furrowed. The venous vascularity assumes to a considerable extent the stellate form; the polygons are mostly absent; and the extreme

irregularity and abruptness of distribution of the superficial veins gives to the surface a variegated or "marbled" appearance, which is quite characteristic of this stage of the affection. (See Rayer, Plate VI. Figs. 2, 3, 5; Bright, Plate II. Fig. 1.) Occasionally, also, amid this unequal injection there are to be found scattered petechiæ, indicating recent extravasations of blood into the tubes. On section the cortical substance has considerable volume, and presents a smooth, glistening, almost semi-transparent appearance, which cannot be better distinguished than by the term *waxy*. It may partake in a slighter degree of the variegated character of the surface; more commonly it is of uniform appearance, and of a yellowish or fawn-colour, sometimes verging into a pale flesh tint. The vascular striæ of the cortical substance are generally to be traced by a more or less distinct injection, and a few injected Malpighian bodies, or petechiæ of extravasation, are sometimes dispersed through the section. (See Rayer, Plate X. Fig. 3.) In other cases a little further advanced, both the striæ and the Malpighian bodies are nearly destitute of blood. (Rayer, Plate X. Fig. 1; Bright, Plate II. Fig. 1.) The pyramids frequently retain their normal vascularity; sometimes, however, they are of a pale colour, and their bases are indistinctly marked,—a condition which indicates the progress towards a further disorganization.

When a kidney in this condition is carefully and minutely injected, the greater proportion of the cortical substance remains impervious; the injection, however, can frequently be made to penetrate as far as the cortical striæ, and even to some of the Malpighian bodies. (See Rayer, Plate X. Fig. 2; Bright, Plate II. Fig. 3.)

From these circumstances it is obvious, that the lesion above described consists in an obliteration or obstruction of the capillary system of vessels throughout the organ, and a partial obliteration of the veins on its surface. There is also every probability that this condition is secondary to one in which there is a high degree of congestion of the organ. The extravasations, the occasionally injected Malpighian bodies, and the highly injected, though partially distributed, stellar veins, leave no doubt that the state of congestion described as the first form of albuminous nephritis by Rayer, is really the antecedent of the present or second form.

To any one who is familiar with the *marbled* and *waxy* kidney here described, there can be no difficulty in recognising a further stage of the same lesion, in which the organ is perfectly pale both on the surface and on section, with the exception, perhaps, of a very few stellated superficial veins. The kidney in this stage (the transition to which seems to be represented in Rayer, Plate VI. Fig. 4) is still heavy and voluminous; it acquires additional firmness and elasticity, and assumes much of the general appearance of a true non-vascular texture. It varies from a light yellow to a fawn-colour, which extends to the pyramids, the bases of which become still more confused and intermingled with the cortical substance than in the

marbled kidney. The capsule is frequently more firmly adherent to the external surface than in health.

From the pale and yellow appearance of the kidney in this stage, it is very apt to be mistaken, even by a practised eye, for an extreme degree of the fatty degeneration. A well-marked example, indeed, will hardly give rise to this error, if attention be directed to the degree of firmness of the organ, the peculiar lustrous character of the cut surface, and the entire absence of the opaque granulations of Bright, or of that dull tint which distinguishes the excessive degrees of the fatty disease. The appreciation of these characters is, however, more difficult where, as sometimes happens, exudation is also present; and the distinction which has escaped the acute observation of M. Rayer, has undoubtedly been overlooked by many other observers.[1]

The microscopic characters of this lesion are chiefly negative. There is not unfrequently an entire absence of exudation; indeed, in the most marked cases of the lesion, I have seldom found even the slightest trace of any abnormal deposit. Occasionally, however, there is a very minute quantity of fatty exudation in the tubes, generally in very small granules, and scattered throughout the organ. The tubes are either natural, or in the advanced stages pass into some of the states hereafter to be described. The capillary vessels surrounding the tubes are not visible, and in their place there is fibrous tissue, which in this form of lesion always appears somewhat exaggerated. The Malpighian bodies are also frequently seen in process of obliteration, and surrounded by dense capsules of fibrous tissue. The epithelium is frequently altered in character, but its changes follow no fixed rule.

The absence or scantiness of exudation, taken in connexion with the extent of degeneration appreciable by the naked eye, are amply sufficient characters to distinguish this lesion from the extreme stages of the fatty disease.

I shall add three observations which illustrate the different stages and varieties of this important form of renal degeneration. I do not at present mean to enter on a consideration of the symptoms, further than to say that both Bright and Rayer have figured it as being connected with albuminous urine and dropsy; and these facts entirely concur with some of my own observations. This lesion is therefore undoubtedly one form of what is commonly called Bright's disease.

OBSERVATION XI.—*Marbled and Waxy Kidneys* (without exudation) *Bronchitis—Large Abscess in Labium.*—A woman was admitted into the Royal Infirmary in a state of great exhaustion from an abscess, the size of a child's

[1] Plate VI. Fig. 4 of Rayer's work, is probably an example of the waxy pale kidney; Plate VII. Figs. 2, 3, 4, of the fatty disease. The distinction is sufficiently evident even in the plate. All of these are referred by Rayer to the third form of "Néphrite Albumineuse."

head, connected with the external parts of generation. This was opened, but a few days after admission she died.

On dissection, November 28, 1847, the lungs were found much engorged, and the bronchi full of fluid. The kidneys were greatly enlarged, and weighed 8¼ and 6¼ oz. They were nearly of the usual consistence. The surface was very uneven, from being marked all over with irregular depressions and furrows. There was a considerable amount of venous injection, but very irregularly distributed; so much so, that the surface, which was at some parts quite bloodless, had an irregularly variegated appearance (not unlike the case of Sallaway, in Dr Bright's work, Pl. II. Fig. 1.) The depressions in the surface were mostly filled with stellated veins. On section, the cortical substance was of increased volume, particularly between the pyramids; these were broad at the bases, and the line of separation from the cortical substance was quite distinct. The pyramids were well injected; the cortical substance contained, in the line of its striæ, a few points of unusually distinct injection, but was generally anemic, being of an exceedingly pale and clear salmon-colour. The surface of the section was very smooth, and neither the section nor the surface presented the slightest trace of granulations. The mucous membrane of the pelvis of both kidneys was deeply injected with arborescent vessels. The renal veins were distended with dark blood.

On examination by the microscope, a few injected Malpighian vessels were seen; the majority were bloodless. The capillaries were uninjected. The normal epithelial cells were in great abundance; but neither in these, nor in the tubes, could any fatty granules be observed, although numerous sections were made for the purpose of determining this point.

The remarkable similarity in appearance of the section of this kidney to that in Rayer, Pl. X. Fig. 3, was noticed at the time. Both of them may be considered as good and characteristic examples of this affection, in a moderately early stage. The marks of recent vascular excitement were observable in the irregular injection of the surface, the congested state of the pelvis of the kidney, and the points of injection in the cut surface. On the other hand, the absence of exudation, and the smooth waxy appearance of the section, distinguished it from the fatty kidney. The increase in size and weight must have been owing to the great abundance of secreting epithelium within the tubes.

Observation XII. *Mottled Waxy Kidneys* (with slight exudation)—*Fatty Liver—Softened Spleen—Fever and Scurvy.*—Daniel Kean, æt. twenty-three, was admitted May 19, 1848, into a fever ward. In addition to the ordinary symptoms of eruptive typhus, he suffered from painful induration and ecchymosis in the calves of both legs, with spongy and ulcerated gums (symptoms in every way similar to those of the endemic scurvy of last year). For some time before admission he had lived very poorly, chiefly on bread and coffee. He died on June 6th.

On dissection, the lungs were congested, the heart and liver paler than usual, the spleen very soft, and slightly enlarged. The kidneys were slightly above the normal size, their capsules more firmly adherent than usual. Their surface was rendered uneven by the presence of shallow grooves and irregular dimples. The venous polygons were very obscure, being mostly supplanted by stellated and arborescent vessels, which were so distributed as to give the surface an irregularly mottled appearance. Over different parts of the surface were scattered petechiæ, varying from a dark purple to a slate colour. On section, the cortical substance appeared generally of a somewhat bright fawn

colour. The cut surface was perfectly smooth, and had a waxy lustre. Here and there were seen points and lines of injection, corresponding with the vascular striæ of the cortical substance; also a few larger and deeper-coloured petechiæ. The whole of the injection was very irregularly distributed. The pyramids were paler than usual, and the line of demarcation with the cortical substance was slightly irregular. The pelvis and calyces were normal. At one point of the cortical substance, in the left kidney, there was found a patch of rather light yellowish colour, about the size of a pea, which was surrounded by a very distinct and abrupt rose-coloured border; this, on minute examination, was obviously composed of highly injected Malpighian bodies.

On examination by the microscope, the fibres of the heart were found in most places occupied by small granules, which, however, did not generally completely obscure the striæ. The liver contained oil-globules in considerable numbers, some free, and others within the epithelium cells. In the tubes of the kidneys there could be seen in many parts of the cortical substance a few minute and scattered granules, but they were mostly filled by perfectly clear cells and nuclei, which were in preternatural abundance. The nuclei were mostly smaller than usual. The inter-tubular fibrous tissue appeared unusually dense, and no injected capillaries could be seen. A few Malpighian bodies were partially injected. In the part above described as surrounded by an injected border, the tubes and cells contained granular exudation in a quantity much more considerable than elsewhere.

In this kidney, we have the waxy degeneration accompanied by a certain degree of exudation, although not to such an extent as to give any special character to the lesion. The petechiæ are interesting in connexion with the scorbutic diathesis which existed; nevertheless, the state of the superficial veins, and the injected state of some of the Malpighian bodies, along with the fact of extravasation into the tubes, appear to indicate, as in the former case, a recent condition of vascular excitement. Whether the exudation occurred as a consequence of this condition, or of the general tendency to fatty degeneration, as shown in the liver and heart, is open to question. The former view appears, from the character and partial distribution of the exudation, to be not improbable.

The succeeding case is an example of the more confirmed form of this degeneration.

OBSERVATION XIII.—*Pale yellow waxy Kidney* (without exudation)—*Enlargement of Liver and Spleen—Bronchial dilatation—Pneumonia.*—Isabella M'Kinlay, æt. twenty-three, admitted November 15th, 1847, into Ward 15 as labouring under fever. She was weak and emaciated, suffering from severe dyspnœa and cough, with fever and pain of the left side of the chest. Her complaints were of long standing, and she had had occasional diarrhœa. There were distinct physical signs of cavities in the left lung, and of general bronchitis. She was supported by wine, but continued in a febrile state, with much depression, and slight jaundice, and died December 1st.

On dissection, the heart was pale, but of natural size. In the left lung, which was much diminished in size, there were numerous large cavities. The vesicular structure of the lung was wholly obliterated, and the cavities were found to consist of dilated bronchi, the walls of which were much hypertrophied. The right lung was slightly condensed in the upper part. The liver was enlarged, pale, and firm. The spleen was large and soft. The kidneys weighed 9 oz. and 8 oz. The capsule adhered with unusual firmness. The surface was uneven and furrowed, at some points roe-like or botryoidal; but the projections here indicated had not the opacity and whiteness characteristic of the

granulations of Bright (for an illustration of this point, see Rayer, Pl. X. Fig. 10). The surface was perfectly pale, with the exception of a very few straggling arborescent veins. The whole cortical substance was of a pale straw colour, tumid, and perfectly anemic ; the pyramids were very slightly mottled from vascular injection, and appeared to present straggling radiations, which were prolonged indefinitely into the cortical substance towards the surface ; the bases of the pyramids were thus rendered very indistinct. The whole kidney was of much firmer consistence than usual. Pelvis and calyces normal.

The principal microscopic appearances were dilatation of the tubes, and entire absence of all appearance of vessels. The epithelium was in considerable abundance, and had a tendency to cohere in masses, bearing the form of the tubes. Many of the cells were also compressed. A doubtful trace of granular exudation was here and there observed ; but, after a most careful search, nothing very definite could be seen.

Some of the alterations in this kidney will be considered under the head of lesions of the tubes and epithelium. At present it is sufficient to say, that this case was considered by many persons who were present to be one of fatty degeneration of the kidneys, as well as of the liver and heart. Microscopic examination proved, however, that none of these lesions existed in any appreciable degree; the paleness of the heart and liver being, I believe, the result of anæmia, while that of the kidneys was the indication of an advanced stage of the waxy degeneration.

Connexion of Congestion with the Process of Exudation.—I have already stated, that in general the degree of fulness of the vessels stands in an inverse ratio to that of the tubes ; and that accordingly an amount of exudation, so great as to produce distension of the tubes, generally produces a corresponding depletion of the vascular system. I have likewise shown, by reference to the works of Rayer and Bright, as well as by original observations, that paleness of the cortical substance is one of the most frequent characters of kidneys containing exudation.

A moderate or small amount of exudation, however, sometimes occurs in organs which present considerable vascular injection, and sometimes even a distinctly hyperemic condition. Such cases occur not unfrequently in the latter stages of heart disease. In only one or two cases have I found the vascularity much greater than usual; in these the kidneys were of large size (weighing from six to eight oz.), and contained very little exudation. In one instance they presented on the surface the petechiæ indicative of extravasation into the tubes (as in Bright's fifth Plate); in this case there had been albuminous urine and dropsy during life, with obstinate hæmaturia during two months before death ; there was also intense dysentery and disease of the liver. The kidneys were otherwise diseased ; but the exudation was in small quantity, and in very minute granules. In another case (a woman who died of convulsions in the eighth month of pregnancy, and in whom an apoplectic clot was found in brain), the kidneys weighed four and four and a half oz. ;

they were firm in texture, and of a tawny orange colour, pretty generally diffused. This was found to proceed from the presence of blood in the tubes, intermixed with small fatty granules, which were in considerable abundance. There was, however, little capillary injection, and the Malpighian bodies were mostly bloodless. The urine in this case had not been examined.

The analogy between the results of exudation in the kidney, and those in the lung and other organs, would naturally lead to the idea, that, as in the red hepatization of the lung, the deposition of granular exudation is preceded and accompanied by vascular turgescence, so in the kidney there may be a form of the exudative process in which active congestion of the organ plays a part. Almost all pathologists since the time of Dr Bright, who have written on the diseases of the kidney, have dwelt more or less strongly on the forms of renal disease, accompanied by acute symptoms and a congested state of the organ, as being the precursors, in some instances, of the more ordinary changes, in which no increased vascularity occurs; and Bright himself has indicated "an inflammatory state of the kidney" as a probable cause of the structural changes which he described and classified.—(See Bright's *Reports*, Vol. I. p. 3). This remark is strongly borne out by the case of Evans (p. 33), to which Pl. V. refers. Christison and Rayer concur in describing a state of congestion as the commencement of many of the chronic changes, and have established the coincidence of such a state, in some instances, with symptoms of acute dropsy and albuminuria. Even supposing the opinions of these authors as to the connexion between the hyperemic and anemic changes to be, as they probably are, founded partly on theoretical views as well as on direct observation, they must be admitted to be of great weight in a question which requires for its solution so large a field of experience.

From the comparatively small number of cases of acute disease of the kidney, and more especially of acute dropsy, which have occurred in the Edinburgh Royal Infirmary since my attention has been turned to this subject, I am unable to add any observations bearing more directly upon this question than those to which I have already alluded. Whether, in the two cases above mentioned, the evidences of vascular excitement, and the exudation in the tubes, can be considered as any thing more than accidental concomitant circumstances, I am not prepared to say without further opportunities of investigation; but if these cases can be considered as examples of a state of the kidney analogous to red hepatization of the pulmonary texture, I am satisfied that this state is of much rarer occurrence, or at least more rarely fatal, in the former organ than in the latter.

I have alluded incidentally to Dr Johnson's views on this subject. He considers the deposit of fatty granules in the kidney as being invariably a chronic process, never preceded, in any case, by congestion or extravasation. On the other hand, admitting that these

states are frequently found in connexion with fatty exudation, he considers them as secondary results of the distension of the tubuli uriniferi.—(*Med. Chirurg. Transactions*, Vol. XXIX, pp. 4, 8, 9.) I have already shown (*May* No., p. 796), that this view is opposed both by anatomical considerations and by pathological facts; and, in particular, that the confirmed fatty degeneration of the kidney is usually accompanied by nearly complete depletion of the vessels. The comparative rarity of congestion in connexion with fatty exudation, under all circumstances, and its greater frequency while the exudation is still small in amount, sufficiently show the inadequacy of Dr Johnson's hypothesis to explain the phenomenon. The assertion of the exclusively chronic nature of the deposit in the kidney, is probably founded on an imperfect theoretical view of the nature and origin of fatty exudations in general.—(See *May* No., pp. 803-5.)

On the whole, the supposition which appears to harmonize best with the analogies of other organs, and also with what has been hitherto observed in the kidney, is, that the oleo-albuminous or fatty exudation is sometimes preceded and accompanied by a congestive stage of short duration, in the course of which extravasation of blood into the tubes may occur. When, however, the exudation has accumulated within the tubes to such an extent as to cause fluid pressure by obstruction, the vascular system of the organ is emptied of its blood in a degree proportionate to the amount of distension; and, as the exudation continues to increase, the stage of congestion is rapidly superseded by the development of the pale yellowish colour so frequently mentioned in connexion with the fatty degeneration. This view appears to be strongly supported by the cases formerly mentioned, in which the oleo-albuminous exudation occurs in scattered whitish patches, surrounded by a distinct vascular rim.—(*June* No., p. 864.) On examining microscopically a section of such patches, I have observed the line of demarcation between the congested and the depleted Malpighian bodies to correspond accurately with the boundary of the exudation, so as to render it probable that the congestion, originally present throughout the diseased portion, had been superseded by the presence of the abnormal deposit.

Whether any connexion exists between the development of the congestive form of exudation and the presence of acute symptoms, is a question for further clinical experience, united with careful pathological investigation, to determine. As it is evident that the earlier stages of disease in the kidney have been hitherto to a great extent overlooked, both at the bedside and in the dissecting-room, it is highly probable that many of those affections which have been considered as most obviously chronic in their nature, may in reality be the advanced stages of processes more or less acute, which have not been fatal in the first instance, or which, if fatal, have not presented lesions appreciable by the unaided eye.—(*To be continued.*)

ARTICLE IV.—*On a New Galvanic Battery.* By THOMAS WRIGHT, M.D., Fellow of the Royal College of Physicians, Edinburgh.

THE voltaic battery, as an agent in the treatment of disease, has for some time been superseded by the electro-magnetic coil machine; and it has been rather too hastily concluded that the physiological action of the two instruments is identical. The electro-magnetic coil machine, even in its most improved form, is only capable of giving a succession of shocks and sparks resembling repeated discharges from a large Leyden jar weakly charged; whereas, in the action of the voltaic battery, three distinct effects may be readily observed:—first, *a shock* both on making and breaking contact with the battery, succeeded, with some interval, by the second effect, a *momentary sensation of strong heat;* and thirdly, the *continued passage of the voltaic current,* which, as Matteucci has shown, tends to depress or exalt the energy of the nerve as it is passed in the same or in the contrary direction to that in which the nervous influence is naturally transmitted. The above considerations have led me to give considerable attention to the improvement of the battery, and to the construction of the arrangement I proceed to describe.

Having left a Smee's battery (platinized silver and zinc) in a very strong solution of sulphuric acid, I found, on attempting to use it the next day, that a great part of the zinc was dissolved, and the electrolyte saturated with the sulphate. On transferring the plates to a vessel of fresh dilute acid, a considerable quantity of hydrogen continued to be evolved from the platinized plate, although the poles of the arrangement were unconnected; and a further examination showed that a crust of anhydrous (?) sulphate of zinc had extended itself between the plates, which, possessing a low conducting power, had closed the circuit as effectually as a fine copper wire could have done. It instantly suggested itself to me, that a plate having a coat of this salt might be rendered available as the negative plate in a voltaic arrangement, and that a battery might be thus formed of one metal, zinc, alone. After a long series of experiments, which I need not detail, it was found that sheet zinc continued inactive in a mixture of nitric and sulphuric acids, in the proportion of one part of the former to five of the latter; and that a battery formed of a porous cell, including a plate of zinc in the above mixture, surrounded by another zinc plate immersed in a saturated solution of common salt, possessed considerable electro-motive power, but not nearly equal to what I had calculated on. The negative zinc plate was now removed, and, after having been well washed and brushed over with a solution of chloride of platinum, again washed, dried over a gas flame, and replaced in the porous cell. This arrangement (platinized zinc and zinc) formed a most powerful battery; a single cell, the platinized zinc of which was only two and a half inches square, gave most brilliant sparks and intense

shocks with an electro-magnetic coil, and, after the plates had continued immersed more than five hours, was found not to have declined in power. The same arrangement was also found to have the power of decomposing water, with elimination of both the gases; and a small compound battery of twelve pairs, the porous cells[1] of which were each thrust into a liqueur glass of moistened salt, and the arrangement completed by arcs of zinc, one half of each of which was platinized, gave shocks of considerable intensity. A zinc plate covered with plumbago was found to act well on first immersion, but soon declined, the coating having floated away into the solution. Copper and brass, which were also inactive in the acid mixture, developed a current more powerful than *unplatinized* zinc. A plate of sheet iron gave good results; this metal, which in concentrated nitric acid alone is inactive only for a short time, continues so in the nitro-sulphuric acid mixture.

The advantage of the platinized zinc battery is its simplicity of construction, the troublesome adjuncts of binding screws or mercury cups being unnecessary. Where several pairs are employed, the plates are bent into arcs, in such a form that the zinc, after leaving the solution of salt in one cell, rests with its platinized leg in the porous vessel of the next. The zinc, which need not be thicker than a card, is easily cut into proper shape with a pair of common scissors, and moulded on the porous troughs.

Since writing the above, I find that zinc is also inactive in a mixture consisting of water three parts, nitric acid one part, and sulphuric acid five parts, and that a pair of plates simply immersed in this solution, without the intervention of a porous cell, is capable of exciting a voltaic current, which I think will prove one of considerable energy. Care must be taken that the mixed acids are allowed to cool before use.

ARTICLE V.—*On the Specific Gravity of Chloroform, and its Superiority when Pure as an Anæsthetic.* By GEORGE WILSON, M.D., Lecturer on Chemistry.

THE object of the following communication is to make known to practical chemists and to medical men, that much of the chloroform at present in use is not furnished by the manufacturer in such a condition of concentration and purity as is essential to its producing its maximum effect as an anæsthetic; whilst, on the other hand, there is no difficulty, so far as either trouble or cost is concerned, in supplying it of great strength and purity.

Two of the physical characters of chloroform, viz. its density and

[1] The porous cells in the arrangement were formed of the bowls of tobacco pipes.

its odour, furnish convenient approximative tests of its purity; so also does its physiological action, which is sensibly altered by the presence of even small quantities of foreign matters.

First, of its density. This is given in the text-books as 1·480; but I believe that it has not escaped the notice of almost any of the manufacturers of chloroform, that its specific gravity is considerably higher than the number stated above.

In the course of some purely physical and chemical researches, carried on by myself and my friend Mr William Swan, it became essential to know what was the density of pure chloroform. For this purpose I carefully rectified two specimens of the liquid, and as their specific gravities proved to be much above 1·490, I thought it desirable, from its importance to medicine, to ascertain the densities of specimens of chloroform prepared by different makers, and purified in different ways.

The following table gives the specific gravities of nine specimens. Those marked commercial were examined as they were furnished by four different manufacturers. Those distinguished as washed and rectified, were commercial specimens which were agitated with water, and afterwards rendered anhydrous by being left for a night over chloride of calcium and subsequently distilled. One specimen was washed but not rectified.

The densities of these chloroforms were determined by Mr Swan, with a balance in my possession, weighing delicately to the thousandth of a grain. They are stated in the order of their density:—

Specific Gravity of Chloroform at 60° F.

1. Commercial,	1·4636	6. Washed and rectified,	1·4966
2. Commercial,	1·4851	7. Washed,	1·4968
3. Washed and rectified,	1·4929	8. Commercial,	1·4974
4. Washed and rectified,	1.4937	9. Washed and rectified,	1.4980
5. Commercial,	1·4939		

The mean of these numbers is 1·4909. The first specimen, however, must be excluded as of much too low density for pure chloroform. The second, too, is so much below all that follow it, that it must be set aside also. The mean of the numbers for the remaining seven specimens is 1.4956. One of these, however (7), had been purposely saturated with water, and could not therefore have the density of pure anhydrous chloroform. The specimens numbered 8 and 9, are those on which most reliance is to be placed. A fluid ounce of the former evaporated to dryness, left no residue, nor could any impurity be detected in it. The latter I washed and rectified very carefully. It was ultimately distilled twice: once from chloride of calcium, and a second time in a dry retort, to remove any trace of that salt which might have passed over in the first distillation. The mean of Nos. 8 and 9 is 1·4977.

From these results, it appears that pure chloroform, instead of having a density of 1·480, should have one of at least 1·495, and

may be prepared, as it is indeed by at least one maker in town, with a sp. gr. of 1·497.[1]

A greater density, then, than has hitherto been required, should be demanded in the case of chloroform. A high specific gravity, however, is not enough to warrant the purity of this liquid. Specimens with the density 1·494, do not in many cases produce the full and pure anæsthetic effect of chloroform containing no foreign matter.

The presence of impurity in such cases can frequently be detected by reference to the second character of good chloroform already alluded to; viz., its odour. It should possess, according to Dr Christison's apt description, "a peculiar fragrant, ethereal, apple-like odour." Specimens, however, which act imperfectly as anæsthetics, are generally wanting in the fragrance of the pure liquid, and exhibit, on the other hand, the ethereal fruity smell, modified by a sharp odour (or flavour), probably occasioned by some acid or irritating volatile compound.

I have not had the opportunity of observing, except on the most limited scale, the physiological action of this substance, so that I do not presume to make statements on my own authority in reference to the relative anæsthetic powers of pure and impure chloroform; but I was led incidentally to notice, what I believe is familiar to all who have made considerable medicinal use of it, viz., that a very small amount of impurity alters the physiological action of this body; and further, what is not I believe so generally known, that the impurities contained in commercial chloroform may be removed by a very simple process, and its anæsthetic qualities thereby greatly increased.

A relative having occasion to have a tooth extracted, I requested Mr Nasmyth to use the chloroform which I had washed and rectified, as mentioned above in reference to the specimen numbered 6, of which it was a portion. Mr Nasmyth was so struck with the unusual rapidity with which anæsthesia was induced by this chloroform, that I left a portion of it with him, that he might ascertain whether it exhibited the same superiority when used with other patients, as the quick result in a solitary case might be the result only of individual peculiarity of temperament or constitution. In further trials, however, it maintained its superiority so unequivocally, that, at Mr Nasmyth's request, I washed, and rectified from chloride of calcium, a considerable quantity of commercial chloroform. On recently applying to this gentleman to ascertain from him what estimate he now formed of the relative anæsthetic powers of the ordinary and the purified chloroform, he stated to me, in reference to the latter, "I am quite satisfied that it is superior to any other kind which I have used. A smaller quantity produces the anæsthetic effect, and it is not so liable to cause coughing or other irritation.

[1] I have been informed that one retail druggist in London advertises chloroform of sp. gr. 1·500. This number has been probably obtained by using an ordinary balance, or the hydrometer, or the hydrostatic beads, none of which, as generally constructed, would distinguish between the densities 1·498 and 1·500.

Probably avoiding the disturbance caused by the irritating cough, allows the chloroform to take effect more readily, and a smaller quantity, in consequence, answers the purpose."

The density of the rectified chloroform, which Mr Nasmyth first employed, was 1·496, a higher sp. gr., as already mentioned, than that of any of the commercial specimens examined, except one, which was quite pure.

A medical journal is not the place for speculating on the cause of so much of the chloroform offered for sale being impure. Nor, indeed, would it be easy to find data for such a discussion, even if it were admissible; for different manufacturers follow different processes in purifying the substance, and few of them are communicative as to their methods. My chief object at present is to point out to medical men, that the anæsthetic properties of chloroform may be much increased by simply agitating it with water, and then drawing it off. Rectification from pure oil of vitriol, or chloride of calcium, must further be practised, if the substance be wished in a state of absolute purity.

If chloroform, however, be freed from all other impurities, the rendering of it anhydrous may, I believe, be dispensed with. The amount of water which this substance can dissolve, is much too small to affect sensibly its physiological action. In proof of this, I mention the following result:—A specimen of chloroform was divided into two portions. The one was washed and rectified, the other simply washed, and after it had been well agitated with a large volume of water, and left for a night under it, drawn off from beneath the lighter liquid. The rectified chloroform had the sp. gr. 1·4980, the washed portion the density 1·4968. The difference here is very small, and the result shows that pure chloroform may be saturated with water, and yet show a density of 1·496, which is above that of much of the so-called anhydrous chloroform of the shops.

Two thousand parts of water are necessary to dissolve one of chloroform (Christison). The loss, therefore, in washing cannot be great, and, as Mr Nasmyth reports that less of the pure chloroform suffices to produce its characteristic effects, it may be questioned whether the washing will not effect a clear gain to the medical man.

The observation of this fact led me to hope that distillation and rectification might be dispensed with in the preparation of chloroform, and the price of that substance lowered. Messrs T. and H. Smith of Duke Street, who make chloroform of great purity, were kind enough to try a modified process at my request, but circumstances have prevented me examining the results, and any reference to it must be reserved to another occasion.

ARTICLE VI.—*Case of Acute Hepatic Abscess in a Man eighty years of age; Evacuation by the Lancet; Recovery.* By WILLIAM M'LEAN, Surgeon, Kilmalcolm.

March 8th, 1847.—I was requested to visit Mr R. Taylor, miller, æt. eighty, a stout and athletic individual, who had been affected with rigors, alternating

with a hot and dry skin, followed by profuse perspiration. These symptoms were now accompanied by obtuse pain in the right hypochondrium, and stretching upwards to behind the right shoulder. There was great tenderness and tumefaction in the hepatic region; pulse was 120, strong and bounding; appetite impaired; thirst urgent; tongue covered with a dark-brown fur; bowels constipated; urine of a dark wine colour, and depositing, on cooling, a bile-coloured sediment. *He was bled to thirty oz.; twenty leeches were applied to the hepatic region, and he was ordered a purgative powder of calomel and jalap.*

9th.—Had a restless night from severity of pain; febrile symptoms still urgent; pulse 110, wiry. The leeches bled freely; blood obtained from the arm buffed. The powder operated well; dejections bilious and very fetid. Swelling in right hypochondrium much increased, and occupying the entire extent of the liver. *A blister to be applied to the hepatic region, and repeat purgative powder.*

11th, Slept badly last night, having had several rigors. To-day the pulse is 100, and wiry; the tongue is cleaner and the thirst less urgent. The pain in the hepatic region is less severe; the swelling is more diffused, and presents obscure fluctuation. The blister rose well, and is discharging freely; the powder procured four large fetid bilious stools. *He was bled again to twenty oz.; a poultice was directed to be constantly applied to the right hypochondrium, and he was ordered a morphia draught at bed-time.*

14th, Tumefaction much increased, with evident fluctuation. An incision was made with an abscess lancet, and four saucerfuls of well-formed pus tinged with bile were evacuated. *A seton cord was inserted into the wound, and the poultice was ordered to be continued; morphia draught to be repeated.*

15th, A large quantity of matter was discharged during the night, and on withdrawing the seton to-day as much pus mixed with bile was evacuated as at the time of incision. Passed a bad night; pulse eighty, soft; thirst much abated. *Quinine with a little wine was prescribed; and to have beef tea for dinner.*

From this date the swelling and discharge diminished gradually, and the general symptoms improved. On the 17th a light bandage was applied round the abdomen. A colocynth pill procured two dark fetid stools intermixed with pus. In three weeks the discharge had completely ceased, and my patient, who was able to walk about, was rapidly regaining his wonted health. At the present time, April 1848, he continues well.

Part Second.

REVIEWS.

Principles of Medicine, comprising General Pathology and Therapeutics, and a Brief General View of Etiology, Nosology, Semeiology, Diagnosis, Prognosis, and Hygienics. By CHARLES J. B. WILLIAMS, M.D., F.R.S., &c. Second Edition, considerably enlarged. London: 1848. 8vo. Pp. 533.

THE actual state of pathology must be regarded as one of transition. Almost every school of medicine contains one or more enthusiastic labourers who are cultivating morbid anatomy on the basis of pathological histology. Almost every laboratory possesses men who are working out the problems connected with the chemistry of organic

tissues in a state of health and disease. At the present moment both pathological histology and organic chemistry are advancing with astonishing rapidity, and our own pages offer a convincing proof that scarcely a month passes without new facts being discovered and new ideas evolved of the utmost scientific importance. So quickly indeed does one new fact succeed another, that the time necessary for carrying a large work through the press is almost sufficient to vitiate its doctrines. Hence, all systematic books on pathology are necessarily premature. They are like infants born before their time; though newly formed they bear the characteristic features of old age, and rapidly sink for want of vital strength.

Five years have elapsed since the appearance of the first edition of Dr Williams' work on the principles of medicine. A copy of the second edition, now before us, is nearly twice the bulk of its predecessor. This is owing to the accumulation and addition of new facts and observations. We nowhere find, however, any important change in doctrine. On the contrary, the author tells us in his preface, "that in very few instances has it been necessary to retract or supersede the inferences and views set forth in the first edition." Knowing, as we do, how difficult a task it is to eradicate inferences and views once adopted, we are not surprised at this, although we are prepared to maintain, that very few of those regarded with so much favour by Dr Williams are capable of standing the test of time, if, indeed, they may not already be regarded as obsolete. It is, of course, impossible for us to criticise one tenth part of the doubtful and undecided questions treated of in this book; we shall, therefore, limit ourselves to the consideration of the author's opinions on subjects stated in the preface as having "been confirmed and extended by recent researches to a degree that has not less surprised than convinced him of their truth." These subjects are *Congestion, Dropsy, Determination of Blood, Inflammation, and Deposits.*

CONGESTION.

The older pathologists considered congestion to be connected either with an asthenic or sthenic state of the system, and to depend upon determination of blood on the one hand, or atony of the vessels on the other. These views are adopted by our author, who further speaks of these two kinds of congestion as being attended, the first with diminished, and the second with increased motion.

The chief causes of congestion with diminished motion are, according to Dr Williams, first, those of venous obstruction; second, those of atony of the vessels. Instances of the first from ligature or other obstructing causes are so common and self-evident, that we need not dwell upon them. Congestion from atony of the vessels may depend on numerous circumstances. It may be general, as in cases of debility, adynamic fevers, and the sinking which precedes death; or it may be local, from long continuance of a part in one position, from cold, the influence of malaria, and over excitement. Under the last head, we, for the first time, meet (p. 183) with an

allusion to a theory to which the author attaches great importance, not only in the production of congestion, but of inflammation also. We allude to the formation of colourless corpuscles in the vessels, which, combined with diminished tone of the vascular walls, causes them to be obstructed. This theory we object to *in toto;* but as we shall allude to it again under the head of inflammation, we need not enter upon its consideration at present. We need only cite a passage from Mr Erichsen's paper on Asphyxia, quoted by the author at length, who says, "Nor have I *ever* been able to discover any obstructions in the vessels in consequence of the adhesion of colourless globules to the sides,—a phenomenon that I especially watched for, and which has by several been supposed to occur."

Vessels which have lost their tone, according to the author, become inelastic and tortuous, and the very accumulation of blood in them opposes an increasing obstacle to its passage through them. This point he illustrates by some experiments in which water was forced from a syringe through brass tubes and coils of intestine, the latter being twice as long and twice the diameter of the former. The result was, that the small metal tube discharged from two to five times the quantity discharged by the larger but membranous tube. These experiments illustrate a principle that is too little considered in animal and general physics; *the loss or neutralization of force by misdirection.* In health the blood-vessels are so arranged as to make the most of the heart's propulsive power, and transfer it throughout their whole length; but when dilated, tortuous, flaccid, and otherwise altered, they misdirect and exhaust it; a sufficiency does not remain for the onward propulsion of the blood, which, therefore, stagnates and accumulates in the congested vessels.

DROPSY.

Congestion leads to an increased transudation from the distended capillaries, causing effusions of the watery and saline part of the blood, more or less impregnated with albumen, and sometimes even with fibrin, as exemplified in the fluids of fluxes and dropsies. Thus, congestion of the bronchi causes bronchorrhœa; congestion of the intestines causes diarrhœa; congestion of the uterus, leucorrhœa; congestion of the kidneys, watery and sometimes albuminous urine; congestion of the lungs and pleura, hydrothorax; of the heart, hydropericardium; of the abdomen, ascites, &c. Dr Williams considers this result to be purely physical, and the different characters of the effusion to depend not only on the amount of distension of the vessels, but on the condition of the blood. A watery state of this fluid promotes transudation, whilst a highly albuminous and fibrinous blood requires more pressure to make its watery parts pass through the coats of the congested vessels. In the latter case the fluid often contains self-coagulating fibrin, the "fibrinous dropsies" of Vogel. To this kind of dropsy the author refers the gelatinous masses of lymph often found in the peritoneal sac of the abdomen, the fluids of bronchorrhœa, mucous diarrhœa, and leucorrhœa, the

pseudo-membranous fibres occasionally effused on mucous surfaces, and albuminous urine. He considers also that hypertrophy may result from this form of congestion.

We are of opinion that Dr Williams has here confounded together two kinds of congestion, and that the phenomena he has latterly described, the fibrinous dropsies of Vogel, are erroneously ascribed by him to congestion arising either from obstruction or atony of the vessels. We will undertake to say, that there is no undoubted instance of a mere mechanical obstruction, either by ligature or otherwise on a vein, that has ever caused a true exudation. The author, indeed, refers to the experiments of Dr Robinson of Newcastle; but when we consider the injury to which the kidneys, in his experiments, were exposed, we can have no difficulty in attributing the effects to an inflammation rather than to a mere congestion. In short, we believe that two distinct lesions have been confounded together by Dr Williams,—that the pressure theory with respect to veins will not hold; and that, if it be applied to the capillaries, it ought to be considered as a result of inflammation, and treated of under the author's next section, namely, congestion with motion increased, or what, following the older pathologists, he denominates

DETERMINATION OF BLOOD.

In discussing this subject, Dr Williams first refers to examples of local active hyperæmia in health, such as the face and neck in blushing, the uterus and breasts at the period of gestation and lactation, the gums during the period of dentition, and the antlers of the stag at the season of their development. In disease, he alludes to cases of what is commonly called "determination of blood" to the head, which, it is stated, have been seen to come on first with heating of the carotids, then flushing of the face and head, suffusion of the eyes, and sensations of distraction in the head.

With all deference to the author, we cannot help suspecting some fallacy in this observation. To suppose that congestion of the brain, and, as it is said, fits of epilepsy and hysteria, are preceded by throbbing of the arteries; or, in other words, that this throbbing of the carotids is the first in the sequence of changes leading to the congestion, is opposed to all known facts on the subject. The carotids have no such power of active contraction, independent of the heart on the one hand, or the capillaries on the other. No doubt, the arteries leading to inflamed or actively congested parts, do throb in an unnatural manner; but we believe that this is a result, and not a cause. In no place is this throbbing so well observed or felt as in the finger from a festered wound, yet this is evidently a propagation backwards from the injured part. It is the result of stimuli originally applied to the extreme vessels.

The author next alludes to the action of local stimuli to the vessels, such as snuff to the nose and eyes, spices to the mouth, and food to the stomach, which, he says, produce local determinations of blood. Here we cannot recognise any "determination." We ob-

serve only an impression on the contractility of the vessels, the effect of which is to cause enlargement of them, and as a matter of course congestion or hyperæmia. Nothing more. But what does the author mean by this term determination? He tells us, that, on irritating a frog's web, we see the arteries become enlarged, supplying a larger and more impulsive flow of blood to the capillaries and veins, which all become enlarged also; and the whole vascular plexus, including vessels which before scarcely admitted red particles, then become the channels of a much increased current. "This is determination of blood."

But here again we must differ from our author on a matter of fact. We have performed the experiment frequently, but could never observe that the arteries were the vessels first affected. On the contrary, it always appeared to us, that the influence of the stimulus spread over a portion of the surface at once, that it was impossible to distinguish any difference between the effect on the capillaries and the effect on the minute arteries and veins, but that in all cases the smaller vessels (the capillaries and minute arteries) were influenced first, and the larger branches last. We hold, therefore, that the idea of "determination of blood" existing in the body is altogether erroneous, and that in no case do the arterial trunks take upon themselves the initiative in pouring blood into the minute vessels.

Dr Williams next enters into an argument to prove, that his determination of blood depends upon physical causes, upon purely hydraulic laws; but differing from him, as we do, with respect to the facts on which he rests his theory, it is unnecessary for us to attempt its refutation. The principal error, we think, lies in attributing too much to physical, and too little to vital causes, and in almost entirely overlooking that wonderfully great contractile and vital power of the capillaries, first demonstrated physiologically by John Hunter, and shown to depend upon a peculiar structure by Henle.

INFLAMMATION.

Dr Williams defines inflammation to be "too much blood in a part, with motion (of that blood) partly increased, partly diminished." The increased motion is, according to him, dependent on "determination," a subject we need only again allude to as a proof of the inconsistency of the whole theory. The "determination," that is, the throbbing of the arteries, can never exist *in* an inflamed part. It is in the neighbourhood only, where the vessel is still pervious, that the phenomenon is manifested, and we have endeavoured to show that this is the effect and not the cause. All the facts with which we are acquainted prove that the condition of the vessels actually causing inflammation is one of obstruction, with complete stoppage of blood, and it is the cause of this stoppage which constitutes the only difficult subject of inquiry connected with the subject.

Here, again, we find that the author's opinions regarding a tonic congestion and "determination," are made the basis of a complex view

of inflammation, in which, of course, the contractility of the capillaries is denied, and the influence of hydraulic laws altogether depended on. Even these powerful aids, however, do not enable him to explain the stagnation of the blood in inflammation, and he has consequently recourse to another theory, said, like those which have preceded, to be founded on well observed facts, but like them, as we shall endeavour to show, unable to bear the test of close examination. This theory provides for another mechanical cause of obstruction, originating in the blood, by the formation of an increased number of colourless corpuscles, which stick to the sides of the vessels, and block them up. This would certainly be a very satisfactory cause did it really exist; but in this, once more, the author's facts have been wrongly interpreted, and made subservient to an hypothesis which has no real foundation.

Is it then a fact, as Dr Williams and Mr Addison assert, that a large number of colourless corpuscles are formed in the vessels of the frog's web, on irritating it? Dr Hughes Bennett, in a paper published in this *Journal* for January 1847, says, No. We quote the passage in that paper which refers to this subject, so that our readers may judge for themselves :—

"In the autumn of 1843," says Dr Bennett, "we carefully repeated all the experiments described by Mr Addison and Dr Williams, with Dr Redfern, now professor of anatomy in King's College, Aberdeen. Since then we have repeated them four separate times, twice publicly in November before the class of pathology and practice of physic, and twice in June, in conjunction with the gentlemen attending the class on histology. We have just terminated a series of these experiments for the fifth time, and the results have been so decided and satisfactory, as to render it imperative upon us to publish them, and to demonstrate the errors into which the promulgators of this doctrine have fallen.

"It is true that in young frogs, and in many full grown ones fresh from the country, the colourless corpuscles are very numerous. It is equally true that in old frogs, and in many which have been long kept, they are few in number. It is in the web of such animals that, according to Dr Williams, the increase of the colourless corpuscles, as the results of irritation, are well observed. Now, we have applied salt, capsicum, and essential oils, and have never seen these corpuscles increased in number, relatively to the yellow corpuscles. When, by means of these irritants in young frogs, congestion is occasioned, all the blood globules within the vessel increase in number, and that of the colourless corpuscles is of course augmented. But the yellow corpuscles are also more numerous, and neither we nor any of the assistants (and more especially Dr Wilkinson, our assistant of this year), have ever seen that the former existed relatively in greater proportion.

"Again, in old frogs, in the vessels of which the colourless corpuscles are very few, we have frequently excited stoppage and exudation, without seeing any colourless corpuscles from the commencement of the process to the end. In other words, on the addition of acetic acid, or other irritants, all the phenomena we have described make their appearance, producing oscillation and stoppage of the current of blood, crowding together of the yellow corpuscles, without the appearance of one colourless one. It often results, however, from the action of re-agents, especially of acetic acid, that the nuclei of the epidermic cells covering the web are rendered very distinct. These nuclei are round colourless bodies, exactly resembling in size and appearance the colourless blood corpuscles. It consequently happens, that the vessels running between the two layers of epidermis present at different foci an appearance as if the

internal wall of the vessel was studded with colourless corpuscles. This, I am persuaded, is the phenomenon which has given rise to the idea of the calibre of the vessel being crowded with them. Under such circumstances, however, it may be seen, that the parenchyma of the web in the meshes of the capillary plexus is also studded over with similar nuclei, owing, of course, to the epidermis being spread uniformly over the whole web. Mr Addison says, that colourless corpuscles escape from the vessel into the surrounding tissue, a circumstance which is physically impossible without rupture of the vascular wall, although the origin of the statement may probably be explained by the facts we have pointed out.

"If further proof is required, it may be found on the examination of the circulation in the tail of the tadpole. Here the vessels are so easily excited and paralysed by irritants, that before stoppage of the blood they frequently burst, causing extravasation of blood, and yet without any increase in the number of colourless corpuscles. Moreover, not only have none of the histologists of Germany, who have carefully watched the inflammatory process, ever observed an increase in the number of these bodies; but this supposed occurrence is directly negatived by recent observations and experiments of Remak, who has shown that their increased number in the blood follows large abstractions of that fluid, and that the portions first drawn in inflammations contain very few. It may be concluded, then, that there is no increase of the white corpuscles in inflammation; no crowding together of them, so as to produce obstruction of the vessel; no escape consequently into the surrounding tissue; and that the observers who have stated these circumstances to have occurred, have mistaken the nuclei of the epidermic cells in the web of the frog's foot for these structures."—*Monthly Journal*, January, pp. 503-505.

The following is Dr Williams' commentary on the above passage—

"The presence of the pale corpuscles in inflamed vessels has been questioned by Dr Hughes Bennett, who hints that Mr Addison and myself may have mistaken for them epithelium cells lining the vessels; but the plain description above given, and which I have repeatedly verified, admits of no such interpretation. The granular corpuscles may be distinctly seen to roll sluggishly, and with a *dragtail* before they stop; which they obviously do by adhesion to the interior of the vessel. I cannot understand why Dr Bennett has failed to see so clear an appearance."—P. 266.

Now, a perusal of Dr Bennett's statements will prove that he *has* seen all the appearances described by Dr Williams. The facts themselves are correct as far as they go. What is denied is, that such appearances are the invariable forerunners of obstruction in inflamed vessels, and are the cause of the stoppage in the circulation. What Dr Bennett asserts is, that he and his assistants have again and again watched the whole process of inflammatory obstructions in old frogs, without seeing any of those white corpuscles; and, on the other hand, they have seen all the phenomena described by Dr Williams in young frogs, without inflammation or obstruction having occurred. The inference is, that Dr Williams has mistaken an accidental phenomenon for an essential part of the process, and attributed a uniform effect to an occasional occurrence. On the other hand, the only explanation offered by Dr Bennett of Dr Williams' error, is the supposition that he had mistaken the nuclei of the epidermic cells running above and below the vessel (not the epithelium cells lining the vessels, as erroneously stated by Dr W.),

which often, from the action of acetic acid, give to the vessels the appearance of being crowded with colourless corpuscles.

Our readers must judge for themselves as to the correctness of these opposing statements. Inexperienced persons, however, should not be misled by the diagram given by Dr Williams, p. 262, which he says "exhibits the appearance of a small portion of the capillaries." From the size of the vessels in relation to the blood globules they are probably small veins. For ourselves, we are by no means inclined to believe that the mechanical notions of Dr Williams, and the creation of a number of colourless corpuscles to stop up the vessels, can explain the nature of inflammation. But not to dwell upon the experiments of Remak, which appear to be unknown to the author, there is a series of cases on record, quite sufficient in itself to overthrow the theory. We allude to the four cases by Craigie, Hughes Bennett,[1] Virchow,[2] and Fuller,[3] in which the blood was crowded with colourless corpuscles in every vessel, without any of the phenomena of inflammation being present, nor any of its usual appearances existing in any of the solid tissues. With these cases also, the author seems to be entirely unacquainted.

We hold, therefore, that this peculiar theory of Dr Williams' is not only not proven, but is extremely improbable; and think moreover, that the physical and mechanical causes to which the author ascribes the phenomena, are altogether inefficient for the purpose. Whether the views of the vitalists, and more especially the vital attraction and repulsion theory of Professor Alison, be better founded, we shall not stop to enquire. Certainly it is not free from objection: but on the whole we have more faith in it than in the mechanical notions of our author.

We might dwell at considerable length on the author's account of the results of inflammation, in which the same imperfections in appreciating facts, and the same unacquaintance with the labours of modern pathologists, are every where observable, as in the points we have just noticed. But, confining ourselves to those matters, which, according to Dr Williams, have been most perfected by recent researches, we have next to notice what is said on

DEPOSITS.

The subject of deposits in or upon textures, constitutes section 6 of chapter iv. on structural diseases, or diseases of nutrition. Here we cannot refrain from observing, that nothing can exhibit so clearly the faulty arrangement of a work on pathology, and the confused notions of its author, as separating the phenomena of inflammation from diseases of nutrition. Surely that process which perverts the growth of a part, leads to formations of pus, lymph, ulceration, and gangrene, must, more than any other, be regarded as

[1] Edin. Med. and Surg. Journal, October 1845.
[2] Frorieps, N. Notizen, No. 780.
[3] Lancet, July 1846.

a lesion of nutrition. Yet Dr Williams makes inflammation and its phenomena constitute one section, and diseases of nutrition another. It is under this latter head the author describes hypertrophy, atrophy, induration, softening, transformation, and degeneration of textures, deposits, non-malignant growths, malignant growths, and disorders of mechanism. On each of these subjects we have much to find fault with; indeed none of them may be considered as communicating to us the actual state of science regarding them. What is said as to deposits will serve to illustrate this statement.

The term *deposits* is applied by the author "to matters which result from an overflow of the nutritive material beyond what is necessary to nourish the textures themselves." These matters he has divided into *euplastic*, *cacoplastic*, and *aplastic*, by which barbarous words he means substances that exhibit a perfect, imperfect, or absent organization. We are surprised to see that the euplastic deposits are formed of cicatrices, which in our opinion ought, according to the author's views, to be considered as cacoplastic. We find, however, that Dr Williams does not mean cicatrices properly so called, but regenerations of perfect tissue.

For an account of the healing of wounds the author is indebted to Carpenter's physiology, and it is to be regretted that he should have had recourse to a mere compilation, when so many masterly accounts of the process, founded on original observation, are to be obtained in the works of British and continental pathologists. But in this, as well as throughout the work, there is to be discovered a very deficient knowledge of the labours of his contemporaries, and especially of the investigators in Germany.

Under the head of cacoplastic and aplastic deposits, there is a description of true cicatrices, which we are told "exhibit various degrees of organization, some being vascular, and some not." Here, as in many other places in the work, the author confounds organization and vascularity with each other, an error that no one acquainted with the present state of physiological-pathology could have committed.

The structure of tubercle is described by the author as consisting of a "predominance of minute and often irregular granules, and the comparative absence of fibres and cells, of which mere traces are seen, at least in the older specimens. The granules are aggregated together by an amorphous material, the solidity of which gives hardness and some translucency to the mass; for acetic acid or alkalies, which dissolve this cement, loosen the granules and render them distinct. The chemical nature of granular tubercle is albuminous, with some gelatine and a little fat, the latter in very minute proportion, and occupying the centre of some granules; and the gelatine being probably the amorphous cement just noticed," p. 391. Now, these so called "granules" are, we presume, the tubercle corpuscles of modern pathologists. They cannot be granules properly so called; for the author tells us, p. 269, that a

granule appears "as a light spot, surrounded by a dark circle." Calling, then, these bodies granules, is not only contradictory, but, in the case of a structure having contents, is faulty in the extreme. Why has not the author given us the admirable description of these corpuscles by Lebert, instead of an account so vague and confused, that even few histologists can understand what is meant, not to speak of the generality of his readers? But here, again, no allusion is made to the accurate researches of Lebert, with whose labours the author seems to be wholly unacquainted. As to the amorphous matter which is associated with tubercle being gelatine, we know of no fact which can support such a statement. Most assuredly the author has not given us one. Neither has he noticed the numerous analyses of tubercle by Glover of Newcastle.

There are numerous speculations in this section with which we cannot agree, and which a more extended knowledge of the writings of other pathologists would have prevented the author from forming, especially the idea that tubercular matter may form within the vessels, p. 398; that tubercle is most common in the lungs, because they are the chief seat of the formation of fibrin, and so on.

Dr Williams tells us, that cod liver oil is assuredly the most efficacious of all medicinal agents in the treatment of cacoplastic and aplastic deposits, and is one which, after two years' constant experience in its use, is still frequently surprising him by the wonders that it occasionally works even in aggravated and advanced cases of scrofula, mesenteric disease, pulmonary consumption, chronic pnuemonia and pleurisy, and chronic rheumatism. He conceives it to act as fatty matter. The nuclei or rudimental molecules of all structures appear to consist of fat, which the oil in its highly divisible state supplies and renews in the manner most conducive to active and healthy nutrition. We are surprised to find that this explanation of the action of the oil is put forth by Dr Williams as his own, and alluded to both in the preface and in the text (p. 404) as the result of the author's reflections on the subject. Though unacquainted with modern researches made so far off as France and Germany, he might surely have known that this very theory had been formerly published by Dr Hughes Bennett in 1841, and publicly taught by him at no further distance than Edinburgh, for the last seven years.

Our limits prevent us from proceeding further. We have confined ourselves to a notice of those portions of the work considered by the author himself as being most perfect; and we need not say that if such be the correctness of those inferences and views, which in the opinion of Dr Williams have been confirmed and extended by recent researches, the other portions we have left unnoticed are open to even more objections. And such is the fact. Now, as stated at the commencement of the article, this is not altogether owing to Dr Williams, but to the imperfect state of our knowledge. The error consists in speaking too positively on subjects which are

still doubtful, and in writing a systematic book on what cannot be systematized. We had reason to expect, however, a candid and fair statement of the opinions of others; but we regret to say, that either owing to inadvertence, or to a want of information, this has not been furnished. Had the author perused the works and memoirs of Remak, Gruby, Rokitansky, Engel, Henle, Lebert, Virchow, Bruch, and other members of the physiologico-pathological school of Germany, it would doubtless have merited a different notice from us.

Notwithstanding this work contains so much that is deficient in observation, research, and correct theoretical deduction, we have no hesitation in saying, that it also possesses merits of a very high order, which will not only render it acceptable to the medical practitioners of this country, but probably make it valuable to our continental brethren. In Germany, indeed, it is likely to be even more useful than in England; for whilst its errors and deficiencies will be palpable to the foreign pathologist, the chances are, that if these do not induce him to throw the work down too soon, he will obtain from it a degree of information that none of the books published in his own country will afford him. We allude to that portion of Dr Williams' "Principles" which refers to treatment. In all that is said on this subject, we recognise the enlightened and practical physician. No doubt we owe to Germany much of what is known of pathology as a science. There, it is pushed forward with a rapidity far too great for a London practitioner. But it is destitute of that great aim which can alone make it valuable, namely, the cure of disease. In uniting theory with practice, and in thus following the great examples of the Hunters and the Bells, Dr Williams has done well. Such, we trust, will always be the chief characteristic of British works on medical science. Dr Williams has many claims to the gratitude of the cultivators of medicine; but the work we have just noticed, is sufficient to prove that these claims do not so much rest upon his knowledge of pathology, as on his powers of diagnosis and skill as a practical physician.

Traité de l'Art de Formuler, ou Notions de Pharmacologie appliquée à la Médecine. Par M. MIALHE, M.D., Pharmacien, Professeur agrégé à la Faculté de Médecine. Paris: 1845. 8vo. Pp. 518.

A Treatise on the Art of Prescribing. By M. MIALHE, M.D., &c. Paris: 1845.

THE ordinary run of treatises on the subject announced in the title of this work are, for the most part, characterised by the absence of original matter; the book before us is distinguished by the opposite quality. In Mialhe's work we are furnished with an extensive series of original experiments, having for their object, on the one hand, the

explanation of the action of medicinal substances, and, on the other, the improvement of the formulæ made use of in their administration. The "formules rationelles" of this writer are founded on a careful consideration of the chemical reactions that medicines undergo in the stomach and intestines, and are devised with the view of securing, as far as possible, the absorption of the whole dose of the remedy prescribed, in every instance where its entrance into the blood is essential to its therapeutic action.

Five chapters are occupied with general considerations on the absorption of medicines. As the fundamental proposition of his treatise, our author contends, that "every substance capable of exerting a *remote* action on the animal economy is soluble, or susceptible of becoming so in the fluids of the body" * * * "such substances alone being capable of absorption,"—pp. 17 and 22; and his researches have served to solve several apparent exceptions to this law. His experiments show that sulphur, phosphorus, metallic arsenic, the insoluble compounds of lead, antimony, mercury, silver, &c., the action of which is often cited to prove an opposite opinion, are all chemically influenced, and rendered wholly, or in part, soluble by the agency of the gastro-intestinal fluids; and to the change thus effected, we can have little hesitation in attributing their physiological action.

If, however, the recent observations of Oesterlen are to be relied upon, solubility does not appear so exclusively essential to absorption. This writer has observed that finely powdered charcoal, when administered by the mouth, finds its way into the blood, in which fluid the carbonaceous particles may be detected by the microscope. Admitting Oesterlen's observation to be correct, it is improbable that charcoal or any analogous substance could, in this way, be introduced into the blood in sufficient quantity to act upon the economy.

In relation to absorption, Mialhe divides the articles of the materia medica into two classes. The *first* class comprehends those substances which are soluble, and hence capable of immediate absorption;—it includes potassa, soda, ammonia; all the soluble vegetable acids; some mineral acids; arsenious and arsenic acids; all alkaline salts; and of the metallic salts, all those which do not coagulate albumen, as the cyanides of potassium, iron, mercury, &c.; some vegetable alkaloids, and indeed we may say all, as there are none entirely insoluble in water.

The *second* class comprises all insoluble drugs which require the intervention of the gastro-intestinal fluids to effect their absorption, and is divided into—A, substances which are rendered soluble by the *acids of the gastric juice.* This division includes all the metals except silver, gold, &c., which have no action on the economy when taken in the metallic state, all the insoluble metallic oxides, and the sparingly soluble vegetable alkaloids. B, substances which are rendered soluble by the *alkalis of the intestinal canal.* Sulphur, phosphorus, iodine, almost all the mineral acids, tannic acid, and all the

insoluble organic acids; the electro-negative metallic oxides of antimony, tin, zinc; resins, fixed oils, &c., belong to this division. C, substances requiring for their absorption the intervention of *alkaline chlorides*. This division includes all the oxides and salts of lead, mercury, silver, gold, and platinum. Their absorption may be effected throughout the entire length of the alimentary canal, or wherever alkaline chlorides are present in the secretions.

That a classification of this kind has considerable practical value is at once evident. In administering a drug belonging to division A, very little fluid should be given along with it, as a large quantity not only weakens the solvent power of the gastric juice, but also hastens the passage of the drug into the intestines, where it meets an alkaline reaction. Of course, alkaline drinks should be here avoided; while it is equally evident, that to obtain the maximum effect of a medicine included in division B, acid drinks should not be taken.

But as the quantity of acids, alkalis, and of salts contained in the fluids of the body, is limited, it follows that the action of insoluble bodies is also limited, and often bears no relation to the amount of medicine taken. We are thus enabled to understand why sixty grains of calomel should not act with twelve times the energy of five grains of the same medicine, or why so little difference should be observed in the remote effects of small and large doses of such drugs as the carbonate of the peroxide of iron, nitrate of bismuth, oxide of silver, &c.

In the use of soluble medicines which are not decomposed by the gastro-intestinal fluids, we can predict with considerable accuracy the effect which a given dose will produce, and can increase or diminish that effect by increasing or diminishing the dose. But in the case of insoluble drugs, many circumstances may interfere to prevent the chemical reaction necessary to render them active; and consequently we find that the same dose may in different individuals, and at different times, produce very unequal results. Calomel, oxide of antimony, and other insoluble medicines, afford constant illustrations of this remark. In many diseases, the secretions undergo important changes in their chemical composition, and in their power of acting on insoluble bodies, which present in consequence unusual inertness or activity, as the case may be. Many anomalies of this kind have been hitherto referred to idiosyncrasy. It is further clear, that, beyond a certain limit determined by the solvent power of the gastric and intestinal fluids, we cannot increase the remote action of an insoluble drug by augmenting the dose. When this limit is exceeded, a portion of the medicine remains undissolved, and is either expelled with the fæces; or what is very likely to happen, at least if the use of the drug be continued for some time, it accumulates in the stomach and intestines, and afterwards leads to unpleasant consequences. On this subject our author makes the following useful remarks:—

"When," he observes, "an insoluble medicinal compound is introduced into the system, and cannot be entirely dissolved by the gastric and intestinal fluids, the insoluble portion of this body, or the insoluble whole, either traverses the entire length of the alimentary tube, to be expelled with the fæces, or is arrested in its course, and lodges for a time in some of the folds of the intestinal mucous membrane. Thus, the inconsiderate employment of caustic magnesia has given rise in the stomach of the gouty to extraordinary magnesian incrustation; insoluble preparations of iron, and especially the subcarbonate of the peroxide, administered in too large doses, often occasion intestinal concretions. The accumulations may even occasionally be the direct consequence of the exhibition of too great a quantity of the medicine. But the insoluble matters thus accumulated do not all present the same dangers; those on which the vital fluids exert no effect, act on the surfaces with which they are in contact only as foreign bodies—that is, by causing irritation and symptoms of inflammation; while matters capable of becoming soluble in consequence of a change in the quantity, or in the composition of the visceral humours, may thus become active, or often poisonous, and by their absorption give rise to severe or even fatal effects.

"It is thus that calomel, given as a purgative in large doses, occasionally causes ptyalism, and deeply affects the system; so also basic or officinal sulphate of quinine, administered in the dose of several grammes daily, though at first producing no remarkable physiological effect, has all at once given rise to symptoms of poisoning, followed by death. A few glasses of tartaric lemonade have occasioned vomiting and diarrhœa in a patient who had taken protoxide of antimony some days before, it is scarcely necessary to add from the formation of tartarized antimony. Ioduretted water, given to a patient affected with a cutaneous disease, who had a short time before been taking calomel as an alterative, has given rise to most profuse salivation, occasioned by the biniodide of mercury, resulting from the action of the ioduretted water on protochloride of mercury still remaining in the system."—P. 260, *et seq.*

An important practical conclusion to be derived from the foregoing is, that insoluble agents should not be exhibited in single large doses, but in frequently repeated small doses, so as to expose the medicine to the solvent power of a large quantity of the fluids of the body. In practice this precept is frequently followed; but its *rationale* is, we believe, not generally understood.

The proportion of water taken into the stomach along with an insoluble medicine, exerts considerable influence on its therapeutic action; the solvent power of the secretions being diminished by large dilution. Theoretically, this is exactly what we should expect; but the following experiments will enable the reader to see clearly the full import of the proposition that we have just laid down. Under the influence of alkaline chlorides in solution, calomel undergoes partial conversion into corrosive sublimate and metallic mercury, to which change, as will be afterwards seen in the course of our remarks, we have good reason to attribute the physiological action of this important remedy. In four experiments calomel, six decigr, common salt and muriate of ammonia, of each six decigr, and a different proportion of distilled water, were placed in contact during twenty-four hours, at a temperature of from 40° to 50° centig. The quantity of distilled water used, and the amount of corrosive sublimate formed, in each experiment, were as follows:—

Distilled water	5 grammes.	Corros. subl. produced	24 milligs.
„ „	10 grammes.	„ „ „	19 milligs.
„ „	20 grammes.	„ „ „	12 milligs.
„ „	40 grammes.	„ „ „	9 milligs.

Clearly showing, that the quantity of corrosive sublimate produced is greatest when the alkaline solution is most concentrated.

Of the sixth chapter, two sections are devoted to the consideration of the changes which medicines are supposed to suffer in the blood, and the mode in which they act on the economy. They contain much ingenious speculation, and merit careful perusal.

The greater portion of the work contains a detailed examination of the individual members of the materia medica. We have space to notice only a few of the more important.

Sulphur.—In this drug we have an excellent illustration of an insoluble body capable of exerting a remote or constitutional action on the economy, and the question arises, How is its absorption effected? Mialhe's experiments show that, when exposed to the action of alkaline carbonates in solution, sulphur is transformed in part into the sulphuret and hypo-sulphite of the alkali, compounds which are soluble, and hence capable of absorption. It is therefore probable, that sulphur enters the circulation only in virtue of the changes which it undergoes by contact with the alkalis of the intestinal fluids; in favour of which view, it may be urged that the most active preparations of sulphur are the alkaline sulphurets, or those which contain them. Its action is much promoted by the addition of an equal part of carbonate of magnesia, which probably serves to saturate the free acids of the stomach, and so enable the alkalis and their carbonates to operate more effectually.

Mialhe advances, as another argument in favour of this view, the fact, that the energy of sulphur ointment as an anti-psoric remedy is much enhanced by the addition of the carbonate of potassa; but the latter remedy alone forms perhaps the most active application in scabies that we possess.

Iron.—In France it is a common opinion, that in the treatment of chlorosis and other affections demanding the tonic powers of iron, those ferruginous preparations only are efficacious which contain the protoxide, or the metal itself, which in the stomach is converted into protoxide; further, that the protoxide must be united with carbonic acid or some organic acid that can be assimilated, as the citric or lactic; and lastly, that all the salts of the peroxide of iron, and all ferruginous combinations with the strong mineral acids—as the sulphuric, phosphoric, or muriatic—are not assimilated, and are only useful as astringents (Bouchardat, Gélis, and Conté, *Annuaire de Thérapeutique*). Mialhe maintains, and very justly, that clinical observation is altogether opposed to these conclusions, as is shown in the recognised value of such preparations as the subcarbonate and citrate of the peroxide, the hydrated peroxide, and the permuriate

of iron. On his part, he advances the following propositions:—
1. "That all ferruginous preparations, soluble or capable of solution in the acids of the gastric juice, and susceptible of decomposition by the alkalis or their carbonates contained in the blood, may be advantageously employed in the treatment of chlorosis." This includes all the preparations of iron in common use; as the pure metal, its combinations with oxygen, the chloride, bromide, iodide, and, without exception, all the oxisalts of iron. All these suffer decomposition in the blood, the alkalis of which unite with the acid, and the base is set at liberty. This is the property that, according to Mialhe, constitutes the essential condition of the tonic (or, to speak more precisely, "reconstituant"[1]) powers of a ferruginous preparation. As the theory indicates, salts of this class are slow to appear in the urine, and if given in small quantity, cannot, with one or two exceptions, be detected in that fluid.

2. "That none of the martial compounds, soluble or capable of solution in the acids of the stomach, but not susceptible of decomposition by the alkalis of the blood, can be of any service in the treatment of chlorosis." This class comprises only a few compounds; as the ferro-cyanide of potassium, the sulpho-cyanide of potassium and iron, &c. These traverse the circulation unchanged, and soon appear in the urine.

We know, that in chlorosis the constituent parts of the blood which are deficient are the iron and the blood globules, and that these are promptly restored by a course of chalybeates. Whether, in producing this effect, the iron acts simply as a general tonic, or whether it is assimilated, and enters directly into the formation of the blood globule, the present state of our knowledge does not enable us certainly to determine. The latter hypothesis is, we think, very probable; but Mialhe, without giving any satisfactory reason, assumes it as an established fact. The arguments in favour of it are these, (1.) That the other general tonics have not the same effect as iron in chlorosis; (2.) That those chalybeates which, from their nature, are detained in the blood, are the most efficacious.

Mialhe goes on to consider the condition in which iron enters into the formation of the globule. Liebig had already shown that the iron contained in the blood was combined with oxygen, and Mialhe observes, that the probabilities are all in favour of the peroxide being the active agent; it is unalterable in the air, and its salts are in general stable, and present a red colour more or less resembling blood. If iron were assimilated only in the first degree of oxidation, it is very unlikely that the persalts would form such energetic preparations. He believes that the protoxide only becomes active by passing to the higher degree of oxidation. His ingenuity carries him a little further, and offers us the following hypothesis

[1] Trousseau and Mialhe mean to indicate by the use of this term, that the iron is retained in the blood and assimilated.

respecting the formation of the blood globule. The ferruginous salt and the albuminate of soda contained in the blood suffer mutual decomposition; there are formed a new salt of soda and the albuminate of iron, which, according to our author, constitutes the base of the blood globule. An experiment, advanced in support of this opinion, merits attention. When to a solution of albumen we add a perfectly neutral persalt of iron, no precipitate forms; but on adding to this mixture a certain quantity of chloride of sodium, a copious precipitate falls. Now, we know, that while the globules of the blood are soluble in distilled water, they are not so in a solution of common salt.

When an insoluble preparation of iron is used, it should be administered in frequent small doses, and its use continued for a considerable period, as it acquires activity by solution in the acids of the gastric juice, which exist only in small quantity. Among the insoluble preparations, he gives the first place to the metal in fine division and the carbonate of the protoxide, then come Ethiop's mineral and the hydrated peroxide. The insoluble protosalts are preferable to the insoluble persalts, on account of the greater facility with which they are acted on by the acids of the gastric juice.

The soluble preparations of iron are incomparably more active than the insoluble. Among these there are some, as the sulphate and muriate, the absorption of which is incomplete on account of their active astringent power. "Of the soluble compounds, those which have the least styptic taste are richest in iron, and are most readily absorbed, ought to be preferred; and, on these grounds, no ferruginous compound can be compared with the tartrate of potassa and the peroxide of iron."[1]—(*To be concluded in next Number.*)

Remarks on the Employment of Anæsthetic Agents in Midwifery. By G. T. Gream, one of the Medical Officers of the Queen Charlotte's Lying-in Hospital; Fellow of the Royal Medical and Chirurgical Society, &c. &c. London: 1848.

In December last, immediately after the anæsthetic effects of chloroform in midwifery were announced, a notice against it was published in the *Medical Times*, averring stoutly that anæsthesia could not be of so much advantage, as had been reported, in first deliveries, because in first deliveries the principal impediment "is," said the writer, "found in the pelvic bones and rigidity of joints." Of course, every tyro in obstetric knowledge was capable of detecting the intense absurdity of the allegation, that the *bones* and *joints* of the pelvis were the cause of delay in first labours. The author of this amusing averment was Mr Gream, a London "Late Lecturer on

Midwifery and the Diseases of Women and Children." He has stated his objections against anæsthesia in midwifery at a greater length in the present publication; and we have no doubt that, a century hence, Mr Gream's pamphlet will be prized and quoted as a literary medical curiosity in its way, and another proof of the absurd and lamentable lengths to which medical prejudices will sometimes lead medical men, whenever any thing new is proposed in practice.

We believe it is needless to encumber our pages with quotations from a work exhibiting such extraordinary unacquaintance with physiological and pathological science as this pamphlet betrays; but lest any of our readers, perchance, deem our verdict too harsh, we will give an extract at random. At p. 26, on the "latent effects" of ether and chloroform, Mr Gream illustrates these effects by the case of the young druggist at Aberdeen, Arthur Walker, who poisoned himself in February last with chloroform. The italics in the following extracts are, we beg to say, Mr Gream's own :—

"Dr Jamieson, who examined the body of Arthur Walker, in his report says: 'Lungs turgid, loaded with dark blood, and *extensively adherent to the walls*' of the chest, and the '*heart extensively adherent to the pericardium.*'[1] '*The walls of the right ventricle thin, cavity enlarged.*' That we find these appearances in individuals not unfrequently, it will be allowed; but the combination of them in a person habitually a drunkard, through the effects of etherization, at once gives rise to the suggestion that they have arisen from this cause."—(P. 26.)

These adhesions, Mr Gream proceeds to show, were probably "the *effect* primarily of *venous* congestion," produced by the chloroform inhalation; and he thinks "it is impossible to be certain," that those who have inhaled ether or chloroform "have not at *this* moment similar morbid adhesions."

We feel almost ashamed to offer a word of commentary upon such statements and reasoning as this. But surely no other man in the educated medical profession of the present day, except Mr Gream himself, could possibly believe that old and intimate adhesions between the opposed surfaces of the pleura or pericardium, such as were found in Walker's case at Aberdeen—adhesions evidently of very long standing—could have been produced by chloroform, the anæsthetic effects of which were only first discovered in Edinburgh a few weeks before Walker died.

[1] Mr Gream gives this and the other extracts from Dr Jamieson's report, both within inverted commas and in italics, thus doubly marking his wish that they should be regarded as actual and true quotations. Dr Jamieson's published report of the case (*Medical Gazette for February* 25) was the official one given in to the law authorities. Mr Gream has completely changed its meaning. The actual words of Dr Jamieson's report, regarding the state of the heart, are these, "The pericardium was generally and closely united to the heart by *old* adhesions, which had to be forcibly torn asunder with the finger," &c. Mr Gream has carefully omitted the word "old," as that word would have totally destroyed all the arguments he wished his "fair" readers to draw from the case. We need not say that this is a most unwarrantable and unjustifiable proceeding, and, we fear, must seriously compromise Mr Gream's character as a medical author.

If Mr Gream places any faith in his own reasoning (though, for the sake of Mr Gream's medical knowledge, we charitably hope he does not), he must further believe that Walker, for some few weeks before his death, was going about his usual occupations with acute or subacute Pleurisy and Pericarditis marching on in full career within him, and without producing a single symptom. Certainly Mr Gream must have uncommonly innocent and strange ideas of pathology and disease.—Patients operated on under anæsthesia die sometimes, like patients operated on without anæsthesia, though by no means so frequently. If Mr Gream will inquire at any of his surgical friends—or any one who knows pathology at the London hospitals—he will find that these surgical patients do not exhibit appearances of pleurisy and pericarditis more frequently, after ether or chloroform, than those who die without having inhaled ether or chloroform.

Mr Gream's other illustrations of the "real hazards" of ether and chloroform are all precisely of the same kind as the above, and all betray a similar want of temper and of knowledge. According to his catalogue of their "real hazards," almost every corporeal disease is the result of their inhalations, and a few mental also; for he humanely suggests to the "heartless mother," who seeks by anæsthetic means to avoid her own sufferings, whether this practice may not make an 'IDIOT' of her child; and that, no doubt, on grounds as good and just as Dr Rowley—another fashionable practitioner in London, and "Lecturer on the Practice of Physic"—gravely suggested half a century ago to the "heartless parents" of his day (when vaccination was first introduced), that an artificial cow-pox might certainly, in the human subject, ultimately lead to the growth of cow's hair upon the human skin, and cow's horns upon the human head. Nay, a coloured drawing of a case, in which he averred this last transformation to be taking place, was published by him as a frontispiece to a pamphlet against the horrors of vaccination, written in the exact spirit of Mr Gream's publication. We would suggest Mr Gream's own portrait as a fit frontispiece for his next edition. And perhaps some of his readers will recollect Byron's commentary on the poet, who, like Mr Gream, selected and sung for his hero of "An Idiot Boy:"—

> "A moonstruck, silly lad, who lost his way,
> And, like his bard, so mingled night with day,
> That all who viewed the Idiot in his glory,
> Conceived the bard the Hero of the story."

There is one subject prominently brought forward in Mr Gream's pamphlet, intended, as its pages are, to "fall into the hands of persons not belonging to the medical profession," on which we would make one parting remark. He avers that the use of chloroform is calculated to excite improper sexual feelings and expressions in women, when given during labour. Some time ago, in consequence of seeing this gross mis-statement loathsomely repeated over and over again, by the same hand, in a London journal, we made extensive inquiries among

our obstetric friends in Edinburgh, who were constantly, and with unvarying success, using chloroform in midwifery, as to whether they had, in any case, observed any thing that could give rise to such an idea. Not one of them had ever remarked any thing of the kind, or any thing approaching to it. But to those who are themselves impure in thought, every thing, in others, looks and sounds impure. And, on the present point, we quite agree in the opinion which we heard expressed by Dr Simpson at the Medico-Chirurgical Society a few nights ago. Persons, he stated, sometimes, when imperfectly under chloroform, carry on for a time the same train of thoughts, and even of conversation, as they were engaged with when the inhalation was begun; and if some obstetricians find their patients talking obscenely when chloroformed, the fault does not depend upon the chloroform, but upon the coarse and improper previous conversation of themselves or their assistants.

After what we have stated it is almost unnecessary to add, that Mr Gream makes very unscrupulous and distorted misquotations throughout his pamphlet, and revels very largely in a number of very frightful but very apocryphal cases and facts, which if inquired into, would, we doubt not, turn out as groundless as his own attempts at reasoning. Indeed, in glancing over his pages, we have sometimes felt at a loss whether to imagine that Mr Gream *really* believed in what he stated, and was so unacquainted with pathology and medical reasoning as his observations seemed to show; or whether the logical ignorance of the author was not real, but merely affected and assumed for the purpose of enabling him to make broader misstatements. At all events, this is evident from the pamphlet, that if it is to be taken as a criterion of the arguments that can be used against ether and chloroform, the success of anæsthesia in midwifery is as certain as the success of vaccination in medicine,—seeing that the lovers of pain must be desperate, and that Mr Gream, with all his anxiety to retard and decry the new practice, has not been able to adduce against it a single case or argument that any educated medical man would place the slightest degree of value upon.

The pamphlet, in fact, is only fit (to use his own expressions) for "the hands of persons *not* belonging to the medical profession;" and we owe an apology to our readers for noticing a production like the present at such length. We need only add, that an author who, like Mr Gream, is so defective in other forms of knowledge, is sadly deficient also in the knowledge of his own and of other languages. He writes, for example, more than once, "ebriety" for "inebriety,"—conjoins plural nouns with singular verbs,—spells forceps "forcepts"—sets at defiance all rules of punctuation; and translates the French expression "*salive écumeuse*," by "*squamous* saliva," instead of "spumous;" "rushing (*raptus*) of blood to the head," he translates "*ruptures* of blood," &c. &c. He is, in short, himself apparently much in the state in which he pathetically describes

a patient under ether. "He has undoubtedly lost his reasoning power, for his conduct is most outrageous. He closes his eyes" (*to the truth?*) "and foams at the mouth."—(P. 12.)

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVII.

MEETING XI.—*May 17th*, 1848.—Dr HAMILTON, President, in the Chair.

ON THE PHYSIOLOGICAL ACTION OF CHLOROFORM. BY DR GUNNING.

THIS paper consisted of illustrations, by experiment and observation, of the theory of the author, that in the mode of action on the nervous and vascular systems, the effects of chloroform were similar to those of a modified asphyxia. The conclusions of the author were as follows :—

1. That there is, after inhaling chloroform and similar substances, an impediment to the transit of the blood through the pulmonary capillaries, which impediment reacts, through the right side of the heart, upon the nervous centres; in which the pressure of the venous, as well as the want of arterial blood, induces the changes which produce the striking effects upon voluntary motion and sensation.

2. That, in all essential particulars, the full effects of chloroform are the same as those of confessedly asphyxiating causes, such as carbonic acid, hanging, drowning, &c.

3. That the same treatment is applicable to those suffering from any of these causes, namely, to renew the relation between the blood in the lungs and the atmosphere by artificial respiration, and to take off the pressure from the right side of the heart by blood-letting.

4. That, therefore, the distinction of substances into "Stimulants" and "Sedatives" is, in many cases, not scientific, seeing they produce similar symptoms and similar material effects.

Dr Simpson said, that he sincerely believed Dr Gunning's theory of anæsthesia to be entirely incorrect; but it was difficult to state, on the spur of the moment, the reasons which might be adduced against so new and strange a view of the subject as Dr Gunning had taken. But the state of anæsthesia was assuredly not a state of asphyxia, any more than the state of narcotism from opium, &c., was a state of asphyxia. Dr Simpson had often in midwifery seen the state of complete anæsthesia from chloroform kept up for one, two, three, four, or more hours, and the person immediately and perfectly recover as soon as the inhalation of chloroform had ceased. If you induced and maintained asphyxia, simple asphyxia, by closing the glottis, compressing the windpipe, or any such direct means, for as many minutes, you would have a very different result. Yet, if Dr Gunning's doctrine were true, and anæsthesia was asphyxia, you ought to be able to induce and maintain it by merely drawing your patient's neckcloth tight for the time being, or by covering over his mouth and nostrils by a handkerchief or towel. Surely Dr Gunning would not venture to reduce his theory to practice. Ville and Blandin, in their experiments on ether, had actually found the elimination of carbonic acid from the lungs increased during anæsthesia, instead of decreased as in asphyxia, and as they *a priori* expected. Dr Gunning believed the more immediate cause of chloroform anæsthesia, was an increased degree of pressure on the brain from impeded return of blood in the jugular veins. But mere increas-

ed pressure on the brain did not produce anæsthesia or coma. Send a man down thirty-four feet in a diving bell, and you have the brain through the blood-vessels (as Dr Neil Arnott has shown), sustaining an additional pressure of fifteen pounds on the square inch; but the man by this—or by a double depth and double pressure to this—is not rendered anæsthetic or comatose. In the experiments performed by a committee of this Society, in November last, on the lower animals, no cerebral congestion was found in any case, and pulmonary congestion in only one or two, by no means in many. The Society were bound to this statement by their own report. *Litera scripta manet.* Nor was there any thing like adequate proof in Dr Gunning's few and imperfect experiments on the lower animals, that the state of anæsthesia essentially produced impediment—as he supposed—in the return of blood along the jugular and other veins to the heart. Some struggling on the part of the animals operated on, would produce the distension of the jugulars which Dr Gunning supposed he had seen. Indeed, that the state of anæsthesia produced the very reverse of this effect—and the very opposite of that which was required by Dr Gunning's theory—seemed shown in a late operation of Professor Miller's. He was removing a tumour from the axillary region of a child; some of the large veins there were opened, but bled little, till the effect of the chloroform accidentally passed off and the child began to cry; then the blood flowed freely; but it became arrested again when the child, by a few inhalations, was again chloroformed. The state of anæsthesia acted like a hæmastatic; it should have produced exactly the opposite effect if Dr Gunning's theory had been true. Altogether, the explanation of anæsthesia from nitrous oxide, ether, chloroform, &c., was, he believed, to be sought for in some law of a far higher degree of generalization than that adopted by Dr Gunning. Dr Simpson stated, that he had successfully chloroformed several of the lower animals, fishes, crustacea, annelida, &c., and last year had etherized the sensitive plant (*Mimosa pudica*); and Dr Gunning's theory could not by any possibility apply to some of these beings, where yet the phenomena were essentially similar to what they were in man. He had found, for example, the common earthworm (*Lumbricus terrestris*) very susceptible of the action of the vapour of chloroform. But the phenomena in it could not be explained by impeded jugular circulation, compressed brain, &c., as it had neither jugular vein nor brain. Dr Simpson further stated, that he had found he could, with chloroform vapour, render anæsthetic individual parts of the worm merely, as the tail merely, or the head merely, or the middle part merely, the head and tail remaining unaffected. He could produce thus *local* anæsthesia in the worm by applying the vapour locally. In the human subject, *local* anæsthesia of a portion of the gums could be produced, as he had stated to the Society on a former occasion, by rubbing the part with hydrocyanic acid.[1] These and other circumstances, and the local action of chloroform on plants, seemed to Dr Simpson all to point to the idea, that we are to search for the true explanation of anæsthesia from ether, chloroform, &c., in the direct introduction of these agents into each affected part, either through the blood or juice of the part, or by actual direct imbibition into the tissues.

Professor Miller mentioned the particulars of the case alluded to, in confirmation of what Dr Simpson had stated. It certainly proved the very reverse of Dr Gunning's idea of the state of the large veins, to be true.

Dr Robertson stated a case of convulsions in a child five years old, where the attendant symptoms were those of cerebral congestion, and, after other means had failed, he subdued the convulsions immediately by submitting the child to the influence of chloroform. As the action of the chloroform passed off, the

[1] We are informed by Dr Simpson, that in several individuals he has lately been able to produce a partial, and perhaps superficial, *local* anæsthesia of the human hand, &c., by exposing the part for ten or fifteen minutes to strong chloroform vapour.—ED.

convulsions returned; and before his patient was quite relieved, and the convulsive tendency passed off, he was obliged to keep up the action of the chloroform for five consecutive hours. Next day he found the child well. He did not think this effect compatible with Dr Gunning's theory.

Dr Skae said, that in asphyxia from carbonic acid, which had been referred to by Dr Gunning as analogous to the action of chloroform, the phenomena had been proved to depend to a great extent upon the narcotic action of the gas, and narcotism differed from pure asphyxia.

A few remarks were then made by Dr Glover, Dr Pirrie, &c., after which,

Dr Gunning, in reply to Dr Simpson's remarks, urged, that mere denial of the propositions laid down was little to the purpose, especially as Dr Simpson confessed he had paid little attention to the *modus operandi* of chloroform. The case he alluded to in Professor Miller's practice, where the venous hemorrhage returned after the action of chloroform began to wear off, could not disprove the cases in the rabbits experimented on, where the venous hemorrhage was increased immediately after the use of chloroform. Both might be true, yet not contradictory. Indeed, the objection was rather a confirmation of this view; for while the pulse is quicker and stronger, and the impediment is only beginning, bleeding from the veins should be more ready; but when the action is fully developed, and therefore less blood emitted, that too with less force, from the left side of the heart, it ought to be less, as in Mr Miller's case. Exceptional observations of this kind were of little worth unless all the circumstances were at the same time weighed. He would like to know how Dr Simpson could explain the phenomena in hanging, except upon the view of impediment in the lungs? There is nothing absorbed, and yet there is immediate loss of sensibility and motion, besides all the other effects of chloroform, as extreme pleasure, flashes of light, hissing sounds in the ear, &c. Also, how should artificial respiration, by taking away the arrest at the lungs, so easily restore sensation and voluntary power, if chloroform had some peculiar and mysterious influence upon the nervous matter itself? If the treatment for the extreme effects of chloroform is the same as for asphyxia, if the *post-mortem* appearances are the same, and if the symptoms during life are the same, it is not unreasonable to believe that the *rationale* is also the same. The special provisions for taking off the pressure on the venous system of the cetacea and diving birds, he could not but regard as corroborative of the view he had taken in his paper. If the blood could get easily through them, why such a conformation of the veins? Dr Simpson's other objections, taken from its effects upon plants and animals, he (Dr G.) regarded as irrelevant. That it could not kill by altering the physical relations between the blood and the brain, seeing it killed animals without one—and also plants—was as good as saying, that cerebrate animals could not be killed by concussion, seeing that concussion killed animals without a brain. Dr Skae had reminded the Society that carbonic acid acted as a narcotic poison, but he forgot that narcotic poisons were fatal by asphyxia. If similar symptoms follow hanging, he (Dr G.) preferred believing that the origin of these was at the lungs even when carbonic acid is absorbed.

Meeting XII.—Dr Hamilton, President, in the Chair.

History of a fatal case of arrest of a foreign body in the œsophagus. By Robert Paterson, M.D.—(This case was illustrated by a drawing of the parts).

On solutions of gun-cotton, gutta percha, and caoutchouc, as dressings for wounds, etc. By Professor Simpson.

At different periods in the history of Surgery, very different practices have prevailed in regard to the treatment of wounds. At one time, in injuries or incisions of any great extent, the whole sides of the divided or cut surfaces

were dressed as separate and distinct wounds; and all chance of immediate union prevented. Slow reunion by suppuration and granulation, or, as the old surgeons termed it, by "digesting, mundifying, and incarning" wounds, was alone attempted. Afterwards, however, and yet not without much doubt and opposition,[1] the practice was introduced of placing from the first the sides and lips of the wound in contact, and thus allowing nature to produce the spontaneous adhesion of the whole wound, or as much of its surfaces as will thus adhere. In other words, reunion by the first intention came to be more and more attempted *after* the discovery of the doctrine of adhesion (as it was termed) was duly made and fully acted on.

But no small difference of opinion and practice has prevailed as to the best mode of bringing and retaining in contact the sides of such wounds as are capable of healing by the first intention. A great variety of bandages, plasters, needles, and stitches have been invented for this purpose. And, the propriety or non-propriety of using sutures in preference to plasters (the *sutura cruenta*, as it was termed, in preference to the *sutura sicca*), was at one time a special subject of dispute. Louis, Pibrac, and Young maintained that in all, or in almost all cases, the employment of the suture should be abandoned as irritating and hurtful. At the present day, both modes of artificial reunion—the *sutura cruenta* and *sicca*—are followed by the generality of surgeons; and often both modes are used simultaneously.

It seems not at all improbable, that another step in advance will betimes be effected, and that surgeons will be enabled to apply to the wound, after its edges are brought in contact, some material or other which, like an artificial plasma, or lute of coagulable lymph, will at one and the same time serve the following purposes:—

1. It will be sufficiently strong and adhesive to retain the edges of the wound together, without the irritation sometimes following the use and removal of sutures or pins;

2. It will serve as a perfect dressing to the wound.

3. It will, however, not be soluble in water or be easily removed, and hence will enable the surgeon to apply cold, &c., to restrain and modify the action in the wound, if required.

4. Though insoluble in water, the material used must be soluble in some menstruum that is easily and readily vaporized.

5. It will be applied in a fluid or semifluid form, and be thus capable of adapting itself to any irregularity in the edges of the wound or in the neighbouring cutaneous surface; the fluid part evaporating speedily on exposure;—and a solid tissue or substance possessing sufficient tenacity, adhesiveness, and insolubility in water for the above purposes, being left like a plaster on the edges of the united wound.

The intention of the present imperfect communication is to bring before the Society some of the attempts lately made in order to attain the above objects.

Two or three weeks ago, it was announced in our weekly medical journals, that a solution of gun-cotton in ether had been used in America as a dressing for wounds; and, within the last two or three days, the American journals which have arrived in Edinburgh, show that the suggestion of this practice is claimed by two different persons, viz., by Mr Maynard, a medical student of Boston, and by Dr Bigelow of the same city.

Soon after the discovery of gun-cotton or pyroxyline, Schoenbein and Boettger showed that acetic ether was one of its best solvents.—(*Chemical Gazette*

[1] "I would ask, (gravely writes O'Halloran in 1765), I would ask the most ignorant in our profession, whether he ever saw or heard even of a wound, though no more than one inch long, united in so short a time (as three days). These tales are told with more confidence than veracity; healing by inosculation, by the first intention, by immediate coalescence without suppuration, is merely chimerical, and opposite to the rules of nature."

for April 1, 1847.) Richner had ascertained that it was entirely soluble in the acetic ethers both of alcohol and wood spirit.—(*Ib.* for February 1.) When xyloidine, or the analogous compound made by the action of nitric acid on starch, is dissolved in ether, or ether and alcohol, Schoenbein found that it was reduced to a colourless gelatinous mass, and that, if spread upon a smooth surface, "*it left a dull white opaque membrane.*"—(*Ib.* for April 1.) I am not aware whether he has recorded any similar observation regarding the ethereal solution of pyroxline; but it is this quality of it that has been turned in America to practical purposes as an application and dressing for wounds.

When gun-cotton is fully dissolved in strong sulphuric ether, it forms a semi-transparent gelatinous pulp. When a layer of this is laid on any surface, the ether speedily evaporates, and leaves an adherent whitish cotton-like web, which contracts strongly as it dries; and possesses still all the usual inflammable and explosive properties of gun-cotton. In a great variety of trials which I have had made as to the best mode of forming the solution, I have met with very various results. Many of these variations were probably owing to imperfections and differences, either in the quality of the gun-cotton or of the ether. But occasionally, with the same ingredients, the quality of the solution seemed liable to differ. Sometimes after the gun-cotton had been immersed in the ether for ten or twelve hours without much effect, the addition of a little strong spirit immediately effected a perfect solution. The gun-cotton often possesses strong adhesive properties when used before it is all completely dissolved. In using as a solvent, aldehyde, which had been kept for a considerable time, I found a perfect pulpy solution formed in a few minutes. If it could be used as a menstruum, it would possess the advantage of being even more vaporizable than ether. But in trying to repeat the experiment with some aldehyde newly formed for the purpose, the same results were not obtained.

Usually an ounce of strong sulphuric ether will dissolve thirty grains or more of gun-cotton in the course of a few hours. But, to form a complete pulp, it will in general require to stand for a day. The advantages which the ethereal solution of gun-cotton seems to possess as a dressing for wounds, and particularly for those which it is desired to unite by the first intention, are stated in the following terms by Dr Bigelow:—

"1*st*, By its powerful contraction upon evaporation, it places the edges of an incised wound in much more intimate contact than is obtained by sutures and adhesive cloth—unites them by equal pressure throughout the whole extent of the wound, and maintains them immoveably fixed. 2*d*, It preserves the wound perfectly from contact with air, being impermeable to the atmosphere, while its adhesion to the skin is so intimate as to preclude the possibility of air entering beneath the edges. 3*d*, The substance remaining in contact with the skin and wound after the evaporation of the ether, seems to be entirely inert, so far as any irritating property is concerned, and this can scarcely be said of any resinous adhesive cloth or preparation. 4*th*, It does away with the necessity for sutures in incised wounds of almost any extent. 5*th*, It is sure to remain in intimate contact with the skin till union is complete, and being quite impervious to water, and presenting a polished surface, it allows the surrounding parts to be washed without regard to the wound or dressing. 6*th*, It is colourless and transparent, thus permitting the surgeon to witness all that goes on beneath, without involving the necessity for its removal. 7*th*, No heat is necessary for its application, and the presence of any moderate degree of cold is only objectionable in retarding the evaporation of the ether. 8*th*, It may be made at a trifling cost; an ounce phial instrincally worth little, being sufficient for a great number of dressings."

The adhesive power of this solution of gun-cotton is, when it is properly made and applied, certainly very great. In evidence of its strong adhesive powers Mr Maynard states the following experiments:—"He glued a strap of sheepskin to the hand by a thin layer of the solution, nine lines long and one and a half wide, and it sustained a weight of two pounds. A second strap,

attached to the hand by a layer of the substance, nine lines in length and three in width, sustained a weight of three. A third strap, fixed to the hand by a layer of the liquid, twelve lines square, resisted the force of ten pounds without giving way; and a fourth strap of the leather, glued to the hand by a stratum of the solution, measuring one and three fourths of an inch in length, and one in width, was not separated from its attachment by the gravity of twenty pounds!" These statements, says Mr Maynard, may appear incredible, but they are founded on exact and carefully performed experiments.

As to the mode of applying the ethereal solution of gun-cotton, Mr Maynard states, that, "In slight cuts a moderately thick coating of the solution, laid over the incised parts, was, on becoming dry, sufficient to keep the lips of the wound in position till union took place; but in most instances it was employed in conjunction with straps of cotton and sheepskin, and with raw cotton, forming with them strong, unyielding, adhesive straps, bandages, and encasements; and, after many experiments, I am convinced that this is the best and most effectual way in which it can be employed as an adhesive agent in surgery. The solution dries rapidly, and in a few seconds; by the evaporation of the ether it contains, it becomes solid and impermeable to water. And a strap moistened with it, and glued to any part of the cutaneous surface, adheres to it with a tenacity that is truly surprising." Mr Maynard mentions one case in which, after the removal of a tumour from the scalp, the hair in the neighbourhood was shaved, and two pieces of sheepskin were firmly sealed, by the solution of gun-cotton, to each side of the wound at a distance from the edges, and then these edges were approximated and retained in contact by passing stitches through the dead sheepskin instead of passing sutures through the living tissues of the walls of the wound.

Dr Bigelow's directions for applying the gun-cotton solution are as follows:—"For straight incisions of *whatever length*, provided the edges can be brought together without great difficulty, it is better to apply the solution in immediate contact with the skin, as follows:—The bleeding should be arrested, and the skin thoroughly dried. If the lips of the wound are themselves in contact, the surgeon has only to apply a coating of the solution lengthwise over the approximated edges by means of a camel's hair pencil, leaving it untouched after the brush has once passed over it till it is dry, during, perhaps, ten or twenty seconds. This first film will of itself have confined the edges together; but in order to increase the firmness of the support, more must then be applied in the same manner, allowing it to extend on either side of the incision a half an inch or more. If, however, the wound gapes, an assistant is required to bring the edges in contact, and retain them so whilst the application is made. If the incision is so long that the assistant cannot place the edges in apposition throughout the whole extent, begin by covering a small portion at the upper end, and apply the solution to the lower parts as fast as it becomes dry above. In this case something more than the film which is left adherent to the skin will be necessary for a safe and proper support to the wound, which may have a tendency to separate. The transparency of the dressing may be still maintained by adapting a piece of goldbeater's skin or oiled silk to the wound. This should be covered with the solution, and the membrane applied after the coating is on and already contracted. A dossil of lint, or a strip of cloth, or even a piece of tissue paper, which is thus rendered tough and waterproof, will answer the same purpose, though not transparent. Where there is much separation, it is better to fortify the wound in this way at once, and as fast as the first coating is applied and dry. "If, however, adhesion by first intention be not desired, the gum may be painted on in transverse strips, like adhesive cloth, letting the first strip dry, and giving it the goldbeater's skin support before the second is applied. Thus room is left for the escape of pus, and the exposed portion may be watched without removing the strips."

It has been proposed to use the ethereal solution of gun cotton for other purposes than the dressing and union of wounds, as, for example, as a sub-

stitute for the starch bandage in fractures ; as an application and dressing to ulcers, &c. In abrasions, and slight injuries of the skin about the fingers, it forms an excellent and most adhesive dressing. There is one extremely painful and unmanageable form of ulcer in which I applied it eight or ten days ago, at the Maternity Hospital, with perfect success. I allude to fissures at the base of the nipple. Most practitioners know well the agony that some mothers undergo, in consequence of this apparently slight disease ; the ulcer or fissure being renewed and torn open with each application of the child. In two such cases I united the edges of the fissures, and covered them over with the solution of gun-cotton, making the layer pretty strong. It acted successfully, by maintaining the edges so firmly together that they were not again re-opened by the infant ; the gun-cotton dressing was not like other dressings, affected by the moisture of the child's mouth ;—and as a dressing, and at the same time by securing rest to the part, it allowed complete adhesion and cicatrization speedily to take place.

In a case in which Professor Miller lately removed a large portion of necrosed bone from the lower jaw, I dressed the lips of the incision with the gun-cotton solution, and it held them for some days subsequently accurately in contact till adhesion took place. In two minor amputations (one of the finger and the other of the toe), by the same gentleman, it was applied, but required to be removed in a day or two, in consequence of it retaining the discharges.

I have made a number of experiments, with solutions in different menstrua, of various other substances besides gun-cotton, especially viscine, dextrine, caoutchouc, and gutta-percha.

Gutta-percha readily, I find, dissolves in chloroform. When a thin layer of this solution is spread upon the skin or any other surface, the chloroform rapidly evaporates, and leaves a film or web of gutta-percha, possessing all the tenacity and other properties of that substance. A layer of it, of the thickness of good writing-paper, has perhaps as much strength and tenacity as to hold the edges of a wound together, with all the required firmness and strength of sutures. When a film of it is placed upon the skin, and is allowed to dry thoroughly for a few minutes, the subsequent attempt to separate it, is like peeling and tearing off the epidermis after erysipelas, &c. It then forms, as it were, at once a kind of artificial tissue, epidermis, or skin, which adheres strongly for a time. There is one disadvantage pertaining to it. In the course of a day or two it generally dries and crisps up, like court plaster, at its edges. If we could either increase its adhesiveness, or destroy its tendency to dry and crisp, we would render it more useful. I have seen the addition of a little caoutchouc apparently correct it in these respects.

The deposit or " plaster " left by the solution of gutta-percha is far more equable, smooth, and skin-like then that left by the solution of gun-cotton.

A solution of caoutchouc in bisulphuret of carbon, ether, or chloroform, leaves a very thin, but perhaps less regular web, and one which stretches too readily for most practical purposes.

One great deficit in this class of dressings is the want of a menstruum sufficiently powerful, and, at the same time, not stimulating like ether or chloroform. This objection may, perhaps, in practice be got over, by applying an unstimulating solution of isinglass or the like, to the raw surface, before applying the stimulating solution of the gun-cotton or gutta-percha. Or the first layer of gun-cotton or gutta-percha may be made very thin, so as to evaporate almost instantaneously, and then afterwards a series of superincumbent layers may be added till the web is of the required strength. Other better substances for solution may, perhaps, be found. But no material has a chance of succeeding, unless it be insoluble in water after it is consolidated, and unless it be sufficiently strong in its texture, and possesses powerful adhesive properties.

[A variety of specimens, showing the kind of artificial tissue made by the above and other solutions, was shown by Dr Simpson to the Society.]

REPORT ON THE EMPLOYMENT OF CHLOROFORM IN MIDWIFERY, ETC.

Dr Simpson gave a long report and detailed communication on the employment of chloroform in midwifery, stating that he had used it constantly, and with the best results, in his own practice since November; mentioning the rules required to be attended to in its exhibition; answering the supposed objections to its use, &c. &c. He read numerous communications and reports regarding its employment, from Dr Grigor of Nairn, Professor Dyce of Aberdeen, Mr Lawrence of Montrose, Dr Paton of Dundee, Dr Anderson of Glasgow, &c. &c., showing that a great number of persons had been already successfully delivered without pain or suffering under the use of chloroform during the last six months. (*Dr Simpson's* communication will be published in our next Number.)

Drs Moir, Malcolm, Leith, Carmichael, &c., stated to the Society, some verbally, and others in writing, the uniform and successful results which they had met with, employing it, as they did, constantly in their practice, and in all cases of labour.

Mr Crisp of London stated, that though a stranger, he was induced to rise were it for no other purpose than to say, that, after having attended the meetings of many a medical society, he had never till to-night seen one that was unanimous in opinion on any topic. He had come to Edinburgh a fortnight ago, and now entertained a totally different opinion about chloroform from what he did when he arrived; for he had now seen it constantly and most successfully employed in the hospital and elsewhere. At the same time, although this had been the result of additional experience on his own mind, he was not disposed to blame, but, on the contrary, to commend the scepticism which had been shown by many most eminent men in London and elsewhere, on this subject, which he thought was no more than justified in relation to an agent of such a novel kind, and so important in its practical application. He believed that this scepticism had not its origin in any exclusive or bigoted feeling, but would be overcome as soon as the facts came to be as well known in London as they are in Edinburgh.

Dr Bennett considered it probable, that one of the reasons chloroform was not much used out of Edinburgh, was the impurity of the article administered. It was not long ago that Dr Clay of Manchester had stated to the Society, that although he had frequently seen it given in that town, he had never witnessed its proper effects produced until he came to Edinburgh. Mr Crisp from London had just made a similar statement. Dr G. Wilson had lately informed him, that even the chloroform manufactured in Edinburgh was not so pure as it might be, and that he had lately purified some which produced the full effect more rapidly, and with a smaller quantity, than that in ordinary use. He (Dr B.) conceived that the purity of the chloroform was not sufficiently attended to by those who had tried it, and that those who would not, had better, like Dr Clay and Mr Crisp, come and see it given in Edinburgh.

Dr Simpson observed, that he believed the want of success in England was owing also to another cause. From what he had learned, he was quite convinced that our English brethren, in using chloroform, often stopped altogether at that point which really constituted the true commencement of the effects of the inhalation. Immediately before the chloroform produced anæsthesia, more especially if there was any noise or disturbance, it not unfrequently excited the patient, who would talk incoherently for a moment or two, beg the inhalation to be suspended, perhaps struggle to get free from it, and have his arms and legs thrown into a state of strong clonic spasms. In Edinburgh, we all sufficiently know that these symptoms indicate merely that the patient is about to come under the full influence of the vapour, and that, in a minute or so, these symptoms will pass, and he will immediately be completely anæsthetic and completely unconscious. But in England these premonitory symptoms seem to have been often regarded as very alarming, and all attempts at further

inhalation stopt, exactly where and when the dose of the vapour should have been increased. And in the English journals such cases have been repeatedly and gravely recorded as instances of delirium, and spasms, and convulsions, and failure. They are not more anxious, or deserving of attention, than the same symptoms would be in a case of hysteria, and are quite transient if the inhalation is only persevered in. Dr Simpson added, that now, amongst many hundred patients, he had never yet met with one instance in which any person was insusceptible of the full effects of the chloroform. He knew that the experience of many of his brethren around him went to the same effect. Nor, in any one case, had he seen any marked bad effect from the full use of the chloroform. Deaths will occur after operations, and sometimes even during them; but every death during an operation was not, as some of late in the south have argued, from chloroform. A gentleman near him, Dr Paterson, some weeks ago opened with his lancet a large abscess in a child's neck. There was no hemorrhage; but in a minute or two, at most, after the incision was made, the child was dead. An English jury might possibly have anxiously tried to bring it in as a case of death, probably from chloroform. But it certainly was not so, for the very simple reason, that no chloroform whatever was used, the incision being considered too slight to require it. Dr Simpson asked Professor Miller and Dr Duncan to state the extent to which they used chloroform in their public and private surgical practice.

Professor Miller observed, that in the hospital and elsewhere the surgeons of Edinburgh had used chloroform in all their operations, with the exception, perhaps, of any such within the cavity of the mouth as were expected to be attended with much hemorrhage. And he could speak of its perfect success, and perfect certainty, and perfect safety, in the most unequivocal terms. There had been no misadventures, no failures, and now no fears of those spasms and other preliminary symptoms to which Dr Simpson had alluded. In saying all this, he believed he was simply stating the opinion and experience of all his surgical brethren here; and that no one amongst them would deem himself justified, morally or professionally, in now cutting and operating upon a patient in a waking and sensitive state. Every professional principle, nay, the common principles of humanity, forbad it, seeing that surgery was now happily possessed of sure and safe means by which it could avoid the necessity of such cruelty. These were strong opinions, strongly expressed, but, in answer to Dr Simpson's question, it was impossible for him to say less.

Dr Duncan stated, that he sincerely coincided in every part of the statement made by Professor Miller, and that, in his hospital and in his private practice, he constantly, like his other surgical brethren, used chloroform in all his operations, and even when making any painful examination for the purpose of diagnosis. There was only one case in which he had found a difficulty in its application, viz. when operating for internal hemorrhoids, the patient not, of course, having the capability of protruding the bowel when anæsthetic.

Professor Miller stated that, in operating for internal hemorrhoids, he had latterly been in the habit of making the patient first protrude the bowel; he then fixed the hemorrhoid with a vulsellum, chloroformed the patient, and afterwards terminated the operation.

TRIAL OF DR GIBSON FOR CULPABLE HOMICIDE, ETC.—(*Continued from last No.*)

THE medical witnesses for the defence were Drs Couper, W. Thomson, Lawrie, and Rainey of Glasgow, and Drs Alison, Christison, J. Y. Simpson, and Scott of Edinburgh; only three of these, however, were called into court.

Dr Couper, is Professor of Materia Medica in the University of Glasgow, and has been for some time in considerable practice as a medical man. Has known Dr Gibson for a long time. He has been regularly educated, and holds a high place in his profession. Had heard the report of the *post-mortem* examination of M'Ginty. From that examination, could not say with any certainty whe-

ther the disease of the peritoneum could have been apparent on the 19th of January? it is probable, but not certain. Would not have suspected disease of the peritoneum, if, on the 18th, the symptoms had been emaciation, diarrhœa, cheerfulness of spirit, debility, but not preventing the patient from walking, and if disease of the lungs had been clearly manifested. He would have concluded that disease of the lungs was sufficient to account for these symptoms. Judging from the account of the examination he had heard, considered that the immediate cause of death was tubercular peritonitis, conjoined with disease in the glands, which again followed disease of the lungs. Such are common complications of consumption. The symptoms they occasion are very often deceptive, and difficult to discover. One of the symptoms is distension or swelling of the abdomen. In the more advanced stage this may depend on effusion of fluid; in the earlier stage it might be owing to the distension of the bowels with air, or both may be combined. In the case of effusion of fluid, it sometimes comes on very rapidly. It may come on in two or three days, and it may give rise to distension or swelling of the abdomen, which was imperceptible before. It is possible that such swelling might have taken place during a sea voyage of two days' duration. Besides the distension, the other symptoms of peritonitis are pain, disturbance of the functions of the bowels, fever, debility, and emaciation.[1] Of these, pain on pressure is the most characteristic symptom. The other symptoms may accompany disease of the lungs, independent of tubercular peritonitis. It may be conceived that chronic peritonitis might be stimulated into activity by disorder of the bowels from eating fruit. It is quite possible for the disease to exist for a considerable time without being discovered. It occurs often. If the appetite had been good before the voyage, and bad after it, would conclude that the disease had undergone some alteration in the interval. Would not consider a person labouring under the symptoms described, to undergo any danger from a voyage to London. Not even in the month of January, if due care be taken. Sea voyages have been recommended to consumptive patients. The care necessary to be taken, should consist of a conveyance in a close carriage on land, warm clothing, and remaining between decks at sea, to avoid cold air and damp. Had the symptoms been such as have been described, would have signed the certificate.

Cross-examined by the Advocate-Depute.—Understood that swelling of the abdomen was excluded from the symptoms mentioned. Did not consider diarrhœa an objection to the voyage, unless it was very severe. Supposing both swelling of the abdomen, and fluctuation on pressure to have been present, much would depend upon the amount of pain and amount of swelling. Would still feel at liberty to sign the certificate, although a certain amount of pain and swelling had been discernible. A person under such disease would not recover under any circumstances. It was a mere question of place, whether he was to die in Glasgow or at Millbank.

Re-examined by Mr Moncreiff.—If Millbank was a better residence than Glasgow, removal would be beneficial.

Examined by the Lord Justice-Clerk.—If the patient had been confined from the 20th of December, with a pulse varying from 108 to 120, no appetite, taking pills to stop diarrhœa, thought he might still be conveyed to Millbank without injury. He had ordered patients under similar circumstances to be removed in the month of January. Recollected one case where, under his sanction and by his advice, a gentleman was removed several miles by land, to a ship going to Madeira. That was in 1829, and he is now alive and well.

Dr William Thomson is Professor of the Practice of Physic in the University of Glasgow. Is acquainted with Dr Gibson, who stands very high as a medical man. He has been highly educated, and is at present one of the directors of the Glasgow Infirmary. From the account of the *post-mortem* examination,

[1] Here the symptoms of acute peritonitis, owing to the mode of questioning by Counsel, became confounded with those of tubercular peritonitis.—ED.

considered that M'Ginty died of complicated tubercular disease of the lungs and of the belly; but, in all probability, the immediate cause of death was the disease in the belly. Could not tell how long it had lasted; sometimes such diseases are very slow in their progress, at others very rapid. In its early stage, it is often exceedingly slow and latent. It frequently happens that the disease remains latent for a considerable time, and then develops itself rapidly. The disease in the belly is often distinguished by tympanitic swelling, and swelling from effusion. Such swelling might undergo a very considerable alteration in twenty-four or forty-eight hours, particularly if caused by effusion. In many cases there is a total absence of pain, and sometimes it is very painful. Emaciation, quick pulse, and diarrhœa, are independent of abdominal complication. Would not consider a person labouring under such symptoms, unfit to take a journey by sea from Leith to London; sea voyages are often recommended, and have been proved to have been beneficial in cases of advanced consumption. His signing the certificate would depend on circumstances. If required to sign the certificate for the purpose of transmitting a convict without danger to his health, would have thought himself bound to do so.

Cross-examined by the Advocate-Depute.—If there had been no requirement to transmit, would have said it was better for him to remain.

Examined by the Lord Justice-Clerk.—Supposing even that the symptoms were worse than had been stated, and that abdominal swelling were present, would not consider granting the certificate was such an improper act, that the person was very culpable for granting it; thought there was a latitude allowable. Did not consider there were any facts in this case which would justify a medical man in considering the voyage to be wrong. Death might be protracted, or advancing.

By Lord Cockburn.—But, supposing it wrong, there was certainly nothing so outrageously wrong as to exclude the idea of the opinion in that certificate being held by a respectable medical man.

By the Lord Justice-Clerk.—He might be wrong or not, but there was nothing he had heard that enabled him to say, that, professing to be a medical man, he might not honestly sign that certificate, and hold that opinion. It is much easier looking retrospectively.—(Here witness was interrupted by the bench.) When patients obtain any thing they imagine will do them good, they sometimes rally. The mental state may easily be conceived to have a very considerable influence on the progress of the disease. If the patient was anxious to be removed, thought he might be indulged, especially if the point were difficult to determine. If the lungs were primarily diseased, it might account for the attention of a medical man not being so much drawn to the abdomen. Had often found that, by the patient directing his attention to new things, he has discovered what at first had escaped observation.

Professor Christison had heard the account of the *post-mortem* examination. The cause of death was the complicated effect of pulmonary consumption, ulceration of the bowels, and chronic inflammation of the external membrane of the bowels. Could not tell how long symptoms of the chronic inflammation had existed. The disease in the lungs was the primary disease; it was of the older standing. It is not frequently accompanied by chronic peritonitis, but frequently with ulceration of the bowels. Chronic peritonitis frequently exists in a latent state. It frequently advances with no signs, and frequently with such slight signs as will not attract attention for a long while. The patient died on the 10th of February, and it is perfectly possible that the perceptible symptoms might not have developed themselves on the 18th of January, although the disease must have existed prior to that. Thought that a medical man was warranted in signing a certificate on the 18th of January, that the party was fit for a sea voyage. There is a difference of opinion as to how far a sea voyage is advantageous in consumptive cases; some think there is a positive advantage in that mode of carriage. Diarrhœa is one of the symp-

toms of pulmonary consumption in the advanced stage ; also high pulse, progressive emaciation, and loss of strength. Is acquainted with the regulations of the jail in Glasgow. Knows there is not that accommodotion there which a person in such a hopeless state would require.

Mr Moncreiff stated that he had several other medical witnesses waiting, but thought it unnecessary to call them into Court.

Mr Crawford, the Advocate-Depute, made a very temperate and able speech to the jury, stating that they must be satisfied that this was a case which, from its importance to the public, and to the individual at the bar, ought to be fully investigated. He did not feel justified in asking for a verdict of culpable homicide, or for granting a certificate knowing it to be false. On the third charge, of neglect of duty in a public officer, however, he had reluctantly come to the conclusion that it ought not to be withdrawn. It was no part of the punishment of the guilt of M'Ginty, that in any respect his life should be trifled with ; and if he did not receive as much attention as he would have done in a very different position of society, then there was neglect of duty. The law for the protection of persons in M'Ginty's situation has required, not only that there shall be a medical certificate that he can be removed without danger, but that the certificate shall be granted by a public officer, the particular medical man who has had charge of the criminals. It was therefore the bounden duty of a person holding the situation of surgeon to the jail, to form the best opinion in his power, according to his opportunities, his skill, and his judgment. Now, this unfortunate man M'Ginty had been proved to have been labouring under symptoms which indicated the presence both of pulmonary and abdominal disease. Whether it reached the height in Glasgow which it was afterwards discovered to exist in London, is another question. But it is worthy of remark, that on one side there is the evidence of two medical men who saw M'Ginty, who have drawn their deductions not from theory or notes, or at second hand, but from their own personal observation of the symptoms when the patient was living, and of the appearances found in his body when dead. There is a third medical man confirming that opinion by strong testimony. On the other hand, there is high medical evidence ; but still the evidence of gentlemen who never saw the man. It is for the jury to say, whether, weighing the evidence of those who saw, against that of those who did not see him, they will agree with one or the other on the propriety of his removal. Now, that he was not fit to be removed was beyond all doubt. No one will maintain that, because he was dying, it was of no consequence where he died. There is the most conclusive evidence that the symptoms were discernible on the 21st of January, and it was for the jury to decide whether any grounds existed for supposing such symptoms arose between his removal on the 19th from Glasgow, and his inspection on the 21st in London.

Mr Moncreiff then addressed the jury for the accused, in a speech which evidently made upon them a strong impression. He did not complain of his learned friend for bringing the case to trial. Although it was a great misfortune to his client, he made no complaint. The situation in which Dr Gibson stood was one to which the subjects of every free country may be exposed ; his conduct was open to investigation, and the public prosecutor has chosen to have it in this shape. He thought, however, that his learned friend should, after having given up the two serious charges, have taken a step in advance, and withdrawn them all. He would be willing to stand on his speech, and say nothing more ; but, with the interests of Dr Gibson confided to him, he could not treat the case so lightly. Here, after eulogising Dr Gibson's character and skill as a medical practitioner, he entered at some length into the circumstances which we formerly explained regarding the change of certificates. He contended that it was only Dr Gibson's duty to sign the certificate, which, under the circumstances, any man might have done ; Drs Couper and Thomson would have done it, and here Dr Gibson's duty ended. He was bound to do his duty carefully, and give a medical opinion honestly ; but should he be

told, that, although he may have been mistaken altogether, he is to be placed as a culprit at the bar? Dr Couper said not only that the certificate was honestly signed, but that it might be truly signed, and that M'Ginty's health would not be endangered. That is not to say he was in good health. Dr Gibson never imagined that this man was in good health; but what he held, and the medical men confirm his opinion, was, that a man might labour under this disease, and not have his life endangered by the removal. In Glasgow he was pining in a wretched atmosphere, and longing to get away. One of the wardens said, that the disappointment of being detained would have killed him. He thought the milder air of the south would do him good; and under these circumstances, if the state of the weather had been propitious, and his condition somewhat different, he might have been a gainer and not a loser. The eminent Glasgow professors had said, that in the more advanced stage of pulmonary consumption, a sea voyage may be beneficial, and Dr Couper had given a case where it was attended with great benefit. They also stated that chronic peritonitis might in forty-eight hours undergo such a change, that the case may pass from a latent into such a one as presented itself on his arrival at Millbank. Such he believed to have been the case, caused probably by the fruit M'Ginty had eaten, or by one of those dreadful turns that these maladies often take before their fatal termination. Dr Gibson's character, therefore, as a man of truth comes unscathed out of this inquiry. But it is said there was neglect; now, Dr Gibson's assiduity was unsullied by the evidence. There was not a pretence for saying, that Dr Gibson was not in the jail morning, noon, or night, whenever his duty called him. He stripped the man. How, then, did he neglect his duty? His learned friend had said that he must have found the swelling; but he did not find it, and it is not sure that he must have found it. Every day, things that doctors say will happen, do not happen, and what they say will not take place, do take place. The tenure of many professional men's eminence would be very slender, if, on the mere possibility of their opinion being wrong, you were to consign them to jail. He was not defending Dr Gibson's professional character, that was unnecessary; he was defending him from crime. He thought the jury would be satisfied that Dr Gibson had already suffered enough anxiety from what was no fault of his.

The Lord-Justice-Clerk then summed up the evidence at considerable length, and succinctly placed all the facts of the case before the jury. With regard to the medical evidence, he asked—Might it not happen that the coughing and other symptoms might have led Dr Gibson to confine his attention to the lungs, where the disease first appeared; and his attention not being called by the man to the abdomen, he might not have thought that there was a disorder going on there which would have rendered his removal more difficult? This might lead to a complete explanation of the case without any reflection on Dr Gibson, and which we must be prepared to lay aside before we can come to the conclusion that there was neglect of duty. Other theories have been put forward. It is said that an effusion may have supervened during the voyage, and brought the abdominal disease into a degree of activity not previously discernible. But the gentlemen who examined the body gave evidence against that notion, when they say there were no appearances of recent inflammation. But, supposing Dr Gibson's attention had not been called to it, that is a complete answer to the charge of neglect. One witness had remarked, that it was easy to give an opinion retrospectively, but Dr Gibson made such an examination as from his knowledge of the circumstances he thought most proper. The jury had to consider whether he was culpable in not discovering the abdominal disease, when Dr Thomson had stated that cases have occurred where symptoms might escape observation, from the patient not calling attention to them. The essential part of the case, however, was, whether such culpable neglect had been exhibited, as in the language of the indictment tended "to the detriment of the public service, and the serious injury of the health of the lieges." Is it proved that this man did

suffer seriously by the voyage? No facts have been brought forward by the public prosecutor that he was injured. The medical men of London are most anxious to make out that the disease must have been as fatal on the 18th of January as on the 10th of February, the day of his death. There may have been terrible discomfort to this poor creature, but on arriving at Millbank his condition was not worse than when he left Glasgow. The charge of neglect is said to have been of such a kind as produced injury to the health. This had not been proved. He did not think the jury could lay hold of a fact which tended to show that his health was really endangered. The charge tried was, that there had been culpable neglect of duty, "to the detriment of the public service, and the serious injury to the health of the lieges." Unless that fact be proved, the prisoner at the bar is entitled to a verdict. The inquiry which had taken place was a most important one; the great end of the trial had taken place, and that was quite consistent with a verdict in favour of Dr Gibson.

The jury unanimously [1] and immediately returned a verdict of not guilty. The trial excited great interest among the profession, and the court was crowded by practitioners and students of medicine.

This trial has brought forward several points of the greatest interest to the profession, which we feel called upon shortly to notice.

In the first place, it seems extraordinary that the public prosecutor should bring Dr Gibson to trial for such grave offences as culpable homicide, granting a false certificate knowing it to be so, and neglect of duty as a public officer, when he must have known that the proper certificate had not been sent for signature to Glasgow. Whose duty was it to forward that certificate, and why had it not been sent to Glasgow, as well as to Lanark and other towns? Here undoubtedly was neglect of duty in a public officer, and, in our opinion, it was the person who made this serious error who should have been tried, rather than Dr Gibson.

In the second place, it must be evident that no medical man can ever be safe in this free country, so long as coroners permit juries to throw unfounded aspersions on the characters of medical men. In the judgment lately given by Mr Justice Coleridge, in the case of Dr Berncastle, it is stated that a coroner's jury were empanelled to inquire into the cause of death, and that in throwing imputations on the medical man, when he had no hand in producing the death, they were exceeding their jurisdiction. The verdict in that case was, "That Elizabeth Hopkins had died from exhaustion from protracted labour, and that Dr Berncastle had shown the greatest inhumanity in leaving her, and they thought him unfit to remain as medical officer of the union." The facts of the case were, that Dr Berncastle was not bound to attend the woman without an order; that he sent a person to the proper authority to procure that order, which was refused, and then he left the woman, his professional engagements calling him elsewhere. The jury, however, instead of charging the parish officer with inhumanity for not granting the necessary order, charge it upon the medical man, who loses his situation in consequence. When Dr Berncastle made an application for a *certiorari* to bring up the coroner's inquisition for the purpose of its being quashed or traversed, it was refused by Mr Justice Coleridge on the ground that there was no defect, or no material fact which it was desired to traverse. Now, the case of Dr Gibson is somewhat, though not exactly, similar. Here the verdict was, "That the deceased died from natural causes, but that the surgeon of Glasgow jail ought not to have permitted the deceased to take the journey, and that death was accelerated by the journey." This, which in England amounted to a verdict of manslaughter, fortunately for Dr Gibson, did not lead to the loss of his situation, but to a criminal trial, in which his complete innocence, and the absurdity of the

[1] Our English and Irish readers should perhaps be informed, that a Scotch jury consists of fifteen persons, the majority of whom give the verdict.

charge, was full exposed. At the same time it cannot be denied, that such charges are highly detrimental to the medical men concerned, and that coroner's juries should not be permitted to form such verdicts without a much fuller investigation than is usually had recourse to. Cases of consumption are continually happening in Scotland, where persons are sent to various distances advisedly, in a much worse condition than M'Ginty was. Yet should such persons die in England, say *en route* to Madeira, should any suspicion lead to a coroner's inquest, and on dissection organic disease to be discovered sufficient to account for death, surely it is monstrous to suppose that the medical man is to be tried for culpable homicide, because an ignorant jury, even though supported by too confident and ill-informed medical men, are of opinion that the journey ought not to have been taken. As was well stated by Mr Moncreiff, the tenure of a professional man's eminence would be very short if you consign him to jail every time he forms a wrong opinion, although in the case of Dr Gibson even this was not proved.

Turning now to the case of the man M'Ginty, we have some difficulty in ascertaining the facts which ought to guide our opinion. Instead of an authentic record of the *post-mortem* examination drawn up on the spot, and signed by the medical men present, we have three different accounts. Two of these were said to be derived from notes, the third was stated in court altogether from memory. The accounts, although they of course agree as to the general appearances, differ in some important particulars. But, what is singular, although they differed as to facts, the London medical men all agreed, that under the circumstances a sea voyage would be highly dangerous to the man's life. This opinion, given by all three in the most confident manner, was proved to be utterly erroneous, as M'Ginty arrived in London not one jot worse than when he set out. The Lord Justice-Clerk commented on this point with great acuteness, and told the jury that there was not the slightest evidence that any one symptom he laboured under had been increased by the removal.

The disease M'Ginty laboured under, was phthisis pulmonalis, complicated with tubercular peritonitis, and ulcerations of the intestines. The cause of death was, as stated by Dr Baly, exhaustion caused by these complicated lesions. Here it is necessary to remember, that before leaving Glasgow, Dr Gibson had very judiciously placed his patient on an animal diet, and given him cod liver oil with expectorants and anodynes. On arriving in London he was placed on a farinaceous diet, and afterwards had steel pills as a tonic and astringent. We firmly believe that this practice increased the man's weakness, and hastened his death, although we should regret to see Dr Baly tried for manslaughter for so doing. The more immediate cause of death, however, seemed to be something acute; for Mr Hall the house surgeon tells us, that on the evening previous, he complained of pain in his abdomen, with sickness and vomiting. Now, these are exactly the symptoms of intestinal perforation. On examining the body, the peritoneum was covered with lymph and tubercles, causing adhesions between their opposed surfaces. This is said to have been chronic, but the physical characters of the lymph exuded are nowhere described. There was serum in the abdomen, the amount of which was never stated—a very important omission. This, according to Dr Basham and Mr Hall, was turbid and of a dark dirty colour; but according to Dr Baly, was of a straw colour and perfectly clear—a most unfortunate discrepancy. One ulcer in the intestine had gone through all its coats, although no actual opening could be discovered after death. We think it most probable, however, that a minute perforation had taken place on the evening of the 9th, which caused the sudden pain, sickness, and vomiting, although that perforation may have been blocked up with lymph afterwards. On no other supposition can these symptoms be explained; and of the two appearances described we believe turbidity of the fluid to be the correct one, although from the contradictory evidence, and loose manner of recording the results of the examination, this cannot now be positively ascertained. We are inclined then to believe, that

acute peritonitis supervened upon the chronic, owing to intestinal perforation, causing the fatal symptoms under which in his exhausted condition he sank, and also causing the turbid, dirty fluid to be effused in the peritoneal cavity—by no means an uncommon termination of the disease.

The explanation we have advanced, however, is opposed by the testimony of Dr Baly. That gentleman positively asserted that he detected fluctuation of fluid in the abdomen, on the day of M'Ginty's arrival. Now, without wishing to impugn Dr Baly's accuracy, every practical man must be convinced it is not so easy to detect fluctuation of a small quantity of fluid in the abdominal cavity, especially when the peritoneum is loaded with chronic tubercle. We presume that the quantity was small, because if it existed to any amount, surely so important a fact would not have been omitted. But we have the positive statement of Dr Gibson, who examined the abdomen on the 18th, that there was no fluctuation, only tympanitis, to which he ascribed the swelling. The doughy feeling of tubercular peritonitis might have been mistaken for that of fluctuation, even by the hands of Dr Baly. It is certain, however, that he honestly conceived fluid to be present, and, to explain its existence at that early date, tells us afterwards, that to the best of his belief (for he had no note on *that* point) that the fluid found was ascitic, that is, of pale straw colour, and clear. His colleagues, however, declared that it was turbid, opaque, and dirty, which, as we have above endeavoured to show, corresponds with the symptoms of perforation experienced on the night previous to death. We have, therefore, strong doubt as to the existence of abdominal fluctuation on the day of M'Ginty's arrival. Dr Baly may at that time have strongly suspected fluctuation, and this suspicion might have been confirmed in his mind by the results of the *post-mortem* examination.

Such being the doubts and difficulties of the case, even now, when all the facts in connexion with it have been well scrutinized and weighed, we need not point out how exceedingly unjustifiable were the strong and positive assertions of Drs Baly, Basham, and Mr Hall, as to the time the disease had existed, the facility of detecting it, and the danger to which a short sea voyage exposed the man who laboured under it. We have already seen that no danger *did* arise, and, as a matter of opinion, the medical evidence for the defence is directly contrary to that given for the prosecution. Indeed, we believe there are few medical men of eminence who would not have met such unfounded and crude assertions with a direct denial. The following are some of these assertions, made by Dr Basham. "The cause of death was tubercular peritonitis." "A person with such a disease certainly could not be removed from Glasgow without danger." "The swelling of the abdomen arises from the absorption of lymph, and from the effusion of fluid of a very peculiar character." "In scrofulous peritonitis it is almost impossible for the serum to form in the abdomen suddenly—it is contrary to the nature of the disease that pure serum should be evolved, and, except pure serum, did not think there would be any sudden effusion in the course of a few days." "It is rare for an acute attack of peritonitis to supervene upon chronic peritonitis." "He decidedly thought that, if tubercles existed in the lungs alone, not to speak of their presence elsewhere, it would be most hazardous to send a man a voyage in that season of the year." These assertions, and in some instances extraordinary statements, while they could not fail to excite the astonishment of every well-informed practitioner present, were delivered with a quickness and decision, well calculated to impose upon the non-professional public, and the superficial.

An attentive perusal of the medical evidence in this case, both for and against the accused, must we think convince our readers, that the opinions of the London practitioners were far too positive and decided, and that in stating them in so unqualified a manner, especially as regards the existence of obscure symptoms for a long period, and the danger of a sea voyage, they were by no means warranted by the present state of our knowledge. This is the more to be regretted when it is remembered, that the object of these statements was to

criminate a medical brother, whom no reasonable man could for a moment believe was guilty of the offences ascribed to him.

Notwithstanding the length of these remarks, we cannot dismiss this case without observing that it constitutes no excuse for an imperfect diagnosis, that the attention of the medical man was not directed towards this or that organ. The Lord Justice-Clerk in his remarks to the jury seemed to think that if this explanation was accepted, no blame could be attached to the prisoner. Now, with all deference to the learned judge, we are of opinion that if this had been proved, the accused would necessarily have been guilty. No doubt medical men often do make mistakes from hurry or carelessness, but under such circumstances they neglect their duty; which is, carefully to enquire into the state of *all* the organs and *all* the functions, in order to ensure accuracy as far as the art will permit. We are disposed to believe, that Dr Gibson had done this. Of course, some men are more skilful in forming a correct diagnosis than others, and it is impossible to fix a standard which shall declare when culpability commences, and ability ends. It would never do, however, to maintain, that because some men have overlooked important symptoms, that any one can be excused for having done so.

OPIUM-EATING IN ENGLAND.

From the accounts of the Board of Trade, of the imports and exports of the United Kingdom for the month ending May 5, it would appear that the consumption of opium in this country is greatly on the increase. The quantity imported during the month amounted to no less than 7·029 pounds to 3·083 pounds imported during the same period in 1847. The total quantity of opium imported in 1847, amounted to 24·929 pounds. It is reported that the inmates of our workhouses are given to the practice of opium eating.—*Medical Gazette.*

RECOGNITION OF HUMAN SKIN AFTER THE LAPSE OF CENTURIES.

At a late meeting of the Microscopical Society of London, Mr John Queckett read an interesting paper on the importance of the microscope in the determination of minute structures of a doubtful nature. The author stated that his object in bringing the communication before the Society was to point out how minute portions of skin, which had been exposed to the air for centuries, could be recognised as human. There existed in this country certain traditions, that persons who had committed sacrilege were flayed, and their skins nailed to the doors of the churches they had robbed, and three portions of such skin had been forwarded to the author for examination, by Albert Way, Esq., the secretary of the Archæological Society. The first was taken from one of the doors of Worcester Cathedral; the second specimen from the church door of Hadstock in Essex; and the third from a church door in Copford, also in Essex. On all the specimens, Mr Queckett succeeded in finding two or three hairs, which the microscope clearly proved to be human. Thus, this valuable instrument is able to confirm a tradition, and prove the former prevalence of a practice which had been doubted by many archæologists.—*Lancet.*

RETURN OF MR SYME TO EDINBURGH.

Mr Syme returns to Edinburgh in the first week of July. We understand that his resignation of the Chair of Clinical Surgery has been withdrawn, and he resumes his duties in the University and the Infirmary as formerly. The most incorrect information has been circulated by the London weekly press, regarding the movements of this gentlemen, and the proceedings in Edinburgh with reference to the Chair of the Institutes of Medicine.

BOOKS RECEIVED.

1. On the Nature and Treatment of Stomach and Renal Diseases, &c. By William Prout, M.D., F.R.S., &c. The Fifth Edition. London. 8vo. 1848.

2. A Treatise on Diet and Regimen. By William Henry Robertson, M.D., &c. Fourth Edition. Vol. II. London. Small 8vo. 1848.

3. On Wounds and Injuries of the Chest, &c. By G. T. Guthrie, F.R.S. London. Royal 8vo. 1848.

4. Insanity tested by Science, and shown to be a Disease rarely connected with Permanent Organic Lesion of the Brain, &c. By C. M. Burnett, M.D. London. 8vo. 1848.

5. A Descriptive Catalogue of the Anatomical Museum of the Boston Society for Medical Improvement. By J. B. S. Jackson, M.D., Boston. 8vo. 1847.

6. A Descriptive Catalogue of Wet Preparations, Casts, Drawings, Models, Books, &c., contained in the Museum of the Birmingham and Midland Counties' Lying-in Hospital and Dispensary, &c. By John G. Connor, A. C., &c. Birmingham. 8vo. 1847.

7. On Functional Diseases of the Liver, associated with Uterine Derangements, &c. By Butler Lane, M.D., &c. London. 8vo. 1848.

8. An Essay on the Epileptic Form of Puerperal Convulsions, &c. By Joseph Thompson, M.R.C.S., &c. Nottingham. 12mo. 1848.

9. Hospital Elections and Medical Reform, addressed to the Governors of St. George's Hospital and to the Profession. By Edwin Lee, &c. London. 8vo. 1848.

THE FOLLOWING JOURNALS HAVE BEEN RECEIVED IN EXCHANGE.

1. *The British and Foreign Medico-Chirurgical Review.* Received regularly.
2. *The Dublin Quarterly Journal of Medical Science.* Received regularly.
3. *The Provincial Medical and Surgical Journal.* Received regularly.
4. *The London Medical Gazette.* Generally received about four days after publication.
5. *The Medical Times.* Received regularly.
6. *The Dublin Medical Press.* Received regularly.
7. *The Chemical Gazette.* Generally received a month after publication.
8. *The British American Journal.* Received regularly.
9. *The American Journal of the Medical Sciences.* Received regularly.
10. *The Medical Examiner.* Received very irregularly.
11. *The Charleston Medical Journal and Review.* Last No. received for March 1848.
12. *Archives Generales de Medicine.* Received very irregularly.
13. *Annales de Therapeutique.* Received irregularly—the last No. received is for Avril 1848.
14. *Annales Medico-Psychologiques.* Received very irregularly.
15. *Journal de Medicine et de Chirurgie Pratiques.* Received regularly.
16. *Bulletin General de Therapeutique.* Exchange accepted—Please send by Mr J. B. Bailliere's parcel.
17. *Gazette Medicale de Paris.* Received regularly.
18. *L'Union Medicale.* Received regularly.
19. *Gazette des Hopitaux.* Received regularly.
20. *Bulletin de l'Academie Royale de Medicine de Belgique..* Received very irregularly.
21. *Gazette de Strasbourg.* Received very irregularly.
22. *Prager Vierteljahrschrift fur die Praktische Heilkunde.* Last No. received is No. IV. for 1847!
23. *Zeitschrift fur Rationelle Medizin.* All the Nos. have been received, including No. I. for 1848.
24. *Zeitschrift der K. K. Gesellschaft der Artze zu Wien.* Last No. received is for September 1846!
25. *Medicinishe Jahrbucher*, and *Oesterrichische Wochenschrift.* Last Nos. received are for June 1847!
26. *Archives fur Physiologische und Pathologische Chemie und Microscopie.* Last No. received, No. V. 1847!
27. *Gazetta Medica Lombarda.* Received regularly.

As our numbers are forwarded with the greatest regularity to all these Journals every month on the day of publication, we have to request the Editors of these Journals (especially those from Austria) will make some efforts to send us the exchange copies as soon as possible.

NOTICES TO CORRESPONDENTS.

Owing to the press of medical news and other matter at a late period of the month, we have been obliged to leave out some reviews and bibliographical notices which were intended for insertion in this Number.

Mr Lyon's communication has been received.

ILLUSTRATIONS OF Dr PEDDIE'S PAPER ON THE MAMMARY SECRETION.
FIGURES MAGNIFIED 206 DIAMETERS.

(To face page 65)

THE

MONTHLY JOURNAL

OF

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*On the Mammary Secretion; its Character, Chemical and Structural; the Value of the Microscope in the study of it; its Existence an important Sign of Early Pregnancy; and its Pathological Changes; with Hints regarding Lactation and the Choice of Nurses.* By Alexander Peddie, M.D., F.R.C.P., Physician to Minto House Hospital and Dispensary, Edinburgh.

The chief design of this paper is to show, that the existence of milk in the breasts is an important sign or evidence of pregnancy, more especially of a first gestation, and in the earlier months; and to enforce attention to the practical value of the microscope, with the view of detecting the pathological changes of milk, and discerning cases where the propriety or impropriety of lactation, on account of the health of mother or child, may be under consideration. To render my remarks, however, more intelligible to those who may not have given particular attention to the subject of the lacteal secretion, it seems necessary that I should make a few preliminary statements regarding the function of the mammary glands, and the character, chemical and structural, of the fluid formed by them; and, to make this communication more complete and generally useful, I shall in its latter part introduce some general remarks on nursing, the choice of nurses, and the evils of unduly prolonged lactation.

I.—*The Functions of the Mammary Glands, and the Characters of the Lacteal Secretion in its Normal State.*

Providence, kind and wise in every arrangement, has in all respects perfectly adapted these glands, with their peculiar form and

distribution of tubes, blood-vessels, absorbents, glandules, follicles, and epithelial cells, to the purpose of elaborating from the blood a fluid admirably suited for animal nourishment; and while the apparatus and its products are thus so well fitted for supplying physical wants, connected with them are generated sympathies, organic and emotional, which likewise assist in the conservation and growth, first of the embryo, and then of the infant, and draw forth those keen and strong affections which contribute to the mother's health and happiness.

To the accomplishment of those ends, as womanhood is approached the mammary glands are gradually developed, and feelings and desires are excited, and uterine sympathies established, which prepare them for the exercise of their destined functions. Whenever, therefore, conception occurs, these organs and sympathies receive an impulse to activity; so that, on the arrival of the period when the child must maintain a separate existence, the mother is enabled to furnish a supply of food of all others best fitted in quantity and quality for its sustenance.

Human milk, when perfectly healthy, is, to external appearance, thin, turbid, and white, with a slightly bluish tinge. It has a sweet taste, acquires a cream when allowed to settle, will afford butter if churned, will precipitate curd from the serum or whey if mixed with the mineral, acetic, or lactic acids, and finally cheese, if this curd is submitted to pressure. The oily part forms the cream and the butter,—the former by the ascension to the surface of the larger globules, and the latter by the union or incorporation of these with each other; while the casein or albumen—for they appear, according to Liebig, to be identical [1]—gives the cheese. The specific gravity is from 1·030 to 1·035. It is decidedly alkaline when fresh, but becomes acid when kept for any time. Its chemical analysis has been stated by Simon to be

88·06	water.
3·70	casein.
4·54	sugar.
3·40	butter.
0·30	saline matters, &c.
100·00.	

Its similarity, therefore, to the constitution of the blood itself is evident;[2] and reflection on the combination of the saccharine with the oleaginous and albuminous elements, will render apparent its life-sustaining and nutrient properties.

From the above analysis it will also be seen, that the fluid or

[1] Mulder has demonstrated that casein and albumen have the same bases, namely, protein.

[2] The chemical relations of blood and milk are exceedingly well stated by Dr Rees in the Cyclopædia of Anatomy and Physiology, Article *Milk*, p. 363.

serous portion of the milk is much larger than the solid. This part holds in solution the sugar, various saline ingredients, and the largest amount of the caseous element; while the oleaginous or buttery globules, with their thin, caseous pellicles, are suspended and float in it. Under the microscope the milk is seen to consist of a serous fluid in which innumerable globules are suspended. These, when viewed immediately, are seen to have active molecular movements, rolling past or over one another in their own independent courses. They vary in size, according to Donné,[1] from 1-500th to 1-50th of a millimètre. This admeasurement is differently stated by various observers, and the medium size is given as 1-4000th or 1-4500th of an inch in diameter;[2] but I should suppose that definiteness must be next to impossible. They appear—as Fig. I. of the annexed plate, drawn with the aid of a fine achromatic microscope magnifying 206 diameters will show—to range from minute molecular spots to pretty large globules. The smallest seem to be wholly or mostly caseous, while the largest are those described as cream globules. They are perfectly spherical and pearl-like, have clear dark margins, and bright, light-refracting centres. The surrounding margin has a distinct film or pellicle, which, however, the best observers do not recognise as a true organized cell, such as the envelope of the pus or blood corpuscle, but simply as a coating of caseous or albuminous matter, as pointed out by Mandl;[3] and Dr Ascherson of Berlin[4] has shown, that a drop of oil and a drop of the white of egg, dexterously mixed, produces an appearance identical with that of the milk globule. Ether dissolves the oil of the globules.

These, then, are the characters of the lacteal secretion, as found in its normal condition when the mother has quite recovered from childbed, and is suckling her child. But until the process of secretion is fairly established, the milk is marked by other features which it is of importance to notice, namely, the presence of what is called colostrum. The colostric fluid—popularly termed "green milk"—serves an important purpose in the infant economy, being nature's aperient for purging off the meconium which has accumulated in the large intestines during fœtal existence. When present in great quantity, it gives to the milk, seen by the naked eye, a yellowish colour; and, on examination under the microscope, it is found to differ most essentially from the secretion already described. In examining a single drop two or three days after parturition, within the field of the microscope, while numerous proper milk globules may be seen moving independently here and there, they are agglomerated into masses, adhering together by a viscid mucoid fluid; and at the same time there will be seen from one to four, or even more, colostric bodies. These,

[1] Cours de Microscopie.

[2] Hassall's Microscopic Anatomy of the Human Body.

[3] Anat. Microscopie, p. 53.

[4] As quoted by Dr Bennett, in his paper on the Structural Relations of Oil and Albumen.—*Monthly Journal of Medical Science* for 1847, p. 167.

described by Donné [1] as the "*corps granuleux*," vary much in form and size,—a few resembling large oily globules, evidently butter ill elaborated, forming a yellow bed, in which lie a multitude of dusty looking granules, and among which is generally imprisoned one or more true milk globules. The envelope to these masses is at first firm and transparent, but breaks down as the secretion becomes older, and the globules become individually more independent of each other.[2]

I have given a representation of these appearances in Fig. VI., which will answer the purpose of a lengthened demonstration; and as this specimen was taken from a patient in the best of health three days after delivery, and promising to become a most excellent nurse, the same delineation will show the various stages towards the disappearance of these bodies, namely, the breaking down of the firm envelope or binding membrane, the shedding of its contents, and the last cloudy streak or patch which is visible. These changes go on in general until about the tenth day after delivery, though certainly in many good nurses for about three weeks; and then the milk ought to possess the normal characters (Fig. I.) already described. The granular portion of these masses is soluble in ether; the membranous portion in acetic acid; and treated with ammonia the mass becomes glassy and tenacious.

Here I may notice, although having no practical bearing upon the present inquiry, that I have examined microscopically many specimens of the milk which is well known to exist in the breasts of young infants. I have found it almost invariably present, seldom obtainable until the second day after birth, generally most abundant about the fifth day, and usually gone by the tenth day. It presents all the characters of the mother's milk at the same period, with colostric bodies as well as milk globules,—only the latter are in sparing

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[1] P. 400.

[2] There is an evident similarity between colostric and compound granular bodies,—those formations which Gluge denominates *inflammation globules*, which Vogel calls *granular cells*, Henle, *exudation corpuscles*, and Bennett, *exudation masses*. These structures are seen in cerebral softenings, and inflammatory products of various organs; but M. Reinhardt has shown that they are not necessarily characteristic of inflammatory action. In the digest of his opinions given in the *Monthly Retrospect* for Feb. 1848, published along with the *Monthly Journal*, it appears that M. Reinhardt considers the colostric granules as transformations of the epithelial cells of the mammary ducts—as the result of disintegration, or a retrogressive metamorphosis. During pregnancy, when there is an increased action going on in all the tissues of the mammary glands, and a greater determination of blood to them, the epithelial cells are thrown off in greater numbers than usual, distending the milk tubes. "This superfluous epithelium, therefore," the editor says, "is converted into granular cells or colostric corpuscles, and thence into masses of agglomerated fatty granules, which are then broken up and absorbed. During the first days after delivery, those bodies continue to be developed, and are washed out with the first formations of milk which constitute the colostrum." The colostrum generally exists in greatest quantity after first deliveries.

number, and generally of larger size. I find also that it is as often present in male as in female children, and may be got most readily by a gentle pinching or pressure on each side of the tiny nipple. I have given representations of this infantile milk in Fig. XI. Nos. 1 and 2.

After these details, I now go on to consider the existence of the mammary secretion *during* gravidity; and to enforce the practical importance of a knowledge of this fact in the diagnosis of that condition.

II.—*The Mammary Secretion prior to Parturition; and its Existence a Sign of Pregnancy.*

In common with others in the profession, when considering the differential diagnosis of obstructed mensturation and pregnancy in the earlier months, with all due attention to the ordinarily recognised signs, I have often experienced serious difficulty as to the opinion to be given, or the treatment to be pursued; and I believe that I have been misled in some instances by those whose object was, if possible, deception and concealment. The dilemma is most apt to occur in the discharge of hospital and dispensary duties, when required to give advice to young females whose characters are unknown. Under all circumstances, the responsibility which rests upon the physician is great. To convict an applicant of an immoral act, and of present imposture, and avoid the risk of prescribing means which might blight a human existence; or to allay a mother's fears of a daughter's virtue, and silence the tattle and cruel suspicions of a neighbourhood, are assuredly important objects. The determination may concern the health, happiness, and prosperity of individuals and families, and greatly affect the practitioner's peace of mind and prospects in life. Indeed, in every point of view, as well as from regard to moral and medico-legal ends, it is an imperative duty in any doubtful case, to submit to a searching scrutiny every sign, symptom, and circumstance which can throw light upon it.

From the experience of upwards of two years, during which my attention has been directed to the subject, I feel convinced that *the most invariable sign of gestation prior to quickening, is to be found in the presence of fluid in the breasts,*—with the limitations that shall hereafter be noticed; and, consequently, that the absence of the secretion will afford the surest evidence that the suspension of the catamenial flux is an abnormal deviation from nature's course. The sign is indeed, to some extent, an old and popular one, but not to be despised or overlooked on that account, nor to be set aside without consideration. Although some eminent writers on obstetric medicine have passed it by without any notice, and others have pronounced it "an evidence scarcely of any value at all,"[1] I would earnestly invite an unprejudiced attention and scrutiny of the subject; for my own experience

[1] Churchill, Theory and Practice of Midwifery, p. 107.

in judging from the sign, when there was an opportunity of watching the course of events, warrants in me stating, that I have never found it fail in regard to those *who were gravid for the* FIRST *time*, or in regard to those who were not pregnant at all. And although the greater number of cases of pregnancy in which I have had an opportunity of applying the test, have been advancing in the fourth month, yet so early as the end of the second and the beginning of the third months the sign has held good. It is not, however, until about the termination of the third, and more generally in the currency of the fourth months, that the medical man is consulted, when the repeated non-appearance of the menses attract the notice, and excite the interest or fear of the individual, according as the moral feelings may be affected. And as the value of the sign is not insisted upon in its application to other than first pregnancies, within this limitation is included the class of cases, which, above all others, are of most frequent occurrence and importance, and which occasion most trouble and anxiety to the practitioner.

Perhaps the value of this sign in the early months, has been doubted or under-estimated, in consequence of not observing fluid trickle from the nipples, as it frequently does in the last month of pregnancy. I believe that this will rarely happen in the commencement of a first gestation. The fluid must be brought; and the method of obtaining it, under doubtful circumstances, is to press the finger and the thumb firmly on the mammary gland, a little beyond the margin of the areola, and then draw them to the point of the nipple with a stripping and expressing movement. This repeated three or four times, will certainly bring fluid if any be present; and a single drop will suffice to prove the nature of the case. A little moisture from the sebaceous follicles of the areola, which is sometimes produced during these efforts, must not be mistaken for a lactic secretion. I believe that this kind of exudation, which may occur in ordinary circumstances, has actually led, in some instances, to the rejection of the sign which I am now advocating. It is recommended also, that if a drop of fluid is not obtained from one nipple, the other ought to be tried, as the orifices of the lactiferous tubes are sometimes more narrow or glued up in the one than in the other breast, and in primiparæ this is more particularly the case.

In the early months of a first pregnancy the secretion has seldom the external appearance of milk, but is serous-looking, and often very viscid and syrupy. When submitted, however, to the microscope, the characteristic milk globules (described p. 67) will at once be detected; and these will be seen agglomerated *en masse*, the solid portion being at this period in a large ratio to the fluid, which latter is also peculiarly glutinous. Mixed with these groups will be perceived an abundance of large oil globules and colostrum granules, as in the green milk of recent parturition. There are sometimes found also a few epithelial lamellæ, which have been separated from the lining

membrane of the excretory ducts, and which have either not been transformed into colostric masses, or, if this has been so, they have already parted with their mucoid and granular contents.

Of the annexed plates, II. and III. are exact copies of the secretion taken in the third and fourth months of gestation; the first, from a young unmarried woman, who attempted to conceal her pregnancy; and the second, from a young unmarried lady, who was not aware of her own condition, and whose station in life, education, and previous good conduct, was a protection against an early suspicion of her state. Both were convicted by the milk test, when the ordinary signs excited only a vague suspicion; both soon confessed their transgression; and both were, on the strength of the opinion given, immediately placed in the bonds of lawful wedlock.

The value of the lactic secretion as a sign of pregnancy has apparently been disregarded by some, in consequence of the very exceptional cases recorded, more especially the example given by Baudeloque, of a girl eight years old who was able at pleasure to milk her own breasts, and another somewhat similar mentioned by Belloc; and this under-valuation may also be ascribed to the statements of other writers regarding the exudation of milk, even from the breasts of adult males.[1] Such cases, however, even if well authenticated, are worthy only of being ranked among other physical monstrosities occasionally met with; and even instances less wonderful, as the appearance of fluid in the breasts of those who are not, and never were pregnant, ought, I think, to be viewed as rare examples of nature's freaks—her exceptions, and not her rule.[2] While I do not doubt that such cases have occurred—though I think some of them are not unlikely to have been the follicular exudations already noticed—in the very numerous examinations which I have made with a view to detect, if possible, the existence of fluid in the mammæ of the non-pregnant, I have not as yet met with an instance of the kind. In many instances of unmarried women of unblemished character, who were suffering from menstrual obstructions, and of married women under similar circumstances, who never were gravid, I have not been able by expression to obtain a single drop of fluid; and in many instances also of both classes where there was perfect uterine health, I have been equally unsuccessful.

Beyond the limitation of first pregnancies, I would not desire to

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[1] Carpenter in his Treatise on Physiology, p. 626, refers to an instance of this kind, as described in the Phil. Trans., vol. xli. p. 813; another by Captain Franklin, in his Narrative of a Journey to the Polar Sea, p. 157; one by Humboldt, in his Personal Narrative, vol. iii. p. 58; and a fourth by Dr Dunglison, in his Physiology, vol. ii. p. 417. In the last-mentioned case, the subject, a man of colour, is said to have actually officiated as a wet nurse!

[2] In this light I would regard the single instance which M. Donné has noticed, of a little fluid found by him in the breast of a young woman said never to have been pregnant, and which presented the microscopic character of milk, p. 441.

urge the application of the milk test; for when a woman has once suckled, the fluid is apt to linger in the breasts a considerable time after weaning, and the mammæ continue performing a partial function,—in many instances, doubtless, owing to the daily operation of maternal sympathies. In general, I have found the fluid easily obtainable from three to six months after weaning, and the restoration of the catamenia,—although in one instance which occurred lately, it had completely vanished before the expiration of two months. On the other hand, I found it present in one case after the lapse of two years; and I believe that it may exist at a still later period, although in several women still within a child-bearing age, I have been unable to procure a single drop of fluid at the distance of four years from the date of last weaning. It may, however, readily return at a much later period in the case of those who have once given suck, when the uterus becomes distended with any false conception or hydatid accumulation; and very probably it may do so under similar circumstances, in those who never were pregnant. An instance of the former kind came lately under my own observation. The patient had not had a child for nine years, and while the uterus gradually enlarged, and the abdomen became distended, so as to simulate pregnancy, and lead to the necessary preparations, imposing on myself at an early, and on another physician at a later period, the secretion was most abundant. The time of expected confinement, however, passed by to the extent of several weeks, and the mystery was solved by a sudden and large discharge of water.

When milk is procurable a few months subsequent to weaning, and the woman not again gravid, I have found it existing only in small quantity, one or two drops, viscous and cream-like to the unaided eye; and to the microscope it presents milk globules in sparing number, often ill formed, adhering together, or to large oily drops (the creamy part), or to the epithelial scales, mucoid, and other foreign matters, which are generally present in abundance—as is seen in Fig. VII. 1 and 2. The secretion in such circumstances affords, I think, some good diagnostic marks, by which it may be readily distinguishable from fluid obtained in the early months of a new gestation. The latter is comparatively rich in milk globules of normal appearance, and of better medium size, having abundant colostrum, and few if any membranous scales, or debris of disintegrated textures—(Figs. II. and III.); and, as the full term of gestation is approached, the secretion increases in quantity, and becomes better in quality, more evidently suited for the important object in prospect.—(Figs. IV. and V.)

From the above notices, I think it may be admitted that the sign of pregnancy now advanced, although of most value in the diagnosis of a first pregnancy, is not without a certain amount of importance in the recognition even of a subsequent gestation; and I should decidedly affirm, that in general it is more certain in its information as a corroborative evidence of pregnancy than the papular

areolæ, which, when once darkened, seldom lose much of their colour, or of the follicular glands, which retain in a great measure their size after they are once developed.

Compared with any of the ordinarily recognised signs for distinguishing a first pregnancy from a simple suppression of the menses, before any bulk or impaction in the iliac and hypogastric regions can be detected by the eye or hand, or before the ear can discover the unmistakeable sounds of placental and fœtal circulation, there will be found, I think, far fewer exceptions to the milk test. As regards the sign of *morning sickness*, I have had under advice many cases of obstructed menstruation from causes unconnected with gravidity, yet attended with disturbance of the digestive organs to a great extent, of which daily recurring sickness—most frequently in the morning—formed a part; while, on the other hand, in very frequent instances this symptom has been entirely awanting in those really pregnant. Then, again, the signs taken from *the aspect of the mammæ*, are most variable and contradictory. While I have often observed women with fair complexions who had large breasts, well marked areolæ, numerous and large follicular glands, and prominent nipples, suffering merely from suppression of the catamenia, I have seen not a few with dark or sallow complexions, who, although undoubtedly pregnant, had small breasts, small nipples, areolæ scarcely distinguishable from the surrounding skin, and few or no sebaceous glands.[1] Were it not to extend this communication to too great a length, I might give notes of many cases illustrating the variability and uncertainty of these signs. I shall, however, content myself with noticing only one example—which is interesting in several other respects. The patient was brought before the Obstetrical Society in December last[2] by Dr Simpson, chiefly to show the impossibility of diagnosticating pregnancy in her case, in so far as the appearance of the mammæ were concerned. The woman had dark brown hair and a sallowish complexion; she had been four years married, and was then, although presenting no traces of areolæ or glandular follicles, decidedly in the seventh month of her first pregnancy, complicated with large fibrous tumours projecting from the anterior wall of the uterus. Now, it is an interesting fact, that before the fourth month was complete, counting from the last menstrual period, I had carefully examined this patient, who called on me on account of the tumour

[1] In so far as the appearances of the breasts are concerned, it may be curious to notice here, that I had under my care lately an adult male of fair and ruddy complexion, afflicted with disease of the heart, whose breasts were full, slightly pendulous, surrounded with exceedingly dark areolæ, numerous follicles, and whose nipples were large and prominent. On several previous occasions I have met with cases somewhat similar; and in fact the variety in the colour of the male areola, the development of its follicles, and the size of the nipple, is a matter of daily observation.

[2] Monthly Journal of Medical Science, March 1848, p. 693.

of the abdomen; and, in consequence of being able to extract a little fluid from the nipples, I expressed my conviction that she was pregnant, though I was then at a loss to say whether the tumour was a growth from the uterus or an extra-uterine conception. On a second examination, about one month subsequently, I was able by the stethoscope to verify the opinion formerly given as to the fact of gravidity, and I know that she has been since confined at the proper term of gestation.

As regards the evidences of pregnancy from *abdominal exploration* previous to the commencement of the fifth month, there is always ground for much doubt and fallacy. For it is not till then that quickening occurs; and in cases requiring a special scrutiny, this is a symptom which will in all probability be concealed. It is not until then also, that the iliac and hypogastric regions assume a visible fulness, and give to touch a feeling of firmness and impaction; and, although this enlargement was then apparent, it might be owing to causes very different from gravidity. Auscultation, too, comes after this period only to be of any avail,—and perhaps not even until a considerable time subsequently, to the ears of many; and did delicacy oppose no obstacle to uterine examination, until now the finger or the speculum of the most experienced obstetrician, may be unable to detect the true nature of the case.

The presence in the urine of what has been called—but absurdly so—*Kiestein*, is a very important evidence of the existence of pregnancy. It has been found by Dr Golding Bird[1] as early as the second and third months after conception; but as it appears to exist only in small quantities in the early months, as the urine must be allowed to settle for a number of days before the peculiar greasy, cheese-odoured pellicle can be obtained, it is evident that this is not a test which can be often or conveniently employed, especially in the most important class of cases in which the physician is consulted. The fact of the unvarying existence of this substance, however, may I think be assumed as strongly corroborative of the value of the milk test, as it shows the presence of the secretion at an early period; for without doubt, as Dr Bird expresses it, "the imperfectly formed secretion of milk, not having a ready exit by the mammæ, is taken up into the circulating mass, is separated by the kidneys, and eventually escapes from the body in the urine."[2] With the aid of the microscope, I have fully satisfied myself that this product contains some of the elements of milk—the largest amount of which is probably caseous matter, mixed with crystals of the triple phosphate of magnesia—(Fig. XII. No. 1.) That the milk should be thus changed in appearance—if the theory given above is correct—is not surprising, on account of the process which it must go through before its elimination can take place. As a contrast to this, I have given a representation also of the change produced on milk after absorp-

[1] Guy's Hospital Reports, Vol. V. p. 16 and 25. [2] Ibid. p. 22.

tion from the alimentary canal, and excretion by the urinary apparatus—(Fig. XII. No. 2.) It is the greasy pellicle of an infant's urine allowed to settle for fifty-eight hours, and observed by the microscope. The child was deriving its sole nourishment from the breast, and was in the possession of perfect health. A few pretty well-formed globules are apparent, but most of them have lost the rotundity of the original milk globule—are flatter, and present a much thinner investing membrane. One ounce of the fluid was sufficient to yield a well-marked scum; no crystalline matter was present. The delineation of the kiestein which I have given, closely resembles the aspect of caseine when precipitated from the serum of milk by the action of acetic acid,—as is depicted in Hassall's Microscopic Anatomy of the Human Body, Plate XV. Fig. 5.

III.—*The Pathological Changes of Milk; the Value of the Microscope in the Observation of such; and Hints regarding Lactation and the Choice of Nurses.*

I now proceed to remark on the deviations from the normal condition of the milk—in other words, *its Pathological Changes*—which are detectable by the use of the microscope only; and to point out the importance of a knowledge of these, and the circumstances connected with them in practice. For many interesting and instructive remarks on this branch of the subject, I would again refer to the excellent treatise of M. Donné. Having made the greater part of my observations before becoming acquainted with his researches—while much trouble was unnecessarily occasioned, I have had the satisfaction of verifying all that he advances; and if I am not able to bring forward much that is new regarding the microscopical aspects of the mammary secretion in health and disease, I may perhaps succeed in drawing more attention to a subject which has been much neglected, and placing in a clearer light facts which appear to be little understood, and which are high and wide in practical import.

M. Donné makes no allusion to the existence of the mammary secretion previous to the last month of pregnancy, and consequently has given no account of its microscopic characters at an earlier date. This defect I have been able to supply, as will be seen in the description given at p. 72. The Figures II. and III. show the peculiarities during the third and fourth months, and IV. and V. during the seventh and ninth months of gravidity. Drawings illustrative of appearances at intervening periods might easily have been multiplied from the numerous observations made; but those given will suffice to show the chief phases of the secretion during the progress of gestation; and that, while in the earlier months the elements of good milk are apparent as the term of pregnancy approaches towards completion, there is a manifest improvement in the quantity and quality secreted—gradually becoming better adapted for the purpose for which it is so admirably designed. It may

especially be remarked, that the foreign matters, such as the epithelial scales, are less likely to be met with; that there are fewer large butyraceous globules present; and that, from the lessened viscidity of the serous part of the secretion, there is a smaller tendency to aggregation among the milk globules. The colostric bodies, which are present from the very first, continue so, as I have already stated at p. 68, until perfect convalescence from child-bed. It sometimes happens, however, that the milk is not so soon, or is sometimes never, freed from its colostric characters in one form or another, or that these return again at some period during lactation, after the secretion has been perfectly pure. When this occurs, as M. Donné has had the merit of showing, the health of the mother in affording such unwholesome supplies, and the child in receiving them, are placed in equal jeopardy. Of the truth of this opinion, I have found ample proof. M. Donné goes the length of expressing his belief, that from microscopic examination of the secretion alone in the last month of pregnancy, we may ascertain whether a woman is likely to prove a good nurse or not.[1] This I consider to be extremely probable; but be it so or not, there is no doubt that, while certain information cannot possibly be obtained from the external characters of milk during suckling, such as colour, consistence, and taste, or by the application of chemical tests, the microscope enables us at once to detect deviations from its normal condition. Assuming, therefore, the appearances described at p. 67, and depicted in Fig. I., as the standard purity of the secretion, it is surely rational to employ this means—while other considerations are not neglected—in determining those momentous questions which so often occur in regard to the propriety of a mother suckling her own offspring, as the strong ties of nature and the suggestions of duty would dictate, the length of time lactation may be continued with safety to herself and benefit to her child, or the choice of a nurse as a substitute, by whom the future health and happiness of the infant may be so materially affected.

During lactation, the nurse cannot be indisposed without an immediate change occurring in the properties of the milk; and to such vitiation is evidently attributable many of the alarming illnesses of infants. Every nursing mother knows, how apt a purge taken by herself is to affect the child, while she obtains perhaps little benefit from its use; and how speedily some articles of diet excite flatulent distension and colic pains in the infant. It is also a well-known fact, that when a nurse is caught in a shower and gets damp, or is exposed to any sudden danger, or hears any painful intelligence, and becomes violently agitated,—the child is apt to be rapidly and forcibly affected. This latter cause has been known to produce convulsions, and even death.[2] Hence, it is the duty of those who have the charge of young and inexperienced

[1] P. 406.
[2] Eberle on Diseases of Children, p. 35.

mothers, to give wholesome advice regarding the diet and regimen, the care of the breasts, the subjugation of the desires and passions, the cultivation of equanimity of temper and cheerfulness, and the avoidance of unsuitable medicinal agents; and hence, also, the need of keeping a strict look-out on the condition and conduct of the hireling nurse, who has not such strong natural incitements to caution, or motives to self-denial. Now as it is evident that it is through the channel of the breast that many disorders are conveyed to the child, it is equally clear that, in the elaboration from the blood of the milk in such instances, some alteration must take place in its nature, whether perceptible to our senses or not, imparting to it an injurious tendency. As it is in the atmosphere, however, which we breathe, and which may convey to us some severe or pestilential disease, and yet the utmost efforts of science for detection and analysis may be baffled; so perhaps the most skilful histologist, with the best optical instruments, and the most patient attention, may never be able to discover minute deviations from the normal state of the mammary secretion corresponding to the various diseased conditions of the mother, or the disorders produced in the child. Thus, there has nothing hitherto been ascertained peculiar or specific in the milk of a syphilitic nurse, although the virulence of the taint communicated is obvious enough; thus, too, nothing distinctive has been observed in the secretion of the nurse who may be communicating strumous disease to the child of another individual free from all hereditary taint, which in after life exhibits itself in one or other of the dreadful forms of diseased glands and joints, phthisis, or insanity. Even in the secretion from the breast of a patient extensively affected with cancer, which I lately had an opportunity of examining,[1] I could detect nothing essentially different from the appearances of fluid taken from an engorged breast, or in the cases hereafter to be mentioned. The only pathological changes of the milk which have as yet been observed, are the colostric condition already described, and admixture with pus. Associated with the more severe ailments of the mother, which seriously affect the child if not withdrawn from the breast, I have observed that the granular bodies are larger or more numerous, and more frequently of a rich amber colour than of the yellowish or greyish tints observed prior to delivery, and during child-bed. There seems also to be a much greater tendency to aggregation of the real milk globules from the amount of mucoid fluid present, which is turned into a most tenacious mass with ammonia; and there is often seen on the field of the microscope thick patches of grey, yellow, or deep amber-coloured matter, floating raft-like, with milk globules resting on

[1] The tumour in the breast of this individual was first observed about three months previously, and had since grown with great rapidity, involving a large portion of skin, and implicating a gland in the axilla. The child, now sixteen months old, although only partially fed from the breasts, was sickly-looking, and troubled with bowel complaint.

them or clinging to their edges. These characteristics vary in degree in relative correspondence to the deranged health of the nurse and nursling.

In instances of returning catamenia,[1] a single day is sufficient to occasion a considerable change in the milk; and in this affection, as is well known, the infant often immediately refuses the breast, and if not so, vomits severely, and is pained or purged. This abnormal change is seen to a still greater extent in cases of fever, of which Fig. VIII. No. 1, is an example. The patient from whom this fluid was taken, was in the fifth day of the fever. She had made an excellent nurse during two months, but had not suckled for the last two days. There was no engorgement of the breasts, but, on the contrary, they were pendulous and almost empty. The infant in this case did not suffer, as it was withdrawn early from the breast, and suitable nourishment substituted; and as the fever was of short duration, and not very severe, the secretion was kept up by a little being artificially drawn off from time to time, until the child could be reapplied with safety. The same appearances of the milk are observed in cases of severe dyspepsia and general debility, such as is depicted in Fig. VIII. No. 2. This patient nursed nearly eight months, during the two last of which she had gradually been losing strength and flesh, and complained of violent headachs, vertigo, disrelish for food, and other symptoms of indigestion. The child during the same period was much affected, although perhaps dentition may have had a share in its ailments. It was pale, soft, fretful, and had occasional attacks of violent abdominal pain, vomiting, and diarrhœa. Weaning was in this instance recommended to save both mother and child from certain ruin; and the step was followed by the best consequences, more especially to the health of the child.[2]

Many similar cases have occurred to me, which it is unnecessary to particularize,—cases happening at various stages of nursing, some of which required its immediate and entire abandonment, some a temporary cessation from it until the general health was restored to its normal condition, and others where only a partial performance of the function was permitted. The treatment applicable to those various cases, such as the choice of a substitute nurse, the kind of

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[1] As yet no blood globules have been detected in such cases; and it appears that blood is never secreted from the lactiferous tubes during suckling. When blood is drawn, it is from rupture of a capillary vessel by too strong suction.

[2] It appears that in cow's milk granular bodies and agglomerations of globules are present for some time after calving, and that when the animal falls into bad health there is a reappearance of the same characters; and M. Donné has shown that in the disease called *la cocote*, characterised by fever, pustules in the mouth, udder, and hoof—a malady apparently analagous to our "murrain,"—the milk exhibits the same abnormal aspect. Hence the value of a microscopical examination of an article so important in domestic economy, when it is suspected to be rendered impure by the disease of cattle. And great use may also be made at times of optical help and chemical reagents in detecting the adulterations of milk by various substances.

supplementary food proper at different ages of infancy, &c., shall be briefly noticed at subsequent parts of this paper, in so far as is consistent with its special object.

When nurses are affected with returns of the catamenia, I do not think it necessary to put a complete stop to suckling, unless they recur frequently, and are evidently impairing the mother's health. For the security of the child, it is enough to withdraw it from the breast during the continuance of the flux, and otherwise supply suitable nourishment, while the secretion is drawn off several times a-day at regular intervals, either by the lips of an attendant or a suction apparatus:—the first of which modes is preferable, if done slowly and tenderly. I have followed the same practice in other short illnesses of the nurse, occurring prior to the proper time for weaning, until the constitutional disturbance subsides. An engorgement is thus avoided which would produce a bad effect on the glands, and prove certainly hurtful to the child when suckling is resumed. This is known to happen in simple engorgement even of one breast, while the other is performing its proper function, for the milk in it becomes viscid and colostrous. It is of obvious consequence, therefore, to impress upon the inexperienced, from the moment of delivery, the necessity of managing the breasts so as to avoid any cause which might favour over-retention of milk in the mammæ, or a determination of blood to the organs, and thus occasion engorgement, febrile excitement, and perhaps even inflammation and suppuration. It is always expedient during lactation to have the breasts regularly and alternately drawn at intervals of from one hour and a half to five or six hours, according to the age of the child, generally oftener in the case of young children, and less seldom by those who are further advanced, and who are the length of obtaining other kinds of nourishment; and when an engorgement has been permitted to occur, the first drawing off should, if possible, be done otherwise than by the child. It is bad for the health both of mother and child, and materially affects the condition of the mammary gland and the quality of the milk, to deny the breast long after birth;[1] and it is equally bad practice at a subsequent period to give it too frequently,—whenever, in fact, the infant exhibits the slightest uneasiness or pain. To discuss such topics, however, would extend this communication to too great a length, and moreover they do not strictly belong to the subject of our inquiry.

Should an inflammation of the breast be established, the best practice is to remove the child from it at once, and permit it to drink only from the healthy one; while a little of the secretion, if pain will allow, is drawn off from time to time as tenderly as possible. For before suppuration has begun, in consequence of the

[1] The first trial ought to be made within a few hours after delivery, and repeated at intervals of from three to four hours, until the secretion is fairly established, which is generally in from three to five days.

congested and inflamed state of the glands, the quality of the milk becomes much changed, as already noticed; and after an abscess is formed, according to M. Donné, pus corpuscles may mix with the milk in the lactiferous ducts, and pass through their orifices in the nipples, consequently becoming unwholesome and unsafe for the child if still at that breast. That this morbid admixture always takes place I am not prepared to admit; for although I have often seen pus and milk mingling and flowing out at the openings of abscesses and sinuses, the milk from the nipple in these cases appeared, from its thin consistence and bluish-white colour, to be genuine and unpolluted. Microscopic examination, however, will of course be the certain means of detecting the presence of pus; and although in the only two cases I have had an opportunity of making use of the microscope—cases of extensive suppuration—I was unable to find a single pus corpuscle, I do not doubt the correctness of the statement made by so accurate an observer as M. Donné. I think it not unlikely, however, that the delineation given in Mr Hassall's work[1] has been small grey-coloured colostric bodies in tight envelopes, rather than pus corpuscles. I infer this from the large size of the latter compared with the former bodies, and from the granular appearance given to them. The appearances of some of the colostric granular bodies in No. 2 of Fig. IX., are illustrative of this conjecture; and the fluid depicted in this case was taken from a breast which had been suppurating and healing up alternately for three months, but had closed finally only ten days before, leaving the gland hard and knotted. The less regular margin, the spotted and opaque appearance of the pus corpuscles, compared with the clear border, spherical form, and translucent aspect of the real milk globules, ought to be good diagnostic marks in such cases. Should there, however, at any time be difficulty in deciding as to the admixture of pus with milk, the use of two chemical tests under the microscope will settle the point; for while real milk globules are readily soluble in ether, and are unaffected by a caustic alkali, the latter at once dissolves the pus corpuscles, leaving the milk globules untouched. Should any blood globules happen to be present, they may be disposed of by the addition of acetic acid, without interfering either with the pus or the milk.

In the milk of all women who nurse beyond what may be called the natural time, namely, nine months, I believe that the colostric characters will soon be assumed. Such has been the case in all the specimens I have examined microscopically. This, however, may happen some months earlier to women of a particular constitution, and in such instances lactation persisted in must be attended with bad consequences. Hence the utility of the microscope in enabling us to decide when the child should be taken away from the

[1] Microscopic Anatomy of the Human Body. Plate XV. Fig. 1.

breast, and to guard against the choice of a substitute who may not herself be better qualified to furnish a healthful supply.

The case already mentioned, accompanied with the delineation of the milk, Fig. VIII. No. 2, is an instance of early unfitness for nursing, and of the imminent danger from continuing it. Another example I have given in Fig. IX. No 7. The patient from whom this specimen was taken had only nursed for six months. It was her first child; and about two months after delivery she was disabled from suckling with one breast in consequence of suppuration, although the abscess healed up in four weeks. She was much emaciated, dyspeptic, and presented on different parts of the body cutaneous cysts, known by the name of the *molluscum contagiosum*, of various sizes, from that of a pea to a large bean. Her child had the same affection to a considerable extent, eczema behind the ears, constant diarrhœa, and a weakly ill-nourished body. From the breast not suckled I could easily obtain a little fluid; and it presented characters identical with those in the case referred to at p. 80, and depicted in No. 2. of Fig. IX. The colostric bodies were seen to the number of from five to twenty in every drop placed in the field of the microscope; and these were also of small size, mostly ovoid in shape, and girt with a tight envelope. Were it requisite, I could quote many other cases in which microscopic examination satisfied me, that suckling at different periods prior to the termination of the ninth month could not be carried on without danger both to mother and child, or to either; and in several instances I have also been enabled to prove, when otherwise I could not have done so, that the illness of a child was in no way attributable to the milk of the mother. Thus the most tender and important objects and interests may be secured. The attention is at once directed to other causes of ill health; and the proper course of treatment is made more apparent. While in one instance violence is not done to the feelings of an affectionate parent, in depriving her unnecessarily of the pleasure and privilege bestowed upon her by nature, and by the transference of her little one to the breast of a stranger; in another instance, pain is not given to the hired nurse by an uncalled-for interdict on her future services. Thus both may be permitted to exercise a function which, from physical changes and mental sympathies, may prove most salutary to future health; and the child has continued to it the supplies of food best adapted to its tender age, and is not hazarded either by a change of milk, or by an artificial substitute, at a time when ill health makes the success of such an experiment more uncertain and unsafe. Thus too, by the help of the microscope, we may be able to distinguish between instances of real inability to nurse and of sheer disinclination to do so. In this age of luxury and refinement it is to be regretted that there are so many who belong to

the latter class,—individuals who seek to escape the trouble connected with the rearing of offspring, and the trammels which may interfere with a free indulgence in the pleasures and gaieties of the world.[1]

Having now seen that the milk sometimes becomes impure, and consequently injurious, before the completion of the ninth month—the usual period for weaning, I go on to state the morbid changes which are apt to occur in it when that term is exceeded.

In a very few weeks after this period, and I should say almost invariably at the conclusion of one year from the birth of the child, the secretion is found colostric even in the healthiest women. To this I have met with no exception; and, seeing that such is the case, I believe that lactation continued beyond the ninth month is decidedly improper. When mother and child escape injury from such a practice, it is not in consequence of the ability to give, and the suitableness of the supply to the wants of the infant economy, but from the strength of the constitution of both to resist the evil effects which it tends to produce.

The impropriety of such prolonged lactation may appear evident to any one who reflects that the child is now being endowed with teeth, and its organism in all respects so far advanced in development as to require a much more nutritious fare than the mother can supply. Mothers, however, are often found continuing the child at the breast from personal considerations, without particular reference to its wants. What I allude to is, the supposition that during nursing protection is afforded against the occurrence of a new pregnancy. This idea I think is an erroneous one; and it is to be regretted that medical men have not set themselves more against the practice founded upon it. While it is admitted that those who do not nurse at all are apt to bear children in quicker succession than those who do; yet that those who delay weaning beyond nine months are less likely to conceive soon than those who do not, is opposed to all the experience I have had in practice. When fertile power is very strong, a new gestation will commence a few months after the last child-birth, even although the lactic function is in operation;—hence the frequency of pregnancy in the sixth month of suckling; when it is strong, conception is sure to take place about the termination of the ninth month of

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[1] The best artificial nourishment is cows' milk, but diluted from one-third to one-half, slightly sweetened, according to the age of the child, as it is richer than human milk. The milk of the goat and sheep are nearly similar in strength, and that of the ass is lighter; but were the taste and smell of all these agreeable to every child, they are not generally and regularly procurable. The "*pan and spoon*" system is unquestionably pernicious to newly-born children; but when the fourth month is attained, then supplies of rusk, arrow-root, barley-meal, and other light farincaceous food, may be begun in small quantities, and varied as they are found to agree with the infant.

suckling; when it is ordinary, conception generally occurs about one year after the last delivery; and when it is feeble, many months more, or years may elapse, ere the woman becomes again pregnant. On the whole, therefore, it appears that there is no certain relation between periods of lactation and gravidity, and that the latter condition is dependent on constitutional peculiarities. But even did certain data establish the supposed protective influence of prolonged suckling, the end desired would not justify the means adopted, if it is found to endanger seriously the constitution of mother and child. On the other hand, supposing pregnancy should occur while a child is at the breast, as often happens, unknown for a considerable time to the individual herself, the effects on both are still more hurtful; or if, as is also very commonly the case, the mother goes on nursing for twelve, eighteen, or twenty months, or even longer, in the vain hope of averting a future evil, she is not only periling her infant's life, but as she may become pregnant immediately after the weaning, between alternate long-continued suckling and gravidity, her vital energies are so exhausted as to entail a train of the most disastrous consequences on her own constitution and on that of her offspring.

When a woman becomes pregnant during lactation, if other symptoms do not enable us to give an opinion, the microscope is a most important diagnostic guide; for then the colostric characters of the milk are apparent, and thus we can, without hesitation, advise an immediate abandonment of suckling.

The consequences of unduly prolonged lactation are sufficiently well known to render a lengthened statement unnecessary. The generality of women are in the possession of the best of health, and in the enjoyment of the greatest amount of earthly happiness while nursing, unless they are oppressed with much fatigue, or harassed with anxiety or other ills of life. It is a natural function, in the performance of which the warmest sympathies and liveliest emotions are excited, and a tone thus imparted to the physical constitution as well as to the mind. Nor should the good effect of lactation, properly performed, cease with weaning. It is permanent, and tends to the prolongation of female existence; but when continued beyond the term which nature has pointed out, it exhausts her strength, and, according to the character of her constitution, so are the evils resulting more complicated, severe, or lasting. From a consideration of this, it is easy to perceive that an offspring reared from an exhausted body, must, like the plants of a worn-out soil, pine away a sickly and delicate existence. The most common evils entailed on a nurse by this practice are severe headachs, giddinesses, blindnesses, inordinate and irregular action of the heart, loss of appetite, general debility, lowness of spirits and hysterical affections, sinking feelings, pain in back and loins with dragging sensations, leucorrhœal discharges, and menorrhagia. Such symptoms variously combined, and many others, may exist during suckling,

and may continue long after weaning, inducing severe functional disorders, or incurable lesions of the most important organs, more especially of the brain and uterus. Then as regards the child, during the first nine months of its existence, no kind of food answers so well for the purposes of growth, nutrition, and the support of life, as that provided by the mother's breasts; but so soon as the fountain becomes impure, from being overdrawn, the most deleterious effects are produced, where formerly there was apparently the best of health. If children thus exposed are not sustained by a great deal of nourishment otherwise, the consequences are soon displayed in their meagre bodies, pale countenances, in the vomitings, purgings, cryings, spasms, or convulsions; and subsequently these victims drag out a miserable life, or fall before the first accidental illness or epidemic malady with which they are assailed. I am quite convinced that a rigid inquiry into the proximate, as well as ultimate causes of infantile and maternal death, would afford a melancholy return in regard to the consequences resulting from ignorance and culpable imprudence in the matter of lactation.

The milk of those who overnurse presents the diseased characters already described, varying in different cases, not in kind, so far as I can discover, but only in degree. These are well depicted in Fig. X. 1 and 2; and it will be seen that, in addition to the colostric granules, the most important morbid change is in the reappearance of large oleaginous globules, and in the excessive agglutination of all the bodies together. When specimens are first placed on the glass under the microscope, there are scarcely ever any independent motions seen among the globules. Example No. 1, was taken from a patient who had nursed one year, and who, during the last three months, had become pale, thin, feeble, dyspeptic, and affected with psoriasis. Her child, on the other hand, was evidently ill-nourished, was latterly much troubled with diarrhœa, and had had several convulsions. Example No. 2, is the milk of a patient who was in the twenty-second month of lactation. She had been weakly for upwards of two years in consequence of repeated attacks of ague when in Canada West, and was now much emaciated, troubled with cephalalgia, palpitations, pains in the back, downbearing sensations, and was menstruating every three weeks. Her child, although obtaining a good deal of supplementary nourishment, had had much bowel complaint, was pale and flabby, and had not been as yet able to walk. Fig. VII. 1 and 2, although designed to illustrate the bad quality of the secretion obtained after weaning, in contra-distinction to the character of the fluid found in the breasts in the early months of pregnancy (*Vide* p. 72), it may be worth while to notice here, that the individuals from whom the specimens were taken, may be adduced as proofs of the evils resulting from prolonged lactation. The former had been confined one year and a half previously, and nursed for fifteen months. She menstruated

once prior to weaning, and had done so every two weeks since. She suffered chiefly from pain in the back and debility, and is yet in very infirm health. The latter had been confined two years and a half previously, nursed for fourteen months, and ever since had been losing flesh and strength, had an abortion six months ago, since then almost constant menorrhagia, and is now undoubtedly affected with phthisis. The children of both parents, I may also notice, have been extremely weakly. Many such examples might be added to these; but I think enough has been brought forward in evidence of the bad consequences of prolonged suckling. The physician cannot too decidedly set his face against a practice so disastrous, or be too careful in his warnings to young and inexperienced mothers.

It has been supposed that the milk, which in its normal state is distinctly alkaline, may become acid in the breast, and consequently injurious to infant health. No data, however, have been given for this opinion; and I am inclined to think that acidity can only occur when the milk is removed from the breasts, and allowed to stand for some time before it is examined. I would thus also explain the appearance of the beautiful confervoid vegetation, the *penicillium glaucum*, which springs from the milk globule. M. Turpin has imaginatively conjectured this growth to be the cause of the knotted condition of an engorged breast, germination having taken place in the lacteal ducts from undischarged milk. The idea cannot, however, for a moment be entertained. The milk newly drawn is invariably alkaline, it soon becomes neutral, it ere long is found distinctly acid, ferments, is decomposed, and then readily gives origin to confervæ and vegetable infusoriæ.

By the microscope we are enabled with certainty to distinguish poverty from richness of milk;—the former by the sparing number and small size of the globules, compared with the quantity of the serum in which they swim; and the latter, by the large size and number of the globules. The naked eye cannot inform us satisfactorily whether, in a particular instance, the secretion is too thin to afford sufficient nourishment to the infant, or too rich for easy and healthful digestion. Nor can the healthful or unhealthful aspect of the nurse in every instance warrant more than a guess as to the quality of her milk, or her ability to furnish adequate supplies. Microscopic examination, however, will give decision to our impressions, and qualify or corroborate the opinion which we may have been inclined to form of the capacity of a mother to suckle, wholly or partly, or of the qualifications of a nurse as a substitute, and thus solve occasionally very great difficulties, and protect most important interests. The general considerations which ought to guide us in the choice of a nurse are of course not to be overlooked. If the mother has a good constitution, but is from any cause disqualified from suckling her own child, then we should look for a nurse as nearly re-

sembling her in constitution and physical appearance[1] as possible; but if, on the other hand, she is delicate or impaired in health, and if there is any occasion to suspect that she or the child's father have a hereditarily defective or tainted constitution, then it is imperative to choose a nurse of an entirely opposite mould. Hence it is that a change of milk is often called for, and a sickly child, or one in an almost hopeless condition, may be quickly restored to health by a timely transference. A nurse, too, should be chosen as nearly of age to the mother as possible, avoiding the extremes of maternal prematurity, or the period when a woman generally ceases to bear children. That the age of the milk also should be especially attended to, may be gathered from the preceding pages, as it has been shown that the qualities of milk vary materially at different periods, in its adaptation to or unfitness for the wants of the child. It is always improper to give a newly-born infant to a nurse when her milk is several months old. When such is the case, the child seldom thrives, and is apt to become afflicted with rachitis, or some other ailment of imperfect nutrition. Dr Combe, in his excellent treatise on the management of infancy,[2] states the general qualities which are indispensable to constitute the good nurse, as "sound health, a robust constitution, freedom from any hereditary taint, cheerfulness of mind, orderly, neat, and temperate habits, patient kindliness and good-humour, and above all, spontaneous activity, and a strong and innate liking for children." He also mentions that the external appearances should be "moderate plumpness, fresh and clear complexion,[3] clear cheerful eyes, with well-conditioned eyelids, deep red-coloured lips without cracks or scurf, sound white teeth, and well-formed moderately firm breasts, with nipples free from soreness or eruption."

External appearances are, however, often deceptive; and as the main requisite in a good nurse is the supply of the life-supporting element, a microscopical examination of it is recommended as the surest and easiest method of testing its suitableness, especially when there is any ground for doubt. Our treatment of particular cases also, is thus more rationally guided. If a particular specimen of milk is found to be poor in quality, a consideration of the chemical and structural constitution of the secretion in its normal state suggests, that aliment which will supply the fatty and albuminous principles in abundance, will be best fitted to secure in the nurse the elaboration of a good secretion. This will be best accomplished by a mild vegetable and farinaceous diet, and of soups in which are suspended a considerable amount of oleaginous matter. As a proof of the capa-

[1] Children of tall spare women are said never to thrive on the milk of those who are short and stout.

[2] Edition 1847, pp. 74 and 76.

[3] Too much weight, I believe, has been attached to the complexion of nurses, as to whether dark or fair women are preferable. Those who present a medium aspect are the best, always keeping in view the other qualifications desirable.

bility of the former articles to afford good milk, I may instance the dainty food which the cow and the sheep can furnish to their young; and of the fitness when both are combined, I may instance the success with which the countrywoman can perform the function of suckling. All experience demonstrates that strong meats and drinks taken during lactation, beget a heat and irritability of the system, if not feverishness, and thus lessen rather than increase the amount and suitableness of the secretion. In these remarks, I do not mean to aver that solid animal food and stimuli should never be taken by nursing women. Habit, and many other considerations, must render a considerable allowance necessary; but I mean to caution against the immoderate use of a highly nutritive and exciting diet, and a dependence on such from mistaken ideas of expediency. The benefit derived from stimuli in general, I very much question. They induce an unnatural excitation of the nervous and circulating systems, communicate only a forced strength to the constitution, which erelong leads to increased supplies to maintain it, and ultimately to great physical, mental, and moral evils.

When the milk is found too rich for the digestion of the infant, and is creating disorder of its stomach and bowels, it is of importance to give diluents freely, such as barley water, or thin gruel prepared from groats; the latter of which is particularly beneficial, especially in the earlier days of suckling. It is of great consequence also, to attend to the state of the breasts in all instances of faulty secretion. In cases of too great richness of the milk, it has been observed by M. Péligot,[1] that allowing it to remain longer than usual in the breasts, tends to an increase of its serous or watery part, and therefore the child should not be applied in such cases very frequently. Regarding the general management of the breasts, I have already, in the foregoing pages, thrown out a good many hints, more especially as to the prevention and treatment of engorgements. In the case of weakly and unproductive nurses, a great deal may be done to improve the quantity and quality of the milk, by due attention to the regularity and frequency with which the breasts are given, and the amount of supplementary supplies which may be necessary for the infant at different stages during lactation. Although many interesting topics connected with the above subjects, and the influence of exercise, mode of life, &c., on successful lactation, yet remain for discussion, these must be set aside for the present, as I have already exceeded the limits permitted me.

EXPLANATION OF THE PLATE.

Figures magnified 206 *diameters.*

I.—Example of healthy milk at the best period of nursing—mother and child being in good health.

II.—Fluid taken in the third month of pregnancy.

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[1] Quoted by M. Donné.

III.—Fluid taken in the fourth month of pregnancy.
IV.—Fluid taken in the seventh month of pregnancy.
V.—Fluid taken in the ninth month of pregnancy.
VI.—Milk three days after parturition, showing the characters of the colostrum, the appearance of the granular bodies in their perfect state, and the stages through which they pass in disappearing.
VII.—No. 1. Fluid taken five months after weaning; patient not pregnant.
No. 2. Fluid taken one year after weaning; patient not pregnant.
VIII.—No. 1. Milk of a patient in the fifth day of fever; the secretion formerly healthy.
No. 2. Milk of a patient in the eighth month of suckling, labouring under severe dyspepsia and general debility; child affected with vomiting and diarrhœa.
IX.—No. 1. Milk of a patient in the sixth month of lactation; her symptoms being emaciation, dyspepsia, an eruption of the *molluscum contagiosum*, and ability to suckle only with one breast, the other having been early destroyed by suppuration. The child was affected with the same eruption, and presented the general appearance of being ill nourished.
No. 2. Milk from a breast which had suppurated repeatedly, and finally closed up ten days previously. Mother ceased to suckle at same period, after having nursed fourteen months,—but not constantly with the diseased breast:—child weakly.
X.—No. 1. Milk of a patient who was in the twelfth month of suckling. She was thin and debilitated, while the child was affected with diarrhœa and convulsions.
No. 2. Milk of a patient who had nursed for twenty-two months; herself extremely weak, affected with cephalalgia, loss of appetite, &c.; and the child pale, flabby, and unable to walk.
XI.—No. 1. Examples of fluid taken from the breast of a *female* child three days old.
No. 2. Example of fluid taken from the breast of a *male* child six days old.
XII.—No. 1. Example of *Kiestein*, with crystals of the triple phosphate of magnesia.
No. 2. Appearance of the greasy scum from the urine of an infant nourished solely from the breast.

ARTICLE II.—*On the Employment of Gutta-Percha in the Treatment of Club-Foot and Fracture.* By WILLIAM LYON, Senior Surgeon to the Glasgow Royal Infirmary.

EVERY one who has attempted, is aware of the difficulties which attend treatment of the varieties of club-foot.

The apparatus of Stromeyer, splints of wood or leather, retained by starched or common bandage, embedding in plaster of Paris, or use of shoes with iron soles attached to leg, thigh, and pelvic bands, are some of them expensive, others painful, and apt to occasion ulcerations; while they are all exceedingly uncomfortable to the patient, and troublesome for the surgeon to apply, adjust, and remove, operations requisite every two or three days during a considerable part of the treatment.

So much time and attention are required with any of these, that unless liberal remuneration can be given the surgeon must make

great sacrifices. The steel apparatus, with exception of expense, probably combines the greatest number of advantages, but still much time and trouble are required for its management; while, if it cannot be afforded, the unfortunate sufferer has too often to be left to his unhappy fate.

The method by imbedding the member held in the desired position in a quantity of Paris plaster, with one exception, answers remarkably well. The mould quickly becomes hard, the pressure by the resisting parts is equally diffused, and ulceration, and all but the most trivial pain, are avoided.

We have then, by this plan, facility of application, ease, efficiency, and cheapness; but there are the objections,—that the weight of the mould necessarily occasions confinement of the patient to bed, and that it is difficult of removal; a process frequently required for regularly and gradually restoring the foot and limb to normal relations. As substitutes I have thought of a bandage of lead, zinc, or the like, applied over the foot and leg, well protected by a stocking or otherwise, but have not put them to trial.

Keeping in view, however, the mechanical requisites for treatment of this deformity (club-foot), viz. diffusion and equality of pressure, facility of application, removal, and efficiency; it occurred to me that the recently introduced article, gutta percha, possessing pliability and adhesiveness when warm, hardness and strength when cold, would combine all these particulars.

I have now employed this article very successfully in several cases, seen it, on my recommendation, used in others, and write this to bring it under the notice of the profession; convinced, after numerous trials of all the other methods, that it is by far the most manageable and effectual, and that it will enable every one possessed of even the least dexterity successfully to conduct the treatment of club-foot, thus getting rid of the cumbrous, complicated, troublesome, and expensive apparatus in ordinary use, and enabling us to confer equal advantages on the poor as were mainly confined to the rich, and surgeons in situations remote from instrument-makers, with the utmost facility to treat these species of deformities.

It may evidently be applied in various ways; but the method I have followed is this,—they were all cases of Talipes varus in both feet, and after dividing the tendo-Achillis, a procedure probably not required in very slight cases, but if not indispensable, at least greatly abbreviating the treatment in severe ones, a bit of plaster was put on the wound, and a roller from toes to knee for protection of the soft parts.

A bandage of gutta percha, thickness of a penny piece, about an inch more or less in breadth, proportioned to size of limb, softened in nearly boiling water, and dried by gentle pressure between folds of a towel, was then quickly made to enwrap the limb from toes to knee, in usual manner of the common roller.

The limb is thus encased in gutta percha, and while the material is still soft and adhesive it is firmly and equally kneaded by the fingers, so as to mould it closely to the parts, and cause the turns of the roller to adhere to each other. The limb is now firmly held below the knee, while the foot is gently twisted outwards, with the toes in the same direction, and upwards. The desired position is maintained until the material becomes cold, which it does in a few minutes, which time may be shortened by immersion in cold water; when the *light, hard, equal, strong*, mould thus formed, effectually prevents the return of the foot to its abnormal position.

In a few days the gutta percha is removed, a matter easily accomplished by regular unrolling of the turns of the bandage, reapplication is effected, a little further restoration of deformed parts being obtained; and by thus proceeding at intervals of a few days, the foot and limb are, in the course of two or three weeks, restored to their normal relations.

I suspect it is a considerable recommendation of this method by the material in form of a roller, that it is with the greatest facility removed, which I do not think would be effected without difficulty if the mould were otherwise formed. It would add greatly to the value of the practice could the material be easily softened, the parts adjusted, and removal avoided. This I have attempted, but the high temperature required to make the article soft and adhesive, rendered the effort abortive.

I have not experienced any bad effects from the advised practice; but I can readily conceive, if the mould be dimpled when soft, or otherwise applied unequally, pain, ulceration, or abscess must follow such mismanagement, although protection by a thick soft bandage, layers of carded cotton, or a stocking, will go far to prevent or remove them. So likewise, unless the urine be prevented by a piece of oiled silk from constantly soaking the bandage and skin, excoriation will ensue; and it has been said that confinement and accumulation of perspired matters will have a similar tendency; to which I can only reply that it has not happened in my cases, even when the mould has remained on for weeks, which it will very rarely require to do in club-foot.

The cause of the deformity, originally in the nerves of the limb, soon involves the muscular, ligamentous, and osseous structures, and is of course more easily overcome the younger the patient. I have repeatedly, without the least untoward occurrence, operated within a week after birth. The patients treated by the plan recommended were all infants (one excepted, who was three years of age), and it may be questioned whether the method is applicable to more advanced periods of life. From my experience by the analagous method with Paris plaster, I have not a doubt the gutta percha will answer for more advanced patients in the same degree as the more common modes of treatment; that is, that it will be found as efficacious and manageable, if not more so, than the others; but that it

will be necessary to proportion strength of the mould to the resistance to be overcome, a matter easy of accomplishment by several coatings of the gutta percha roller.

There is one drawback to the plan, but it is more in appearance than reality. The articulations in the foot and at the ankle are rendered immoveable, and assistance in throwing upwards and outwards the foot by the weight of the body in standing and walking is lost. This objection does not apply in the cases of very young infants who are unable to stand; and the paralysing effect of perfect repose on the overacting muscles, and the gain by the contraction which is permitted to the opposite and debilitated ones, has rapid effect in restoring balance of action. At all events, it is easy, when the difficulties of removing the deformity have been overcome (by far the most troublesome and painful period of the treatment), to prevent relapse by exchange of the gutta percha for such an apparatus as will allow the muscles to be called into action, the articulations to play, and the weight of the body in standing and walking to aid in the perfect restoration of position and muscular action.

It must not be overlooked that in club-foot (Varus at least) the disease and consequent deformity are not confined to the foot. The whole limb is generally affected, the leg and thigh are rolled inwards; and though the sole may be turned down, if means be not taken to counteract the involution of the other parts, the patient will nevertheless walk with the toes of each foot directed completely inwards; the one requiring to pass over the other at each step, as is often seen, or if existing in a lesser degree, will constitute the ungraceful state, called *in toes*. This is obviated in the treatment with the usual steel apparatus, by fixing the leg straps at a proper point on a metal band which passes round the pelvis. A similar result is obtained in the method I recommend, by placing the feet in a pair of boots, with the toes directed outwards, as in the "first position;" the boots being retained in the proper direction by being sewed on a piece of sole leather, and worn constantly or occasionally, as may be thought requisite. In this way one set of muscles is elongated and debilitated, another abbreviated and strengthened, as by the gutta percha on the foot and leg, and balance of power is in a short time obtained.

By perseverance in such manner for several months, I have never failed in young patients to restore healthy form and action of parts. But if the surgeon is not backed by determination on the part of the parents, and the treatment continued until perfect balance of muscular action be obtained, all our efforts will be lost, and a deformity which most seriously affects the comfort, interest, and status of the patient be unalterably entailed.

In reflecting on the treatment of this disease, and the difficulties met with by the ordinary methods; and seeing that the spasm or overaction of one set of muscles is the original cause of the displace-

ment which ensues, and the principal obstruction to restoration of form and action; it occurred to me, that if the morbidly acting muscles could be temporarily paralysed, their effects, an altered conformation of bones and ligaments, would with comparative facility be overcome. Pursuing this reflection further, and recollecting what is observed occasionally in sciatica, viz. paralysis of the muscles of the entire inferior extremity; and coupling this with the fact of reunion of divided nerve and restoration of function, and likewise that the sciatic nerve may be divided without hazard of injury to important parts: I have often put the question to myself hypothetically, would not division of the sciatic nerve by sub-cutaneous section be an easy, safe, and effectual means of overcoming the difficulties attendant on treatment of club-foot?

The anatomy, physiology, and pathology of this nerve would lead to the conclusion of its beneficial division in club-foot; but it must be remembered, it is composed of sensory as well as motory and other filaments; and observing the occurrence of sloughing from weight of the parts in paraplegia in injury to spinal marrow, a fear arises whether the like might not occur from division of the nerve; or at least that the parts would not with impunity suffer the requisite pressure for overcoming the osseous and ligamentous obstructions to restoration of form. Besides this, for any thing known to contrary, the abnormal functions of nerves and muscles might be reproduced after reunion.

Trials on the inferior animals are necessary to decide as to the consequences of what, I am aware, many will esteem an uncalled-for, unfeasible, and absurd proposal.

The obtention of a *light, strong, equal, hard* mould, quickly and easily formed, as in the method above described, suggested to me the gutta percha as a substitute for the leather splint and starched bandage, which I had often employed in the treatment of fractures.

The slight excitement which usually follows immediately after reception of a fracture having subsided, it must be the opposite of advantageous to derange and debilitate the system by five or six weeks' confinement to bed; and any plan by which the patient can be invigorated by exercise, while the injury and irksomeness of long and perfect rest in bed are avoided, is surely an important gain.

All these purposes are most effectually served by the gutta percha.

In several cases of simple fracture of the bones of the leg, the *excitement* and *swelling* having subsided under attention to perfect repose, elevation of the member, and use of the ordinary roller, lateral cushions, splints, &c., I have most advantageously practised the following method.

The foot being surrounded by a common roller, and the leg with a scultetus, adjustment of the fracture is maintained by extension; while at same time (whether beginning above or below is unimportant) the limb is encircled by the turns of a properly softened gutta

percha roller (that is, of a strip of the material two or three inches broad,) applied edge to edge so as to avoid inequality in the mould, which might irritate when it becomes cold and hard. Over this, a similarly prepared roller is passed from toe to knee, which, adhering to the one below, and to each of its own turns, forms in a few minutes (a great advantage over the starched bandage and leather splint) an equable, light, strong, hard case for the limb, completely preventing motion at the seat of fracture; and in fact acting, like the shell in some of the lower animals, as an *external* bone, within which the soft parts, and in this manner in fracture the broken bone, is protected, and effectually prevented from displacement.

If the patient were to be confined to bed, this method would be applicable to fracture at any part of the leg; but its principal recommendation is, that the patient by it is not necessitated to keep his bed. It will be evident, however, that the plan is not likely to be equally efficacious in all cases, as for instance in those near or in the upper third, where the upper fragments being short, and the lower long, consequently affording by weight a powerful lever, displacement will be hazarded; but may be prevented by extension of the mould to a portion of the thigh.

As to displacement by shortening, this may be prevented by close application of the mould below the broad head of the tibia, which will prevent it slipping upwards; and although it may be supposed that the mould will revolve, and thus alter the proper relation of the fragments, this does not happen, unequal figure of the limb preventing it.

In a week after reception of the fracture, the patient may thus, by a few *minutes'* exertion of the surgeon, be enabled by his own efforts to throw his limbs out of, and sit on the side of the bed: or the limb may be raised and allowed to fall on the bed, without injury, and in fact without pain, to the patient. He may now be allowed to turn from side to side, to walk with crutches, the limb being slung from the shoulder; conditions, the comfort of which, opposed to the ordinary long, irksome, and to the business man very inconvenient, and to all debilitating, confinement to bed, need not be dwelt upon: the advantages of which in shortening and perfecting osseous union, will be generally conceded: and peculiarly valuable, where transport of the patient for considerable distances is requisite: a circumstance recommending this method in military practice, where unavoidable conveyance, with inadequate means, in countries without roads, or very bad ones, is frequently demanded.

In fractures of the bones of leg and forearm, and in that of arm, the eligibility of the gutta percha is very evident; whether applicable to the femur is more questionable: though if the limb were contained in a tightly fitting, *light, strong, hard case,* oblique or transverse displacement would be prevented; and if the case extended to the calf or knee, and to the nates, perineum, ischium, and external aspect of pelvis, the inequalities of the limb below, within the case, and the

resistance to the latter by the pelvis above, would, from the pyramidal form of the contained parts, render displacement by shortening unlikely: trial, however, must determine the point, and I shall take the first opportunity of testing it.

Since writing above, I have employed the gutta percha in the case of a boy with fracture at junction of superior and middle thirds of femur. Extension being made, the material was applied over a roller from toes to upper part of thigh, close up to the perineum, round the pelvis, and again upon thigh and pelvis until the nates of injured side were completely covered. Immediately after, he could without uneasiness be turned from side to side, and carried in his mother's arms, and I doubt not could have readily walked with aid of crutches.

Indeed the thigh and pelvis were immoveably connected, they and the leg might be said to have been in a few minutes converted into a hollow bone, in which the nates, perineum, and tapering form of the limb above, with confinement of the foot in the mould below, entirely removed fear of longitudinal, while the tightness of the case prevented lateral, displacement. Further trial is necessary before inferring a general rule; but certainly the application had a most promising appearance, and enabled the patient to enjoy a degree of ease, and amount of motion, quite new to us in fracture of the femur of a few days' duration.

Several theoretical objections may be made to the practice, such as, that the material being applied warm, it will have contracted when cold, so as to cause injurious compression; that its frequent removal and reapplication will be troublesome and prejudicial, and yet without them the state of parts cannot be observed. None of those occurrences common to this and the treatment by starched bandage, have presented to me, and, if the recommendations are attended to, I do not think they need be feared; while some of them can be prevented, and the others are greatly counterbalanced by many advantages.

I have the method in practice and prospect in several other conditions. Six years ago, a girl was under my care on account of acute necrosis of tibia. Almost all but the epiphyses became loose, was removed by incision, and the limb then kept straight and steady by splints.

At the end of several months osseous matter was deposited throughout all the space from which necrosed bone had been removed, excepting about an inch in length in the centre; where the small apices of the pyramidally shaped portions were connected with soft tissue as in ununited fracture, and, in consequence, the limb has remained useless ever since.

Amputation was proposed by one party, removal of the soft tissue, and repose as in ununited fracture, by another. In mean time, the method of breaking up intervening soft tissue, in non-union of fracture by Professor Miller, being observed; I determined to give it a trial in the analagous circumstances of this case. The breaking up

was very freely executed, and the limb then put up in gutta percha as described, where it has remained free of uneasiness for eight or ten days, and, at least, in such repose as to afford the most favourable local condition for the practice, while vigour is retained by the free exercise permitted.

This girl has had a wooden sole and pin attached to the gutta percha case, the diseased limb being shorter than sound one, and she now walks freely about the ward with assistance of a stick. She is enabled to do this by the weight of body being thrown on the broad part of the tibia within the case, while the hardness of the latter prevents the foot being pushed upwards by pressure from the sole; thus showing another important application of gutta percha, viz. for the formation of a box to receive the stump after amputation in the leg, thigh, fore-arm, or arm. This can be made in a few minutes at a trifling expense; will be light, easy from equality and diffusion of pressure: sufficiently strong: it can be made of any degree of strength: and can be prolonged to the proper extent, either by a pyramidal coil of the same material, or by attaching a wooden cup and pin.

For long stumps of the thigh or leg, I have a strong conviction of the great efficacy of this method; our able instrument-maker, Mr W. B. Hilliard, joins in my opinion: the case of the girl demonstrates it: and I have a strong hope, that to the poor and unfortunate labouring class, who principally require such assistance, it will be found an important acquisition.

A few days ago, I dissected off a flap and removed a detached fragment, three or four inches long, which lay in the cancellous structure at the point of contact of the two fragments, in a case of compound fracture of the tibia, which has been under treatment for six months, and is still ununited. I likewise pared the smooth rounded extremities of the fragments, and, when the excitement has passed off, intend to put up the limb in gutta-percha. By the perfect repose in which, the improvement of the debilitated frame by exchange of exercise for the long confinement to bed, and the removal of the dead fragment which acted like a foreign body, I hope yet to avoid recourse to amputation.

I have likewise the material under trial for preventing motion in diseased articulations, a mode of treatment now universally commending itself in theory as in practice; and here, too, the article bids fair for supplanting the methods of accomplishing the purpose hitherto in use. When employed with this view, its application in form of roller is much superior to that of splint, the latter permitting some degree of motion.

Many other applications of this pliable material suggest themselves; but these trials and propositions will suffice at present for attracting attention of practitioners to its various and important uses in surgery.

July 16.—To-day I applied the gutta percha in another case of fractured femur. The woman, thirty-three years old, was received into hospital a week

since. The material was applied as in the case of the boy detailed above; and, in ten minutes after application, she was raised to the erect posture, walked from the side-room without pain into the ward with aid of crutches, and would have walked to her bed but for faintness. I did not allow her to bear on the injured limb; but from the great strength of the mould formed as described, and the pyramidal form of the hip and thigh, the large circumference being above, fitting into, and bearing upon the hard case, I am sanguine enough to hope that the time has passed for the necessity of confinement to bed in fracture of the femur; and that not only will the patient, in favourable cases, be allowed to turn from side to side in bed, and walk about on crutches, but that he will, with impunity, throw a considerable part of his weight upon the mould, as one who has suffered amputation in thigh, does in the ordinary box of the artificial limb.

The first opportunity which offers, I shall try a different method of putting on the material, the one described being troublesome, and requiring several assistants. I shall shape two pieces of paper sufficient to cover the member and pelvis from each side to an extent equal to that enveloped by the gutta percha in the former cases. These being softened in warm water, will be laid on each side of limb, and behind and on outside of pelvis, their edges slightly overlapping, and will be then surrounded with a common roller; an accurately fitting, close, hard, strong, light mould, will thus be formed, in which, from the resistance above and below, the limb cannot shorten, which, tightly compressing the soft parts, will also prevent lateral displacement or riding of the fragments, and I am hopeful, will enable the patient partially at least to use it in walking, the weight of the body being thrown on the upper part of thigh, hip, and perineum. What the effect temporarily of this treatment as respects speedy and perfect union, &c. &c., remains to be seen; but the following note, just received from the gentleman acting as my clerk in hospital, may prove interesting:—

"DEAR SIR,—I again saw the woman walking with the aid of crutches; she walked half the length of the ward with comparative ease, and almost no pain; —with no assistance but the nurse holding her gown. She, however, requires some lessons in crutch-walking; otherwise the result is highly satisfactory. Complains of a little pain from tightness in groin. She soon became weak, and had to return to bed.—I remain yours truly, GEORGE BUCHANAN."

ARTICLE III.—*Contributions to the Pathology of the Kidney.* By WILLIAM T. GAIRDNER, M.D.

PART I.—PATHOLOGICAL ANATOMY. (Continued from p. 21.)

III.—LESIONS OF THE TUBES AND EPITHELIUM.

SOME of these lesions have been already fully described under the head of exudation (I.); but there remain others which are not less important in themselves than those formerly alluded to, and which are very frequently found in connexion with them.

Imperfect Development of the Epithelium Cells and Nuclei.—The natural condition of the epithelium cells has been fully described in the anatomical introduction to this memoir.—(*April* No. p. 720). The size of these cells, and the thickness of the cell wall, has been stated to vary within certain limits in organs apparently healthy. The size of the nuclei is less variable than that of the cells; but in all kidneys, whether healthy or diseased, the nuclei which are most closely adherent to the basement membrane are less perfectly circular, and of considerably smaller size, than the majority of those lining the tubes, and surrounded by complete cells.

Notwithstanding these differences in the normal condition, the physical characters of the epithelium is capable of affording important information as to the diseases of the kidney. In very many pathological conditions of the organ, the nuclei occur in various places almost wholly devoid of cell walls. They may be more abundant or more scanty than usual; and often appear in great profusion, huddled together in confused masses, and mixed with shreds of membrane and amorphous molecular matter, not soluble in acetic acid. This appearance of debris, which no doubt results from disintegration of the cell walls, most frequently occurs in kidneys which are abnormally soft and large, and from the cut surface of which an unusually large amount of turbid whitish juice may be scraped. It is usually impossible to obtain a satisfactory microscopic section; the cohesion and elasticity of the organ being so much impaired as to present no resistance to the pressure of the glasses. Such softened and altered kidneys occur frequently in fever and other diseases, and have appeared to me frequently to concur with deficiency in amount, or alterations in character, of the urinary secretion. I have not been able to arrive at any very definite conclusion, as to how far the disintegration in question may be the result of post-mortem change. However this may be, it is clearly abnormal, and ought always to suggest careful examination, as it is undoubtedly often connected with other morbid characters.

A more unequivocal pathological change (often occurring along with the above) is the small size and altered form of the nuclei throughout the organ. I have frequently observed the majority of the nuclei to be not more than half the usual size (some of them being even less); in this case they have always been destitute of cell walls, and have presented a more or less oval or slightly angular form. Sometimes they float scattered and solitary in the field of the microscope; at other times they appear aggregated together, either by two and three, or in much greater numbers. When a few of these aggregated nuclei are observed, it can usually be seen that they are not actually in contact, but are inclosed in a very delicate and transparent filmy substance, which is readily twisted about in all directions by currents in the fluid, but which, nevertheless, has sufficient tenacity to prevent the nuclei from being torn asunder. Occasionally, in the midst of this connecting substance, obscure marks of cell walls can be observed around the nuclei; and, from repeated observation of these varieties, I am convinced that this transparent and homogeneous film is nothing else than the nascent or undeveloped cell membrane, which has separated from the basement membrane along with the half-developed or young nuclei above detailed. These aggregations of young nuclei are sometimes mingled with the amorphous debris of effete epithelium, or with granules and molecules of oleo-albuminous exudation, or of lithate of ammonia, which communicate to them a dark and confused appearance. Not unfre-

quently also these masses, when freed from the tubes, retain more or less of their form, and present so exactly the appearance of the casts of the tubuli seen by Franz Simon, and many other observers, in the urine, as to leave no doubt of their identity with these bodies. —(See Figs. 17, 18.)

Desquamation of the Epithelium.—The changes above described are generally accompanied by an extremely rapid generation of nuclei, which are separated from the basement membrane in an imperfect state, and carried away along with the urine. I shall not at present enter into the subject of the changes in the urine, further than to say, that the appearance in that fluid of the immature nuclei and cells, as well as the aggregations above mentioned, forms one of the most sure and undoubted signs of a diseased condition of the urinary tubules. There is reason also to think that disease may be detected in this way, long before the kidney has undergone disorganization at all evident either to the unaided eye or to the microscope.

The analogy of the anatomical changes now described, with those which occur in diseased mucous membranes in general, deserves to be adverted to in this place. Henle was the first to show satisfactorily that the essential phenomenon of mucous catarrhs and inflammations is usually the increased formation of epithelium cells in various stages of growth, and their separation along with an increased quantity of fluid secretion. The same fact has also been clearly elicited by Lebert, in his examination into the microscopic character of expectoration.—(*Physiologie Pathologique*, Vol. I.) The analogy of the phenomena in these cases, with those presented by urine in the various disorders of the kidney, in which desquamation takes place from the tubuli, cannot fail to be appreciated, when it is remembered that the basement membrane of the tubuli is essentially a mucous structure, differing only from mucous membranes in general by its anatomical arrangement, and by its containing none of the accidental or non-essential parts of mucous tissue.

The desquamation of the epithelium of the tubuli uriniferi, has been recognised by several continental pathologists as an important characteristic of renal disease. Vogel (*Icones Histologiæ Pathologicæ*, p. 108), in particular, has furnished important observations on this subject; and several other authors might be cited as having alluded to the different forms and sequelæ of this morbid process.

In the memoirs of Dr Johnson and Mr Simon, published simultaneously in the London *Medico-Chirurgical Transactions* for last year, the desquamation of the epithelium and its anatomical results, are described, from independent observations, as characteristic of the inflammatory affections of the kidney, and as distinguishing these from the chronic *fatty* degeneration of the organ. According to Mr Simon, the latter is the exclusive result of scrofulous disease; while the desquamative disease is the consequence, in general, of a rheumatic or other *inflammatory* diathesis. Dr Johnson describes

the desquamation as occurring both in an acute and chronic form, to which he gives the names of *acute* and *chronic desquamative nephritis;* and these are held to be true inflammatory affections, giving rise to organic changes of a peculiar kind. In the description of these changes, and of their relations to the different forms of renal degeneration described by former pathologists, there are considerable differences between the memoirs of Dr Johnson and Mr Simon, some of which will be presently adverted to.

In detailing the results of my observations on the anatomical changes in the kidney, I have avoided as much as possible all speculations as to the pathological causes or symptomatic phenomena of the lesions described. This branch of the subject I hope to take up at a future period; in the mean time, however, it is necessary, to prevent misconception, that I should repeat as regards the alleged scrofulous and rheumatic forms of renal degeneration, what I have formerly said in reference to the inflammatory and non-inflammatory,—that the application of such names to the anatomical changes in diseased kidneys is subject to this objection, that none of the lesions which I am engaged in describing, can be correctly considered as having an exclusive connexion with any specific pathological cause. In particular, I believe that there is no disorder of the system in general, or of the kidney in particular, with which the desquamative process is not liable to be connected, and that its connexion with the oleo-albuminous exudation is the most frequent of all.

I have therefore thought it necessary to exclude all names having reference to such vague pathological theories; believing that the lesions of an organ must be made the subject of purely anatomical consideration before its pathology can be rightly apprehended.

In some cases of desquamation of the epithelium, it is scarcely possible to recognise any departure from the usual condition of the kidney, either with or without the assistance of the microscope. The degree of vascularity is very various in different specimens, and the epithelium thrown off is so quickly resupplied, that there is no very observable change in the microscopic condition of the tubules. In one very intense case, in which ten pounds of very watery urine, loaded with an epithelial sediment, were passed daily for some weeks before death, the kidneys were small, flaccid, and bloodless; many of the tubes were quite full of nuclei heaped closely together; some of the nuclei were undersized; the cells, when entire, were much compressed and angular.—(See Fig. 15.) In another instance, where urine was passed in large quantity and full of epithelial debris, during the last two months of life, the kidneys were found in an opposite condition; viz. large and congested, and with a firmness and smoothness of section like the first stage of the waxy degeneration formerly described.—(*July* No. p. 14.) In this case the condition of the tubuli was in most parts quite natural; in some, however, there was extravasated blood, and in others the epithelium had accumulated to an abnormal extent. In both these cases there was

imperfect development of the epithelium; but cases have occurred to me, in which this character was by no means well marked. The crowding of the tubes with nuclei, although frequently found in the earlier stages of desquamation, is not invariably present; and I have seen the tubes gorged with epithelium, in a case where none had been separated with the urine for weeks before death.

Fig. 14. Fig. 15.

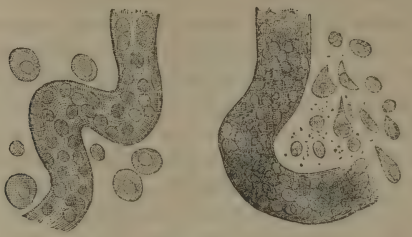


Fig. 14.—*A portion of a tubulus uriniferus normally filled with cells. It presents an obviously cylindrical form, and the nuclei are disposed on the internal surface so as to leave considerable intervening spaces. The cells and nuclei from such a tube are unaltered in form.* (250 *diameters.*)

Fig. 15.—*A tube crowded with nuclei and compressed cells. Some of the cells altered in form, and nuclei are seen mixed with debris, outside the tube. The nuclei are somewhat smaller than those of the healthy tube.* (250 *diameters.*) *Figs* 14 *and* 15 *are from the kidney mentioned in the text,* (p. 99.)

So long, therefore, as the epithelium is freely regenerated, the kidneys may preserve a tolerably healthy appearance even on minute examination. The principal characters of the disease in this stage are derived from the urine. After prolonged disease, however, further changes take place. The epithelium becomes more sparingly generated, and is thrown off in the coherent masses above described (p. 97), leaving the basement membrane in portions bare, or with a few scattered oval nuclei, much smaller than those cast off, adhering to its inner surface.—(See Fig. 16.) In the microscopic examination of organs in this condition, there are frequently seen films of such exceeding delicacy and transparency as to be only visible by very careful management of the light. They preserve the shape of the tubules, and contain no nuclei or structures of any kind. Similar films are occasionally seen in the sediment of urine. They are probably thrown off from the denuded basement membrane.—(See Fig. 18.)

Obliteration of the Tubes.—The basement membrane, which, with the few closely adherent oval nuclei above described, is now the sole remaining structure of the tubes, soon undergoes a change.—(See Fig. 16.) It loses the cylindrical form proper to it in the fresh and natural kidney, and becomes flattened by the pressure of the surrounding parts. Its cavity is thus obliterated, and what was a tube assumes the appearance of a transparent riband, dotted here and there with small oval nuclei, which, when seen at the edges, appear

to be enclosed between two layers of membrane. These riband-shaped portions of membrane appear to preserve considerable tenacity and elasticity; by their greater density, and by the constant presence of the small oval nuclei so often mentioned, between their layers, they are in most cases readily distinguished from the delicate films which have been referred to above. They are very various in diameter, but are always inferior in this respect to the normal tubes; and they appear to break up spontaneously into smaller portions, each of which contains from one to six, or even more, nuclei. These portions are of various sizes, from 1-10th to 1-4th of a millimetre in length, and from 1-120th to 1-30th in breadth. They are usually broadest in the middle, and taper to a point at both ends. The smallest of them contain only a single nucleus, and present an appearance in every respect like that of the young fibres of areolar texture, or those fusiform cells which have been called *fibro-plastic*. I think it probable that the whole of the diseased basement membrane ultimately splits up into fibres of this kind.[1]

Fig. 16. Fig. 17. Fig. 18.

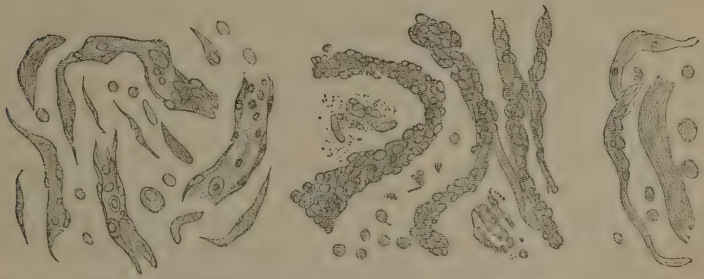


Fig. 16.—*Fragments of tubes from which most of the cells have separated, and which are undergoing the change mentioned above. Some small nuclei are seen adherent, and others free.* (250 *diameters.*)

Fig. 17.—*Debris of epithelium separated from the interior of the tubes, and presenting a mould of their form.* (250 *diameters.*)

Fig. 18—*Delicate films presenting the form of the tubes, and probably separated from their interior. Along with them a few nuclei and cells, which, however, are not imbedded in the membrane, as in Fig.* 16. (250 *diameters.*)

While these changes are proceeding, the capillary vessels, which have ceased to be subservient to secretion, are usually obliterated. The consequence of this double obliteration of vessels and tubes, is a considerable degree of atrophy in the diseased parts; and, as the atrophy takes place at first chiefly in the cortical substance, great irregularities of the surface generally supervene. Thence arises the appearance so well described and figured by Dr Bright (Plate III.

[1] I have never seen any reason to believe, with Mr Simon, that the tubes in diseased kidneys burst from over-distension, discharging their contents into the inter-tubular tissue. The separation of the epithelium from the tubes, under the pressure of glasses, takes place to a considerable extent even in healthy organs, and much more in disease; but it is the result of the manipulation, not a pathological appearance.

Fig. 2), in which, from the atrophy of the cortical substance, the bases of the pyramids "are drawn towards the surface of the kidney."

When oleo-albuminous exudation supervenes on the above derangement of the tubes, or when desquamation supervenes on the former (circumstances which I conceive to be of very common occurrence), the exudation most commonly takes the form of the granulations of Bright, which are deposited chiefly in the diseased tubes; and the atrophy proceeding around these they become salient, and the surface generally irregular, giving rise to the tuberculated state of the surface, so common in all the later stages of the granulated kidney (Bright, Plate III. Fig, 1; Rayer, Plate VII. Fig. 6; Plate IX. Fig. 8.) As the atrophy, however, proceeds, the granulations are gradually absorbed; and when the kidney has become extremely contracted and irregular, they often in great part disappear.

The atrophied portions of the kidney are usually exsanguine and of a tawny or drab colour; they have considerable hardness and toughness. Examined microscopically, they appear to consist of fibres and fusiform cells in great abundance, and of more or less granular exudation, according to circumstances. According to Henle, Eichholtz, Gluge, and others, these fibres are in great part new formations; Johnson and Simon consider them as nothing more than the compressed parenchyma of the gland, from which all the other normal elements have disappeared. I look upon them as formed in great part by the breaking up of the basement membrane of the tubes (as above-described), as well as from the parenchyma and obliterated capillaries. It is not improbable, however, that, in addition to these elements, some new fibrous tissue is formed.

The extreme stage of the atrophied kidney is nearly the same whether exudation have existed or not.

Microscopic Cyst-formation.—It occasionally happens, on examining the section of a kidney with the microscope, that we see scattered through some parts of the section a few small clear vesicles of nearly circular or oval form; they are either of a very pale straw-colour, or nearly colourless, and are perfectly clear and translucent, with a very distinct shadowed margin, which causes them to stand out in bold relief from the other textures composing the section. Their diameter is usually from 1-40th to 1-15th of a millimetre, but in this respect they vary considerably; sometimes they appear to lie in the tubular areolæ, and at other times to be unconnected with these. Very rarely they have appeared to contain a few granules; most commonly, even when there is granular exudation around them on every side, they contain nothing but clear fluid. Their refractive power is not so great as that of oil, while it is much greater than that of the spherical cells of the tubes. Hence their distinct and characteristic shadowed outline.—(See Figs. 19 and 20.)

Fig. 19.

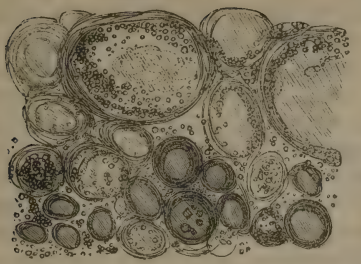
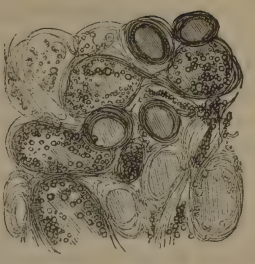


Fig. 20.



Figs. 19 and 20.—*Vesicles described in* p. 102, *dispersed amid the normal elements of the section of the kidney. There is a considerable quantity of granular exudation in the Malpighian capsules and tubular areolæ. One vesicle in Fig.* 20 *contains two or three granules, but all the rest are quite clear. In Fig.* 19, *a tube contorted, and here and there apparently much constricted, is seen to pass from the left (below) to the right (above). One of the vesicles lies over a constricted portion, and two others are seen in contact with the tube below.*

These bodies (which, however, have never appeared to me to present distinct nuclei) are probably the same with the "nucleated cells or vesicles" described by Mr Simon, as resulting from the extravasation of the epithelial cells into the intertubular tissue, and as progressively enlarging so as to form the cysts visible to the naked eye, which are so common in diseased kidneys. To these structures he attaches great importance in the pathology of the kidney, conceiving them to be the invariable result of the desquamative disease when of long standing; the kidney being, in Mr Simon's opinion, changed more or less into an aggregation of microscopic cysts, which either undergo absorption, and lead to atrophy of the organ, or increase in size and monopolize its texture. Thus, according to Mr Simon, the serous cysts so common in the kidney result from an enormous development and hypertrophy of extravasated epithelium cells, which assume the character of the vesicles he describes, and acquire the power of increase and endogenous development.

Whether the bodies described by me above, are the same with the vesicles of Mr Simon, I have some difficulty in determining; but they are the only objects I have seen which correspond at all closely with his description, unless, indeed, it were possible to suppose, as Dr Johnson appears to hint,[1] that he may have mistaken the normal disposition of the tubuli (See Fig. 1, *April* No. p. 722) for a cystic structure.

However this may be, I am satisfied that the vesicles above described are exceptional productions, and by no means invariably connected, as Mr Simon describes his vesicles to be, with the progress of the desquamative degeneration. They are seen in comparatively few cases; on referring to four, of which I have drawings or memoranda, I find two to have been congested and waxy kidneys, with

[1] See first part of article Ren, in Todd's Cyclopædia of Anatomy and Physiology, just published and not yet completed.

slight exudation, one to have been a soft and desquamating kidney, also with slight exudation, and one a granular kidney, with numerous cysts, from the size of a pea to that of a hazel-nut. On the other hand, I have examined organs in every stage of desquamative disease without finding these bodies, the production of which cannot therefore be an essential step in the degeneration and atrophy of kidneys so affected.

The origin and progress of these vesicles is very obscure. It is not improbable that, as Mr Simon asserts, they are transformed into the larger cysts visible to the naked eye; though I confess that I have not been able to trace the intermediate steps of their progress in a satisfactory manner. On the other hand, their origin from extravasated epithelial cells seems exceedingly improbable; indeed, I have already stated that I do not think the epithelium ever becomes extravasated. Moreover, the vesicles in question have all the appearance of being formed *within* the tubes, although they afterwards become separated from them.

From the occasional appearances of alternate distension and constriction presented by the tubes when undergoing obliteration, I am induced to believe that cysts may be formed by the occlusion and isolation of portions of tube which have not yet lost their power of secretion. Whether the vesicles in question are formed in this way, can only be determined by close and repeated observation; and I have not been able to obtain demonstrative evidence on this point. The observation in Fig. 19, however, though not free from sources of fallacy, appears to me to favour this view.

The larger cysts in the kidney present very strong evidence of being formed in connexion with the secreting membrane. In one instance I found their inner surface to be lined at some points with tesselated epithelium, in the form of pentagonal or hexagonal flattened cells, with circular nuclei; in another case there were oval nuclei without any distinct cells, and a large number of free oil-globules of considerable size. The existence of oil in these cysts has also been observed by Dr Johnson. Other products of secretion are also occasionally found. On one occasion I found several cysts in a kidney otherwise healthy in appearance, which contained a turbid ochrey-coloured liquid, presenting under the microscope numerous minute crystals of uric acid. Mr Simon mentions having found on two occasions xanthic oxide in considerable proportion. I have more than once observed them to contain blood in large quantity, and I have likewise found them full of a matter like stiff glue.

The occurrence of cysts in kidneys presenting a generally healthy structure is so frequent, as to lead to the idea that they must be in such cases the result of disease which has been arrested before any considerable disorganization has taken place. Many of the cases of partial atrophy of the kidneys figured by Rayer (see Plate V. Figs. 5, 6; Plate XXXV. Figs. 8, 9, 10), are probably due to the rupture or obliteration of these cysts.

Before leaving the subject of cyst-formation, I may state, that in one instance I have observed the Malpighian capsules to be occupied by distinct cysts. This case will be presently alluded to.

Dilatation and Thickening of the Tubes.—This condition, although by no means a very frequent one, is important as being characteristic, so far as I have observed, of the extreme stages of what I have called the "waxy degeneration." I have scarcely ever seen it unaccompanied by entire obliteration of the vessels, and by enlargement and increased density of the kidney. The organ has the dense, resistant feeling of fibro-cartilage, and both cortical and tubular portions have the light yellow colour, and the appearances described in the July No., p. 15, as those of the waxy degeneration in its last stage. The striæ of the pyramids appear to radiate indefinitely towards the surface, and meet the cortical substance in digitations, instead of being marked off by a sharp semicircular line, as occurs in the healthy kidney. When examined with a simple lens, or even the naked eye, the pyramidal striæ are seen to pursue an unusually sinuous course; this is peculiarly the case where they pass into the cortical substance. Moreover the pyramids are unusually broad at the bases; and the length of the straggling digitations is sometimes so great, that I have measured fully an inch and a half between the extreme end of the striæ and the corresponding papilla. Nevertheless the cortical substance is not usually diminished in quantity, being developed to a great extent between the pyramids.

This condition I have ascertained to proceed from dilatation and thickening of the tubuli uriniferi throughout the organ. The dilated tubes are usually twisted and varicose, as may be seen by inspecting a section of the pyramids with a low power.—(See Fig. 21, A.) When examined with a higher power, the section presents an appearance very similar to some tumours (of the fibrous or fibro-cystic kinds); viz. a number of compressed areolæ, enclosed by fibrous tissue, and presenting an appearance of irregular concentric rings of various distinctness, (an effect apparently due to the peculiar refraction of light by the thickened membrane.)—(See Fig. 21, B.) The

Fig. 21.

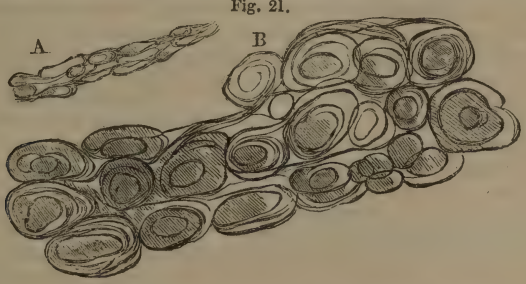


Fig. 21. A.—*Tubes in the pyramidal substance of the kidney, in the case of Campbell.* (Observation XIV.) (40 *diameters.*) B. *A similar section (as described above).* (170 *diameters.*)

nuclei are obscured or invisible, owing to the thickness of the intervening wall, but nevertheless exist in considerable numbers. The Malpighian bodies and capillaries are usually obliterated. The kidney has in fact become, like the tumours whose structure it resembles, a true non-vascular texture.

The explanation of the peculiar extension of the pyramidal striæ towards the surface in these cases, is to be found in the fact, that even in the normal condition the convoluted tubuli have a general disposition from the bases of the pyramids towards the surface, in the direction of the striæ of the cones. This is evident from the facility with which the gland tears in that direction; although in the normal state this disposition is masked by that of the vessels, which, passing in straight lines through the cones, break into a complicated net-work of capillaries at the bases of the pyramids. In the present lesion, the vessels having disappeared, and the course of the tubes being strongly marked, their disposition towards the surface becomes manifest, and the abrupt line of demarcation between the cortical and pyramidal substance, caused by the presence of the vessels, is obliterated.

A very characteristic instance of this lesion is found in Observation XIII. It may be worth while to remark, that in this case the lesion above described occurred in common with bronchial dilatation, there being thus a precisely analagous state of the pulmonary and renal texture. In the following case there are several unusual circumstances—the amount of exudation—the persistance of some of the vascularity of the surface and pyramids—and the cysts of the Malpighian bodies.

OBSERVATION XIV.—*Waxy Kidney* (*last stage, with Granular Exudation*). *Cysts in Malpighian Capsules.*—Agnes Campbell, æt. thirty, was admitted into the Royal Infirmary, February 8, 1848, under the care of Dr George Paterson. She laboured under great general anasarca, and was extremely anemic. There was a murmur with the first sound of the heart. The urine was found to be highly albuminous, specific gravity 1·012, acid reaction. She died February 20th.

On dissection, the external aspect was excessively anemic; the face was puffy, the legs œdematous. The cavities of the chest and abdomen contained a considerable quantity of fluid. All the organs were sound, except the kidneys. These were enlarged by about a fourth, and were firm and dense. The capsule was easily stripped; the surface not uneven; the venous vascularity less than usual, and irregularly distributed. The cortical substance generally was of a yellowish colour, with some opacity, and destitute of vascularity. The yellowish opaque colour penetrated between the tubular striæ towards the bases of the pyramids, which were broad, and displayed a good deal of vascular injection. The Malpighian bodies were prominent on the surface of the section, and presented an unusually transparent and pale appearance.

On examining microscopically a section by Valentin's knife, there was seen dispersed among the tubes of the cortical substance a considerable quantity of very finely granular exudation. A few cells presenting the appearance of the granular or exudation corpuscles were seen. The nuclei in the tubuli were mostly free from cell-walls. There were numerous fusiform cells and rudimentary fibres. The Malpighian capsules were thickened, and no blood-vessels could be seen in their interior, which was divided into loculi.

On separating the Malpighian bodies from the capsules (which was readily done by scraping the cortical substance), and viewing them either with strong or weak powers, they were seen to be composed of pale semi-transparent cysts, from 1-15th to 1-12th of a millimetre in diameter, and compressed together. When floated out, they presented the appearance of a cluster of grapes, except as regards the form of the individual cysts, which was circular. They were apparently retained together by a very slight medium, as by a little manipulation several of them could readily be separated from the mass. They appeared to contain transparent fluid.

The above transformation of the Malpighian bodies has not escaped the attention of Rayer, who says that in an advanced stage of the *Néphrite Albumineuse* "the glandules of Malpighi resemble small serous vesicles, mingled with others a little larger, which still later became true cysts."—(See Rayer's Atlas, Pl. IX. Figs. 6. and 7.) In the case just detailed, however, there was no enlargement of any of the Malpighian bodies.

CONCLUSION.

With the view of enabling the reader to place the foregoing observations in relation with the descriptions found in systematic pathological works, I subjoin the following short remarks on the principal physical characters usually ascribed to diseased kidneys.

Increase of Size and Weight—Hypertrophy.—Enlargement of the kidney occurs chiefly in consequence of three conditions; 1st, from sanguineous engorgement; 2d, from distension of the tubes by secretion or exudation; 3d, from permanent dilatation and thickening of the tubes. Of all these causes, the second is by far the most common. The last is characteristic of the waxy degeneration formerly described.

The quantity of liquid in the tubes is at all times subject to so much variation, that it is difficult to say what amount of increase of weight may be thereby occasioned without the existence of any positively morbid condition. It is not very uncommon to find kidneys otherwise not differing from the healthy standard, about double the usual weight, or between seven and eight ounces each. I have more than once found them to weigh nine ounces each, with very slight marks of disease. When the weight much exceeds this, it is probable it arises from the rare combination of vascular and tubular engorgement.

In kidneys containing oleo-albuminous exudation, the greatest increase of size is attained when the exudation is universal, and unaccompanied by desquamation.

Cystic degeneration of the kidneys, dilatation of the pelvis and ureters (Hydronephrose, Rayer), &c., also give rise to great increase of size and weight.

Diminution of Size and Weight—Atrophy.—This condition sometimes occurs to a certain extent in emaciated subjects, without any disorganization, owing to the diminished activity of secretion. More

frequently, however, it is the result of separation of the epithelium, followed by contraction and obliteration of the tubular structure.

Atrophy, from this cause, is liable to supervene in all other varieties of renal lesion, except the waxy degeneration, which appears to lead to a permanently hypertrophied condition of the organ. In kidneys enlarged from exudation, the occurrence of desquamation and its consequences is frequent; and the diminution of size in such cases, is often not followed by a return to the natural condition but by permanent atrophy.

The course of all disorganizing diseases in the kidney, is to produce first enlargement, and then contraction of the organ. In the extreme stages of the atrophy which results from exudation, exudation is often nearly absent. When exudation therefore, even in very sparing quantity, accompanies a contracted condition of the kidney, there is a probability that it has been abundant at some former period.

Irregularities of Surface—Tuberculated and Granulated Kidneys.—The smoothness of the surface in the kidney is destroyed either by unequal dilatation, or unequal contraction of the tubuli of the cortical substance. The former takes place in the waxy degeneration, the latter in the desquamative processes.

The most frequent irregularities of surface are formed in connection with the granulations of Bright (the origin of which is described, *May* No., pp. 797-8). These are invariably formed when exudation is deposited in kidneys tending to the desquamative lesion; and, as this runs its usual course, the granulations become prominent from the destruction of the tubes around them. An extreme degree of the irregularities thus produced constitutes the tuberculated kidney.

The puckering and partial atrophy occasionally seen in kidneys otherwise not morbid, or comparatively slightly diseased, are probably in many instances the result of the obliteration of cysts.

The more remarkable changes in colour and consistence are described very fully in many parts of the preceding memoir.

On reviewing the whole of the observations, the result of which I have now laid before the public, I am induced to regard the following conclusions as especially important in relation to the pathology of renal diseases:—

1. By far the greater part of the pathological lesions of the kidney arise from, or are connected with, the exudation of oleo-albuminous granules into the interior of the tubes and epithelial cells.

2. The oleo-albuminous exudation is probably often preceded, and certainly occasionally accompanied, by vascular congestion; but when the quantity of exudation is considerable, more or less complete depletion of the vascular system invariably occurs. This is a secondary result of the obstruction of the *tubuli uriniferi*.

3. The oleo-albuminous exudation occurs in two chief forms; viz. *first*, Universal infiltration of the tubes throughout the organ;

and *second*, Infiltration of particular sets of tubules, the rest remaining free, or nearly so. In the latter mode arise the granulations of Bright.

4. There is no essential anatomical difference between the exudations in the kidney which are the result of chronic processes, and those which have been considered as the result of inflammation.

5. The capillary vessels of the kidney are subject to spontaneous obliteration (unaccompanied in the first instance by any visible lesion of the tubes), giving rise to the peculiar affection which I have called the *waxy degeneration*. This obliteration of the vessels is probably in all cases preceded by a stage of congestion.

6. The consequence of the waxy degeneration is thickening and varicose dilatation of the tubuli throughout the organ.

7. The tubes of the kidney are subject to contraction and obliteration, in consequence of the desquamation of their epithelium; a condition resulting in atrophy, and complete disorganization of the organ.

8. The desquamation of the epithelium occurs very frequently in all the other diseased conditions of the kidney. When sufficiently long-continued and extensive, it produces contraction, and this indifferently whether exudation be present or not. It is sometimes accompanied by vascular congestion in every stage of its progress.

9. The earlier stages of the exudations can only be discovered by means of the microscope. The progress of the waxy degeneration, on the contrary, is best traced by the unaided eye. The desquamation of the epithelium is only to be discovered with certainty by means of the microscope, and is particularly apt to escape attention, under all circumstances, if the *kidney* only, and not the *urine*, be looked to. It results that careful investigation, both by the microscope and the naked eye, both of the kidney after death and the urine during life, are indispensable to enable the pathologist to determine with exactitude the presence or absence of disease.

[I propose to publish the results of my observations on the urine, and on the pathology of Bright's disease, and other diseases of the kidney, as soon as some investigations suggested by the present series of papers are completed.]

ARTICLE IV.—*A Case of Feigned Insanity, with some farther Remarks on that Subject.* By C. LOCKHART ROBERTSON, M.D., Army Medical Staff; attached to the Royal Military Lunatic Asylum at Yarmouth; consulting Physician to the Cumberland Lunatic Asylum at Dunston Lodge.

IN a few notes on the subject of feigned insanity and its diagnosis which I recently brought to the notice of the profession,[1] I made the

[1] Notes on Feigned Insanity. The Journal of Psychological Medicine, No. II. April 1848, p. 227, et seq.

following remarks on the case I am now about to detail:—"I have at present a case under my observation, in which I suspect this variety of mental disease (*chronic mania*) has been feigned with the above object (*viz. for the purpose of obtaining a discharge from her Majesty's service*). The patient at present professes to be cured of the insanity which he stated he laboured under on admission into this asylum. At that period he presented symptoms of chronic mania, talking unconnectedly, but only when he considered himself to be observed; while he exhibited a loss of memory not reconcilable with the recent invasion of the disorder or with its extent. The threat of coercive measures sufficed to produce in a few days what he terms his cure. The existence, however, of a depression in the left frontal bone—the result of a kick from a horse—together with one or two minor symptoms of mental derangement, demand a longer period of observation[1] before any decided opinion can be expressed, and aid in the illustration of the difficulty of diagnosing feigned partial insanity." The principles which guided me in confirming my diagnosis, will probably in the first instance be best illustrated by a detailed history of the case.

A CASE IN WHICH THE SYMPTOMS OF CHRONIC MANIA WERE SIMULATED, APPARENTLY WITH THE VIEW OF OBTAINING A DISCHARGE FROM HER MAJESTY'S SERVICE.

Pte. ———; her Majesty's —— Regiment; aged twenty-two; eleven months since, was admitted into the Military Lunatic Asylum on the 26th November 1847.

From his history, it would appear that, in June 1847, on being discharged from the Regimental Hospital at ———, instead of joining his depot there, he embarked in a steamer for ———, then the headquarters of the regiment. On his arrival there he was taken to the guard-room, doubts being entertained of his sanity; and was subsequently transferred to the Regimental Hospital at that station.

On the 24th of June he made his escape in the Hospital dress,[2] in the which, however, he was that same day retaken. The reason he assigned for having taken this step was, to "escape from a man who was going to knock his brains out with a bar of iron." His conversation at this period was rational, and he talked a good deal about his own affairs, but obstinately refused to obey any order. He complained much of headach, but there was no symptom of any physical disorder; he slept and ate well, &c. He continued in the Hospital until the 14th of September, refusing to go out of doors for exercise, assigning as a reason, "that he was sure of being attacked by the stranger, who was waiting outside to knock his brains out."

On the morning of the 14th September he went into the ward adjoining that in which he was lodged, in which there was but one patient whose regimentals had not been removed on his admission that morning, as is the practice. The ward was at that moment empty. He immediately dressed himself in the regimentals, slipped out of the Hospital, and passed the sentry at the gate, holding a handkerchief to his jaw, as if he had just been having a tooth extracted. Half an hour elapsed before he was missed, and no tidings were heard of him until he was taken by the civil powers as a deserter, and lodged in the jail of ———, from whence, in consequence of his violence and presumed insanity, he was removed to this establishment.

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[1] This essay, although only published in April, was written in February.

[2] A blue flannel dressing-gown, trowsers, and waistcoat.

On admission, his manner was calm and collected. His bodily health appeared to have been injured by confinement. On the right side of the os frontis a depression was observed, which he stated he had received by a fall from a horse some years previous to enlistment.

His conversation on parade the morning after his admission was very unconnected. He insisted that he was in a jail, and "that the other patients were either drunk or pretending to be drunk, but not mad," &c. &c. Doubts being, from the previous history of his case, entertained of his insanity, I told him that, if he again repeated such absurdities, he should have a cold shower bath, and be secluded for the rest of the day. *He never again spoke in this incoherent manner during the entire period of his residence here.* He obeyed every order that was given to him, and invariably conducted himself in a most soldierlike manner.

On being questioned as to his former conduct, his desertion, &c., *he invariably denied all knowledge of the matter; of his ever having been in a civil jail, &c.* He likewise professed to have forgot the circumstances attending his arrival here, and exhibited an incoherence of thought and loss of memory in relation to these matters, inconsistent with the recent origin and slight symptoms of his supposed mental disease, as also with the otherwise perfect soundness of mind, which, from the period of his admission, he invariably presented on examination, excepting on the morning after his arrival, as already stated.

He continued apparently of sound mind, satisfied with his position, and employed daily with the working party until the 5th of February, when he addressed the following letter to his uncle:—

LETTER I.[1]—*Yarmouth, February* 5, 1848.—MY DEAR UNCLE,—I take the opportunity of writing you this letter, in hopes to find you and your wife enjoying good health, and all friends, as I am at present, I thank the Almighty God.[2] Dear uncle, I daresay you will be very much surprised, if not before now, by hearing of me being a patient in the military madhouse; but I thank God I am quite recovered this last month or two. I have been labouring under this dreadful malady this last eight or nine months; but I shall ever return the Almighty God thanks that I am quite recovered at present, and I hope I will continue so, which I have no doubt, with God's help, that I will. Dear uncle, I am permitted by the gentlemen of this establishment to write this letter to you, in hopes that you will send for me. *I expected a short time after my recovery to be sent back to my depot, but it appears quite the reverse, according to what I hear.* They will never entrust me nor any other man with fire-arms, for fear of a return of that dreadful malady; but they are glad to get shot of them as soon as they recover. The government would not give twopence for me, nor a ship-load like me.

First, write a letter to the Secretary at War, and another to the gentlemen of this establishment, stating to those gentlemen that I am your nephew, and if they will be so kind as to send me to you, that you will hold yourself responsible; if I am not able to take care of myself, that you will see me taken care of. [*These are exactly the steps which relatives require to adopt who wish to remove convalescent or harmless patients from the establishment.*]

My dear uncle, let this not frighten you. You may depend upon it; if I thought that I would be a burden on you, or any of my relations in particular, I would deny that I had any, and then I would be sent to the parish I was born in, when the gentlemen of the establishment would think proper. I suffer very much by times with the original complaint, that pain in the head. You see that insanity has rendered me unfit for the army, and I am sorry of it; but welcome be the will of God. There are officers, non-commissioned officers, and privates, patients in this establishment. Some of them are here

[1] This and the two following letters are altered as regards the orthography; otherwise they are printed verbatim as written.

[2] The patient is an Irishman.

these number of years, and never get properly well, and the gentlemen doctors would not send them away under any consideration. They are very comfortably situated here, and so am I too; but, as a matter of course, I would much rather be out of it. I am no good for Queen or country here. When I am discharged, I will look out for some humble situation. I never will work at my trade any more, for fear of that other complaint returning. I don't forget my poor mother. On the 7th of this month she will be dead twelve months. That will be next Monday. I hope she is better off than to be in the world, poor woman. Dear uncle, my feelings have been overcome writing these few lines. Death is a debt we all must pay, and we do not know the moment. I will conclude at present. Your affectionate nephew.

On reading this letter, I informed him that he was entirely mistaken as to the facility with which he appeared to think he could be discharged from the service; and I assured him, that as soon as Dr Sillery considered him recovered, he would be reported as fit for duty and sent to the headquarters of his regiment. With this prospect he expressed himself extremely satisfied.

He continued apparently in a sound state of mental and bodily health, daily employed with the working party until the 8th of March, when he handed in the following letter, addressed to the medical officers of this establishment.

LETTER II.—On second recollection, Dr Sillery and Dr Robertson, gentlemen, I write those few lines to inform you both, and you both may inform any person or persons you think proper, I, lance-corporal ———, —— regiment, it is quite evident since my recovery that I have both wished and asked to be sent back to my depot, and now my mind is quite to the reverse. I am certain sure I have not the least idea whether I will be sent back or not; but if I am to go back to my depot, I would certainly not wish to do so on any account. I will now state my reason, *that if I got a return of that dreadful malady, I might perhaps shoot myself or run the bayonet through my carcase, and then there would be no more of me; but then, on the other hand, it would be as likely I would do so to some of my comrade soldiers or officers, and that I would be made a prisoner of directly. But they would soon find out that I was once the patient in the lunatic asylum, and then what would they do or say but that I got a return of my former complaint?* I would be sent to hospital directly, where, I suppose, my head would be shaved and blistered. Do not think for a moment, gentlemen, that I have any particular wish for my discharge. Quite to the contrary. I have not; but I would prefer it before I would go back to my depot. But, whether I go back or not, I would certainly wish to go out of this. I thank the Almighty God I am better fit to be an orderly to take care of the patients, than to be a patient and the orderlies to take care of me. I hope, gentlemen, that you will take it into consideration, and not delay me more than another week or so, whether I am to be discharged or sent back to my depot.—I am, your most obedient servant, ————.

From the date of the above letter (March 8th) up to the 10th of May, he continued in good health, apparently in every way of sound mind, and daily employed on the grounds of the establishment. On that day, he handed in the following letter, addressed to the Right Honourable the Secretary at War.

LETTER III.—Sir, this is the humble petition of ———, —— regiment, to you the Right Honourable the Secretary at War. I have taken it on myself to make known to you my grievances, as follows:—I was sent here last November, then labouring under insanity, but it pleased the Almighty God I was not long here when I was restored to my senses. As my former complaint renders me unfit for further service, I hope my body will not be detained much longer. I can assure you, Sir, it is most heart-rending to a man like me, that is in his perfect senses, to be deprived of his liberty. Sir, I am very well aware there could not be a more comfortable dwelling for men that is not capable of taking care of themselves than this establishment is. I hope, Sir, you will take

it into consideration, and not keep me much longer in bondage.—Your humble obedient servant, ————.

On the 13th of May, while working in the front square, he attempted to make his escape, but was immediately observed and brought back. On being reprimanded next morning, he stated to me that he was in his perfect senses, and ought not to be any longer detained in an hospital for the insane. I told him that, in my opinion, there never had been much the matter with him, and that I considered he had assumed the symptoms of mental disease with a view of obtaining his discharge. He said, "That if he had not been insane, the medical officer at the jail ought not to have sent him to a madhouse, and that I would find my view of his case difficult to prove."

He was ordered again to join the working party.

On the morning of the 15th of the same month, he was found to have escaped during the night, and, notwithstanding the most diligent search, up to the present date (July 1), no tidings have been heard of him.[1]

The gradual development which the diagnosis of this case underwent under careful and continued observation, confirms the value of the statement, that "in cases of difficulty, confinement with observation in an asylum for the insane, is our last resource, and cannot fail in the course of time in proving successful."[2] The doubts as to the existence of mental disease, which the incoherence of thought and loss of memory *on one subject alone* raised, were more and more confirmed by the patient's own letters and conduct, until the proof of his malingering became so clear, that he himself could not deny the truth of the inference.

Again, the influence the emotions of fear and of hope exerted over him prove the value of the diagnostic—I, on a former occasion, laid down, "that as such persons (those labouring under chronic mania) are insensible to the operations of the passions of hope, fear, anger, &c., the emotions of these may, in those feigning this disorder, perhaps be produced."[3] The threat of a shower bath and seclusion removed for good all symptoms of incoherence of thought in general conversation; the assurance, that on recovery he would be sent back to his duty and not discharged the service, as he in Letter I. anticipated, led to the writing of Letter II., which, in the hint of future violence, and in the statement, that if he were sent back to duty he might "shoot or run the bayonet through some of his comrade soldiers or officers," &c. &c., contained farther confirmation of the feigned character of his disease, inasmuch as they evinced in the patient's mind a sensibility to the emotions of hope, fear, and disap-

[1] The manner in which the patient effected his escape displayed great forethought and ingenuity. The details would, however, hardly be appreciated by those unacquainted with the building and locality.

[2] Notes on feigned insanity, already quoted.

[3] Shakspeare, who evidently must have studied insanity from nature, notices this in that beautiful delineation of feigned dementia or chronic mania, in the character of Edgar :—

"My tears begin to take his part too much,
They'll mar my counterfeiting."—*King Lear.*

pointment, which, had the disorder he assumed been real, would not have existed.

Lastly, all doubts as to the nature of the case were entirely removed by his assertion on the 13th of May, when told that he was feigning insanity, "that it would be difficult to prove it, and that the medical officer of the jail ought not in that case to have sent him to a mad-house;" while his subsequent escape, and its attendant precautions, confirmed more decidedly than was agreeable the fact, that the patient had simulated mental disease with the view of obtaining his discharge from her Majesty's service.

In conclusion, I think that the following remarks—the only observations on this subject which I have met with in our systematic treatises on insanity, and which I had not seen when I published the "Notes on Feigned Insanity" I have in this paper more than once alluded to—may be interesting in their relation to the case under consideration, and serve also as a sample of German psychology:—

"The detection of simulated (feigned) psychopathies," says Baron Feuchtersleben,[1] "is likewise attended with great difficulties. The two objective somatically characteristic symptoms which Friedrich[2] mentions as criteria of the psychoses, namely, the specific smell and the peculiar physiognomy, are by no means always present. The physiognomy may, moreover, be counterfeited so as to deceive. Even the sleeplessness, generally (but not always) connected with psychopathies, may be assumed (though hardly beyond a certain point). The reluctance to look you in the face is not to be depended upon, because it also occurs in lunatics.[3] In all cases a knowledge of the object of the supposed simulation is of much importance, because it points out a motive through which (after many a contrivance to be invented according to the individual case) the mind of the supposed simulator may be acted upon. Moria and idiocy are more frequently, mania more rarely simulated. In pretended monomania the existence or non-existence of hereditary disposition, of physical symptoms, and of other (ethical) motives, for the imputed crime, in connexion with a comparison of the deed with the character of the individual, which must be inquired into in all attainable particulars, may serve as guides to the decision of the physician; wherein it is true, as in every decision, no small scope must be left to the judgment for a free exercise of its powers. Hypocrites in general adopt conduct diametrically opposed to their natural character; the cunning pretend to be stupid; the cheerful, melancholy; the gentle, furious,[4] &c. Let the physician, therefore, set out for this point; let him accurately examine the harmony or want of harmony in the psychical and physical qualities of the individual; let him inquire long, surprise, deceive, tire the feigner; let him employ disagreeable psychical and physical means or alleged remedies, with women those which may disfigure them, and in the most obscure cases let him wait, with an indifference which is beyond measure painful to the simulator, the effects of time, the solver and elucidator

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[1] The Principles of Medical Psychology, being the Outlines of a Course of Lectures, &c. Translated from the German, by the late H. Evans Lloyd, Esq., and revised and edited by B. G. Babington, M.D. *For the Sydenham Society.* Pp. 376 and 377. London: 1847.

[2] System der Gerichtlichen Psychologie, p. 162.

[3] Wagner Ger. Arzneik. Vol. II. p. 158.

[4] Sinogwitz Geistes Störungen, p. 468.

of all things. It must not be forgotten, however, that men have really become mad after having been long confined on suspicion of pretending to be so, just as hysterical women often really fall into a state which they have long affected."

Part Second.

REVIEWS.

Traité de l'Art de Formuler, ou Notions de Pharmacologie appliquée à la Médecine. Par M. MIALHE, M.D., Pharmacien, Professeur agrégé à la Faculté de Médecine. Paris: 1845. 8vo. Pp. 518.

A Treatise on the Art of Prescribing. By M. MIALHE, M.D., &c. Paris: 1845.—(Continued from p. 43.)

IN a former article upon this treatise, after a brief survey of the author's views on the general absorption of medicines, we shortly adverted, in detail, to two important drugs which are discussed by him, namely, sulphur and iron. The subjects of antimony, silver, and mercury, will now engage our attention.

And first, as to *antimony*. As this metal undergoes oxidation when exposed to the combined influence of air and moisture, there is no difficulty in understanding its action when taken into the stomach in the metallic form. Further, as the oxide thus generated is, according to our author, in the state of hydrate, it is dissolved in a feebly acid menstruum, as the gastric juice, much more readily than the ordinary sesqui-oxide of antimony, more or less of which is probably anhydrous. Having digested equal parts of the pure metal in a state of fine division, and of the sesqui-oxide in separate portions of water very feebly acidulated, Mialhe ascertained that the quantity of antimonial salt produced with the metal was much greater than with the oxide, from which he infers that the activity of an antimonial preparation, of which the oxide forms the base, will be much increased when the latter is in the state of hydrate. It is not mentioned how the oxide used in this experiment was prepared. Gmelin (*Handbuch der Chemie*, 1844) says, that the sesqui-oxide of antimony does not appear to form a hydrate, but, as Berzelius has shown, it is to a slight extent soluble in water. The sesqui-oxide of antimony belongs to a small class of drugs which are soluble alike in the acids of the gastric juice as in the alkalis of the intestinal canal, comporting itself as a base in the one case, and as an acid in the other; but for easy solution in feebly acid or alkaline fluids like the gastric and intestinal secretions, Mialhe affirms that it must be hydrated.

Of late years in France, the kermes mineral has again come into vogue as an antimonial preparation, and, owing to the recommendations of Clusel, Trousseau, and others, has with many superseded the use of tartar-emetic. It is said that, while in its general therapeutic action it is quite equal to the double tartrate, it is much less apt to excite vomiting, local irritation, or inflammation. This compound seems worthy of a further trial in this country; but the circumstance to which we wish to direct attention here, is the great difference in its strength, according as it is obtained by the moist method of Clusel (officinal in the Parisian codex), when it is most active, or by the dry method of Thierry, when it is often nearly inert. Our author has ascertained, by experiment, that the officinal kermes contains a much larger proportion than the other of the hydrated oxide. May not the very irregular action of the sesquioxide itself, observed in the trials made with it by Professor Christison and Dr Clark, be owing to its containing a variable proportion of hydrated oxide?

The following explanation is offered by Mialhe, of the remarkable difference in the physiological effects of the same doses of tartar-emetic in health and disease. In the healthy stomach the tartrate is, in part, immediately decomposed by the free muriatic acid of the gastric juice, and a highly acrid compound, the hydro-chlorate of the chloride of antimony, is formed. Hence the nausea, vomiting, and diarrhœa. In the course of an acute disease, as pneumonia or bronchitis, in consequence of the loss of appetite and abstinence from food, the gastric juice becomes very feebly acid, and frequently indeed presents no acid reaction whatever. There is little or no formation of the acrid chloride, while the passage of all the tartrate into the circulation, fully accounts for its powerfully sedative or contro-stimulant action under these circumstances. Mialhe endeavours to account for the well-known fact, that vomiting which has been excited by small doses of tartar emetic, frequently disappears when the dose is increased, by supposing that an amount of hydro-chloric acid in the gastric juice, capable of decomposing a quarter or half a grain of the medicine, may be insufficient to displace the tartaric acid when the quantity is larger. To these views it may be objected, that as the emetic action of antimony is obtained by injecting it into the veins or rectum, the vomiting should be regarded as the effect rather of a special modification of the nervous system, than of a local irritation of the mucous membrane of the stomach; and a still stronger objection is found in the results of several experiments in which we administered, along with one grain of tartar emetic, thirty grains of calcined magnesia or prepared chalk, without interfering in any degree with its ordinary emetic action. The antacid was not given in one, but in three separate doses; the first at the same time with the antimony, the second a quarter of an hour afterwards, and the third in half an hour.

While we reject M. Mialhe's explanation, we are not the less opposed to that view which attributes the *tolerance* to the presence of inflammatory fever. In fact, the same tolerance is established, *quite independently of acute febrile reaction*, when the patient is confined to a spare diet; and it may be established as a general rule, that the more rigid the diet, so much the more marked will be the general action of antimony; and, on the contrary, when the quantity of food is considerable, its emetic action is more decided.

Silver.—The insolubility of this metal in the gastric juice renders it inert in relation to the animal economy. The silver of the nitrate, on meeting with that fluid, is immediately thrown down as the insoluble chloride. The complete absence of any sensible effect, in the great majority of cases, during the administration of the nitrate of silver, has led many (we refer more especially to the German) physicians to deny the occurrence of its absorption in any case, and to suppose that the entire dose escapes in the form of chloride with the fæces; but the coloration of the skin, which has followed its use, though only in a few instances, places the fact of its absorption, at least in these cases, beyond a doubt; and this is made intelligible by Mialhe, who affirms that the chloride is soluble to a slight extent in a solution of common salt or sal ammoniac, both of which are present in the gastric juice. The chloride of silver, freshly precipitated in the stomach, is in a more favourable condition for solution than the same preparation in the dry form; hence the superiority of the nitrate of silver to the dry chloride as a therapeutic agent. But how is the rare occurrence of the coloration of the skin to be explained, unless perhaps we suppose, that in those unfortunate individuals in whom it has taken place, the fluids were, from accidental circumstances at the time of administration, unusually saline? As to the oxide, there is little doubt that it also is converted into chloride in the stomach, in which case it must be equally liable with the nitrate to produce the bluish-grey tint of the integuments. The following formula is constructed in accordance with the views above mentioned:—

Nitrate of Silver Pills.

Nitrate of Silver,	15 Grains.
Chloride of Sodium,	60 „
Starch,	45 „
Gum Arabic,	15 „
Water,	q. s.

Pulverize the nitrate of silver; then add the water, and afterwards the salt; lastly, the starch and gum. Divide into 100 pills; each pill contains a little more than the sixth of a grain.

As the quantity of chloride which is rendered active, even under the most favourable circumstances, is very small, it is evident that, when silver is exhibited to procure its *remote effects on the nervous sys-*

tem, a preparation which is at the same time soluble, and incapable of decomposition in the gastric juice, is much wanted. We hope to have this desideratum supplied by a solution of the chloride in the hypo-sulphite of soda. This preparation has a saccharo-metallic taste, and a dose of it, containing an amount of silver equivalent to two grains of the nitrate, is easily borne by the stomach. As it is not decomposed by *diluted* muriatic acid, or solutions of the phosphate of soda, muriate of soda, and muriate of ammonia, there is reason to believe that it is not acted upon by the gastric juice, but passes directly into the circulation. The mode of preparing this solution, together with its physiological and therapeutic action, we shall notice on a future occasion.

We shall close our remarks with an analysis of the author's observations on *mercury* and its compounds.

M. Mialhe has demonstrated by an extensive series of experiments, that all the preparations of this metal used in medicine, in reacting with a solution of an alkaline chloride, with or without the presence of atmospheric air, produce a certain quantity of corrosive sublimate. The amount of bi-chloride thus formed differs considerably, according to the nature of the mercurial preparation employed. The bin-oxide of mercury, the greater part of the binary compounds which correspond to it in composition, and in general all the per-salts of mercury, in contact with an alkaline chloride, give by double decomposition bi-chloride of mercury and a new alkaline salt. The protoxide, and the greater part of the binary compounds which correspond to it in composition, are converted first into calomel, and it is only by a subsequent reaction that a very small proportion of corrosive sublimate is formed. These reactions take place at ordinary temperatures, but are much promoted by a heat equal to that of the human body. Some are affected immediately, and the greater number do not require more than a few hours.

For the reader's convenience, we have thrown into the tabular form the results obtained in experiments with the more important mercurial compounds. In each experiment, six decigrammes of the mercurial preparation were placed in contact with an alkaline liquor having the following composition:—common salt, sal-ammoniac, of each, six decig.; distilled water, ten grammes. The operation was continued in all the experiments for twenty-four hours.

The process adopted by Mialhe to estimate the quantity of bi-chloride formed, is, we believe, quite exact. It is a modification of that recommended by Gay-Lussac.

MERCURIAL PREPARATION EMPLOYED.	QUANTITY OF CORROSIVE SUBLIMATE FORMED.	
	At the Ordinary Temperature, from 15° to 20° Centig.	At a Temperature of om 40° to 50° Centig.
Calomel (prepared by sublimation.)	6 milligrammes[1]	15 milligrammes.
Proto-bromide,	6 ,,	15 ,,
Protiodide,	5 ,,	6 ,,
Protoxide,	11 ,,	19 ,,
Proto-nitrate,	4 ,,	13 ,,
Proto-sulphate,	7 ,,	14 ,,
Proto-acetate,	8 ,,	11 ,,
Proto-tartrate,	4 ,,	8 ,,
Ammonio-chloride,	82 ,,	180 ,,
Bin-oxide,	47 ,,	154 ,,
Bin-iodide,	110 ,,	193 ,,
Bi-cyanide, Per-nitrate, Per-sulphate,	Entirely converted into bi-chloride.	
Turbith Mineral,	112 ,,	228 ,,
Bi-tartrate,	312 ,,	362 ,,
Metallic Mercury,	4 ,,	7 ,,
Per-sulphuret, Black Sulphuret,	Are not acted upon when pure.	

So long ago as 1763 Capelle affirmed, that calomel, when mixed with muriate of ammonia in solution, undergoes some important change; but as to the nature of the chemical reaction thus induced, chemists were by no means agreed. Some maintained that the proto-chloride was merely held in solution; others, that a portion of it was converted into corrosive sublimate and metallic mercury, the truth of which view Mialhe has been the first to demonstrate by experiment.—(P. 10, *et seq.*) When the experiment is performed with distilled water, and the atmosphere is excluded, the change is correctly represented by the following formula:—

$$Hg\ ^{2}Cl\ ^{2} = Hg\ Cl^{2} + Hg$$

But the reaction is more complex when the fluid is exposed to the atmosphere, in which case the quantity of sublimate produced is much greater. Under these circumstances, the calomel absorbs a certain quantity of oxygen, one equivalent of bi-chloride being produced for every two equivalents of oxygen absorbed; further, each equivalent of the bin-oxide of mercury thus formed gives, by double decomposition with the chloride of sodium or ammonium in solution, one equivalent of sublimate and two of soda or ammonia.

In the case of calomel, Mialhe has performed a number of ex-

[1] 100 milligs. = 1 decigramme.
1 decig. = 1·54 gr. English Troy.

periments to determine the effect of circumstances in modifying the amount of bi-chloride formed. He has thus shown:—

1. That when calomel, prepared by precipitation, is employed, the quantity of bi-chloride produced is greater than from the same drug obtained by way of sublimation.

2. That the quantity of bi-chloride produced bears no proportion to the amount of calomel employed; but is in exact relation to the quantity of alkaline chloride present in the solution.

3. That the quantity of sublimate produced is greatest when the alkaline solution is most concentrated.

4. That the presence of organic matter in the mixture does not prevent the reaction, and that while fatty matter retards, dextrine favours it.

5. That although the change may take place in water entirely deprived of air, yet it is promoted in a marked degree by exposure to the atmosphere, the quantity of bi-chloride formed being more than three times greater in the latter than in the former case.

The immediate agent of the physiological action of all mercurial compounds is, according to Mialhe, the corrosive sublimate formed by the agency of the gastro-intestinal fluids, which contain the muriates of soda and ammonia, accompanied or not with muriatic and other acids, which promote the reaction on the preparation employed; and, as a corollary to this proposition, he maintains that their activity is in exact relation to the facility with which they are converted into bi-chloride. However true this proposition may be in reference to the insoluble mercurials, we deny its application to all the soluble compounds. We have induced salivation by the use of *large* doses of a soluble and non-irritant salt, the tartrate of potash and mercury, without exciting the slightest irritation of the stomach or intestines; for which, and other reasons derived from the chemical properties of this salt, that we cannot now enter into, we presume that it is absorbed directly, without previous conversion into the acrid bi-chloride.

By inspecting the table which we have given above, the reader will observe, that it corresponds in its results pretty closely with clinical experience; thus, the bi-cyanide and per-sulphate of mercury, which are known to be heroic poisons, are there seen to be entirely converted into corrosive sublimate; the results obtained in the experiments with the ammonio-chloride, bin-oxide, and bin-iodide, tally satisfactorily with their well-known energy; while the small amount of bi-chloride formed from the proto-chloride, proto-bromide, and proto-acetate, accounts for the lesser activity with which these agents are endowed. At the same time, we think that the proto-salts hold here a lower place than experience has generally assigned to them. That the alkaline chlorides and muriatic acid should have no action on cinnabar, or the black sulphuret of mercury, is in exact accordance with the inert character of these compounds.

The great difference in the quantity of corrosive sublimate form-

ed from the proto-salts, when compared with the per-salts, is well shown in the iodide and bin-iodide, proto-tartrate and per-tartrate. Mialhe affirms, that the physiological action of the last two salts differs in a like degree. The French possess two empirical preparations (liquors of Pressavin and Diener,) which contain the tartrate of mercury for their active ingredient. These are very unequal in activity; the preparation which at one time displays an almost poisonous energy, at another acting as the mildest of mercurials. Mialhe has shown, that this is owing to differences in the mode of preparation, in consequence of which they contain, along with the proto-tartrate, very variable proportions of the per-tartrate.

Our author's experiments show, further, that the pure metal itself, when exposed to the action of the atmosphere and of alkaline chlorides in solution, is converted in part into corrosive sublimate; and to this fact we can have little hesitation in ascribing the therapeutic action of metallic mercury, and of the compounds in which it is present in a state of fine division. When these contain in addition the protoxide, their activity is augmented, as the table indicates; but that their action, as mercurials, is due solely to the small proportion of oxide which may be present, cannot now be maintained. The pure metal, even when given in the fluid form, is not inert. As administered generally, it passes so rapidly through the intestines, that no time is afforded for the gastro-intestinal fluids to act upon it. But if from any cause it be retained, the result is very different; as in the case of a woman recorded by Mialhe, to whom fifteen ounces of the pure metal were administered to relieve obstruction of the bowels. She lived for six days afterwards, during which period the symptoms of obstruction continued, and the mercury was retained in the body. M. Mialhe saw her on the day of her death; the pulse was then almost imperceptible, the skin was cold, and the face was emaciated and painfully contracted. She complained of acute pain in the belly, which was enormously swollen. But in addition to these symptoms, which might be due as much to the intestinal affection as to the mercurial poisoning, she was affected with a peculiar and constant trembling of the superior extremities, and of the lower jaw; the gums were violet-coloured and bleeding; the inferior incisors had all fallen out two days previously; one only of the superior incisors remained, and it was so loose that the slightest effort would have sufficed to extract it; the inferior maxillary bone was exposed in several points over the areolar processes, and the mouth exhaled a strong mercurial and fetid odour. There was little salivation. A post-mortem examination was refused.—P. 107.

This view is opposed to that of Dr Christison, who doubts whether metallic mercury, swallowed in the fluid state, has ever excited mercurial action, and who believes that all the mechanical preparations of this drug probably contain a quantity of oxide sufficient to

account for their specific effects. The researches of Mialhe, in reference to arsenic, mercury, and other metals, fully establish the correctness of the general law in physiology laid down many years ago by Dr Christison, "that metals do not act as such, but must first be converted into oxides or salts" (*Dispensatory*, 1st Ed. p. 503); but they show further, that the changes which are requisite for absorption may take place in the interior of the body.

There are many circumstances in connexion with the physiological action of calomel, which admit of explanation on this theory, which attributes its absorption to a partial conversion into corrosive sublimate; such as the equal activity, in so far as absorption is concerned, of small and large doses, the quantity of bi-chloride formed being determined by the saline strength of the secretions, and not by the amount of calomel taken; the advantage of the frequent administration of small doses, and in this manner exposing the chloride to the agency of a large quantity of the secretions. Ptyalism is said to be readily induced in seamen. Is this owing to an excess of chloride of sodium in their gastric and intestinal fluids, on account of their eating salted provisions? In cholera, and other enteric affections, we know that the blood is exhausted of its saline constituents, and that the secretions consist of little more than water. Now, placing this fact in connexion with the action of large doses of calomel in these diseases, in which, instead of exciting violent mercurialism, or "of adding to the local irritation, as might be expected from the well-known effects of that drug on the healthy bowels in less doses, they have a tendency to soothe pain, allay spasm, abate redness, and lessen excessive secretion" (*Christison's Dispensatory* p. 512), is it not probable that this remarkable suspension of the physiological action of the medicine, is dependent on the watery state of the secretions, and consequent non-production of corrosive sublimate? Unfortunately, this leaves the therapeutic action as far from comprehension as ever.

Having entered the circulation, our author supposes that the bichloride unites with the albumen of the blood; the combination thus formed, insoluble in water, is soluble when in the state of hydrate, in the solution of an alkaline chloride, as the serum of the blood. Of the existence of mercury in the blood, and its passage into the secretions, there is now no doubt. Mialhe succeeded in detecting it in solution in the urine twelve hours after taking a dose of nine grains of calomel. The quadruple compound of albumen, chlorine, mercury, and the alkali, which results, is very fluid and remarkably stable. It is not decomposed by hydro-sulphuric acid, the alkalis, or the ordinary tests of mercury, which fact, we think, offers a probable explanation of the difficulty of detecting mercury in the blood, and the general necessity for that purpose of destructive distillation. Is the formation of this compound, and its circulation with the blood throughout the body, the cause of that remarkable combination of symptoms termed mercurialism? The diminution

of the albumen effected in this way, would account for the dissolution of the blood, and loss of its plasticity, as indicated by paleness and hemorrhages, which is now generally recognised as an effect of mercurial action.

The general opinion that corrosive sublimate in combining with animal matters is reduced to calomel, is denied by Mialhe; and the results obtained in his experiments have been subsequently confirmed by MM. Chantourelle, Lassaigne, and Selmi. When, for therapeutic use, it is allied to certain organic bodies, as milk, sugar, or gum, its action is much milder, as it is thereby deprived of its irritant and caustic properties. Its absorption and general action on the system, are not the less certain, as was pointed out by Soubeiran in 1840.

There is one substance, however, contained in several pharmaceutic compositions, which has the property of converting corrosive sublimate into calomel. This is the formic acid, produced by the action of an alkali on glucose. Thus, when grape sugar is boiled with potassa, there is obtained a brown liquor, which has the property of immediately reducing corrosive sublimate to calomel, and lastly to metallic mercury. It is to the presence of this body that Mialhe attributes the diminished power of corrosive sublimate when added to certain compound syrups, and more especially to the compound syrup of sarsaparilla of the French codex, which is directed to be prepared by boiling. But it is not essential to combine the bi-chloride with organic matters to deprive it of its causticity. It is sufficient for this purpose to unite it with chloride of sodium or muriate of ammonia. After what has been said, it is unnecessary to add any thing in explanation of the *rationale* of the following formulæ :—

Solution of Corrosive Sublimate.

Distilled Water,	1000 parts.
Muriate of Ammonia, and Chloride of Sodium, of each,	2 parts.
Bi-chloride of Mercury,	1 part.

Dose, a table-spoonful three, four, or more times daily.

Pills of Corrosive Sublimate.

Corrosive Sublimate,	8 grains.
Chloride of Sodium,	30 „
Starch,	45 „
Gum Arabic,	15 „
Distilled Water,	q. s.

Divide into 48 pills; each contains one-sixth of a grain.

Ointment of Corrosive Sublimate.

Bi-chloride of Mercury,	4 parts.
Muriate of Ammonia,	8 „
Axunge,	30 „

This is an active mercurial preparation for external use, and ought to be used with circumspection.

From the foregoing outline of some of the subjects embraced in M. Mialhe's treatise, the reader will be able to form an opinion of the general character of the work. We cannot observe too much caution in accepting chemical explanations of physiological and therapeutic phenomena, more especially when coming from one who is not himself a practical physician; but although several of our author's inferences, as we have ourselves shown in one or two examples, should not be confirmed by future observations, still the facts on which they are based remain to science, and the erroneous deductions themselves will not be unproductive of good. Such inquiries are in an eminent degree suggestive; and though they may sometimes lead to error, yet, to use the words of Andral when speaking of the same subject, "L'esprit s'y arrête et y revient, comme s'il avait la conscience qu'elles le placent à un point de vue d'ou des vérités importantes vont lui apparaître."

There are portions of the work with regard to which the office of the critic might be exercised with some severity; more especially is the author open to censure when he leaves his proper science, which is that of pharmaceutical chemistry, and enters the domain of physiology. But the fault for which he is above all blameworthy, is an obvious carelessness in distinguishing facts from mere suppositions, and actual observations from loose conjectures. These and other blemishes, however, we willingly overlook in consideration of the merits of the book, which is the fruit of much patient labour and superior ability. We are no more than just, when we characterise it as forming a most valuable contribution to our knowledge of the changes that drugs undergo in the stomach and intestines—a department of therapeutics, the importance of which can hardly be overrated, but which, it is much to be regretted, is still in its infancy.

Researches on the Motion of the Juices in the Animal Body. By JUSTUS LIEBIG, M.D., Professor of Chemistry in the University of Giessen. Edited by WILLIAM GREGORY, M.D., Professor of Chemistry in the University of Edinburgh. London: 1848. Pp. 109.

THE present work belongs to a class which we are always disposed to receive with pleasure, and which we never peruse without great expectations. We have always held, and have often asserted, in the pages of this Journal, that the true mode of cultivating the science of the vital phenonema is by giving scientific precision to our ideas of the relations of these with physical science; and we are convinced that it is only by a thorough understanding of the latter, that success can be attained in the attempt to reduce the more recondite and complex vital phenomena to those simple and general expressions which we term laws.

Physiological science appears to us to be at present in a transition state. The old parties have been broken up, the new are scarcely yet formed. The vital, chemical, and mathematical schools in physiology have each had their day, and have done their work; but all of these, considered as exclusive systems, may be said, with truth, to have vanished from modern physiology. If a physiologist of the present day speaks of a theory as being too mechanical, he refers to a particular instance; he does not mean to deny in general that mechanical laws enter deeply into the explanation of the phenomena of organized bodies; nor does a chemist attempt to refer *all* physiological phenomena to the laws of his own peculiar science. On the other hand, there are now few who would hesitate about admitting, that the result of chemical and physical reasonings, as applied to physiology, has been only the more clear and distinct isolation of phenomena to all appearence as different in nature from those of dead matter, as the phenomena of chemical affinity are from those of gravitation.

In building up chemical science, it was necessary to eliminate those phenomena which could be explained by the laws of mechanics. This done, the resulting phenomena were seen to be referrible, in great part at least, to a law of affinity which produced combinations of different substances in definite proportions, and with material alterations of their original most essential properties. In building up vital science a similar method must be pursued; the laws of physical and chemical science must be applied rigidly to the investigation of the phenomena of organized life; and thus by the method of exclusion we shall arrive at more definite ideas as to the nature of the laws which are peculiar to organized existence.

In this mode of inquiry Liebig has earned one of the greatest reputations of the present age. If some of his researches have exhibited, in particular cases, a too exclusively chemical bias, this is perhaps no more than was to have been expected; and no one can deny that, in his general scientific doctrines, Liebig has at all times showed the fullest and most correct appreciation of the present condition of physiological science. It is impossible, therefore, to turn to a new work of so distinguished an observer, without having our expectations immoderately raised. We must confess, that the present work does not entirely satisfy those expectations; still, it contains the result of an extensive series of researches, some of which will probably come to be of the greatest value in science, and the entire value of which will, perhaps, not be appreciated until they have been some time before the public. In this article, we propose to give our readers an idea of some of the principal results arrived at in this work, without, in the mean time, attempting to give a *systematic* view of the researches on which these results are founded. This attempt must remain to be made at a future period, if, indeed, that be possible for any one, which the author himself has not even attempted.

The title of this work is scarcely indicative of its exact nature. The greater part of the volume is devoted to an inquiry into the physical conditions of imbibition and endosmosis, phenomena which are by no means exclusively confined to, or illustrative of, the motions of the *animal* fluids, and which have been conclusively shown to occur in unorganized as well as organized textures. The body of the work is, therefore, a purely physical inquiry. Applications to animal physiology, and an account of Hales' observations on vegetables, occupy the latter part; but the former are illustrated by very few new observations, and are, indeed, more of the nature of speculations, thrown out for future inquiry, than of positive additions to physiological science. These hints, however, imperfect as they are, appear to us to be founded on sufficiently good data, and are certainly of the greatest interest in a physiological point of view.

With respect to the physical and experimental part of the inquiry, we find little which is not better and more philosophically illustrated in the lectures of Professor Matteucci of Pisa, delivered in 1844, and now so well known in this country; with which, however, Professor Liebig seems not to have been acquainted, as the name of their author is not, so far as we have observed, once mentioned in the present volume. This is the more to be regretted, as we cannot help thinking, that an acquaintance with the views and experiments of the Italian professor would have caused a considerable modification in those contained in the volume before us. They would, at least, have had the effect of stimulating the acute mind of Liebig to accomplish more than, as it appears to us, has been done in the present instance. We shall probably seize an early opportunity of bringing before our readers some of the recent contributions to this department of physical science. At present, we shall confine our remarks, for the most part, to a theory, which it appears to be one of the main objects of this series of researches to support, and in which Liebig decidedly differs from all other writers on the subject.

The general tendency of this work is, to claim for chemical attraction, or affinity, a much larger share than has been hitherto conceded to it in the production of the phenomena of solution, endosmose, imbibition, and even capillary attraction. In accordance with this view, the term "*chemical* attraction" undergoes, in Professor Liebig's hands, an extension of signification, of which it is necessary that our readers should be aware. Not only is a simple solution, or the intermixture of two liquids, such as alcohol and water, arbitrarily referred to the law of chemical affinity; but the simple circumstance of adhesion of the particles of a liquid to those of a solid, when the latter is moistened, is owing, according to Liebig, to an affinity between the particles of each. He admits, indeed, in the beginning of the work (p. 4), that "the penetration of a fluid into the pores of a porous body, is the result of capillary attraction;" but, throughout the work, he speaks of imbibition, and even moistening of bodies, as mainly the result of chemical attraction. Indeed, in p. 27, he boldly

asserts that chemical affinity is manifested *every where*, when two dissimilar bodies come into contact:—"Although we are accustomed to limit the notion of affinity to such cases as exhibit a change perceptible to our senses, in the properties of the substances employed, as, for example, when sulphuric acid and lime, or sulphuric acid and mercury, combine together, this limitation arises from the imperfect apprehension of the essence of a natural force."

According to Liebig, the solution of a salt in water is an example of chemical affinity; the attraction of the atoms of salt for the water, being in this case stronger than their attraction for each other. In like manner, the precipitation of the salt from the water by the addition of alcohol, is likewise an example of chemical affinity, in which the former attraction is overcome by a yet stronger one, that of the alcohol for the water. Now if, instead of adding alcohol, and thus precipitating the salt from its solution, we place that solution in contact with a piece of dried animal membrane (or indeed almost any body capable of imbibing it), the salt is in this case also precipitated, provided the solution be sufficiently strong; a result which, according to Liebig, springs from exactly the same cause as in the former instance,—viz. that the affinity of the salt for the water has been overcome by a stronger affinity, that of the membrane for the water; and, as the membrane has little or no affinity for the particles of the salt, the latter is partially separated in the crystalline form.

Thus, according to this definition, a change in the properties of the substances (which has hitherto been identified with the very nature of chemical phenomena), is by no means necessary as a manifestation of affinity. Neither is combination a necessary part of Liebig's idea of affinity; combination being only one result, which occurs when the attraction is stronger than all the obstacles opposed to its manifestation, and not otherwise.—(P. 27). Mere adhesion of particles, on the contrary, if they be particles of dissimilar bodies, such as water and membrane, water and clay, even water and glass, when the latter is moistened, is accepted as a proof that chemical affinity has been brought into play.

Now, we must say that this arbitrary alteration of verbal meanings appears to us a most illogical, and (where supported by so great a name) a most dangerous way of dealing with a scientific question. There is not, in the whole of the present volume, the shadow of a proof that the phenomena of imbibition and moistening of porous or other substances are due to any other force than that which has hitherto been well known under the names of attraction of cohesion, molecular or capillary attraction. There is not the slightest proof that the moistening of a porous body is in nature different from the moistening of any other body, or that the affinity which exists between the particles of water and glass, water and membrane, water and alcohol, is in any way different from that which undoubtedly exists between the particles of water and water, alcohol and alcohol, glass and glass, &c. Yet these last have been always admitted to

be the most undoubted instances of molecular attraction, a mechanical, not a chemical force.

That molecular attraction varies infinitely in amount, both between different substances, and in the same substance under different external conditions, is perfectly well known. That a porous substance, therefore, should absorb more of a fluid for which its attraction is great than of one for which its attraction is small, is not new either in theory or experiment. That water, by its attraction for salt, should reduce the particles of the latter to a molecular form, and diffuse them intimately through its substance, is another phenomenon of the same kind. That alcohol, by its greater attraction for the water, should remove the atoms of salt from their molecular condition, and resign them to their attraction for each other, is again due to a molecular attraction of the water for the alcohol, stronger than that of the water for the salt. Liebig has indeed admitted that capillary attraction is present in these cases; the only question is as to the presence of chemical affinity.

Now we maintain, that if molecular or capillary attraction, in the ordinary sense of the term, be sufficient to account for the phenomenon, it is unphilosophical to suppose chemical attraction superadded to it. If, on the other hand, it be asserted that capillary attraction and chemical affinity are in reality one and the same force, we reply that it may be so, but we do not find in the present work any additional evidence on the subject. The analogies between chemical affinity and molecular attraction must, we think, have struck every one, as well as the arguments which have been urged in favour of the identity of the latter force with gravitation. Nay, we are by no means insensible to the proofs which have lately been adduced, that the whole of the physical forces are essentially modifications of one great force acting universally throughout nature; but the evidence of such propositions as these must be drawn from extended observation, and new experiments upon these forces in every possible mode of their action, and not from a few crude analogies, which have been a hundred times observed. In the mean time it is customary and right for scientific purposes to distinguish between molecular and chemical attraction. Thus copper and silver may be intimately mixed, forming an alloy. This is molecular attraction; the combination takes place in all proportions indifferently, and the properties of the two substances remain the same as before, except in so far as altered by the physical law of mixture. Again, copper and silver each combine with oxygen. This is true chemical affinity, with combination in definite proportions, and is manifested by such an alteration in the properties of the compound as cannot be accounted for on simple physical laws. While we hold, therefore, as we do, the provinces of these separate classes of phenomena to be, in the present state of science, sufficiently well defined, we cannot but think it extremely unadvisable to adopt the terms and arguments of Liebig in the present work; for not only have they a tendency to confuse and

unsettle the mind of the inadvertent reader, but they appear to us to have led the author himself in many instances to a false application of the results of his own experiments.

We have already indicated the part of this work which relates to the animal organism, as containing some valuable speculations well and clearly stated. The conclusion at which Liebig arrives in this part of the work is, that, according to the laws of endosmosis and exosmosis, the cutaneous transpiration and respiration must exercise a more considerable influence than has been generally admitted, upon the motion of the fluids in the vessels; that these fluids, indeed, must be drawn towards the skin and the lung with considerable force to supply the loss of fluid in these situations. That the pulmonary and cutaneous exhalations are conformable to the physical laws of endosmosis and exosmosis, and are probably very much promoted by these forces, there can be no doubt. The experiment of Liebig, illustrating this point, is very ingenious; it is, however, nearly the same with one performed by Professor Leslie of Edinburgh, in connexion with a different subject. If a bent tube, widened at one extremity, and closed at this end by a piece of moist bladder, be filled with water, and the narrow end be then placed in a vessel of mercury, evaporation will take place from the surface of the membrane, and the volume of the water within will be diminished. At the same time, air will pass slowly into the tube, and partially replace the water. Nevertheless, it will be found that a pressure from within outwards is generated, sufficient to raise a column of mercury in the smaller end of the tube from 12 to 24 inches, according to the thickness of the membrane. This pressure, of course, is generated according to the law of exosmosis.

Leslie's experiment was nearly identical with that of Liebig, the only difference being, that the evaporating surface was formed by a thin ball of porous earthenware to which a glass tube was adapted. This instrument was filled with water, and inverted in mercury, as in Liebig's experiment; the same results were produced, thus showing what Matteucci's experiments so clearly demonstrate, that exosmosis is a purely physical phenomenon.[1]

In the animal body a similar process goes on. There is a perpetual evaporation from the surfaces of the skin and lungs, which tends to diminish the volume of the fluids; and there is likewise an absorption of gases from the atmosphere into the blood. In so far as this process, therefore, follows a physical law, it is perfectly clear that there must be a constant pressure exerted towards the surface.

Liebig has not investigated the relations of the skin in animals which live in water, to the physical processes of endosmosis and

[1] This instrument was used by Leslie for determining the rapidity of exhalation from a porous surface.—See *Encyclopædia Britannica*, article *Atmometer*, and Leslie's *Short Account of Experiments and Instruments depending on the Relations of Air to Heat and Moisture.*

exosmosis. In relation to this branch of the subject, Matteucci discovered the remarkable fact, that, in the skins of most amphibious animals, endosmosis takes place much more actively from the internal to the external surface, than in the contrary direction. The application of this fact to physiology is obvious. Some differences in this respect also exist in nearly all animal membranes, the transudation being found to differ in rapidity, according as the one side or the other is placed in contact with the denser liquid. The ignorance of this fact by Liebig is an unfortunate circumstance, as it may have unconsciously interfered with the results of some of his experiments.

The influence of these physical circumstances upon pathological processes, is certainly worthy of attention. It is clear, that the physical effect of diminished transpiration, must be diminished motion of the fluids towards the surface. This circumstance, in Liebig's opinion, may be explanatory of many diseases, and is an obvious cause for the relation of diseases to the hygrometric state of the atmosphere. Not only is this the case in animals, but in plants. The blight of hops and potatoes may probably be owing, like the influenza in the human subject, to suppressed transpiration. A considerable space is given to the observations of Hales in reference to the hop-blight, and the motion of the fluids in plants.

We refrain from pursuing this subject further at present. These speculations seem to be sufficiently important; but, in the crude theoretical form in which they are here presented, it would be impossible to make them interesting to the majority of the profession. They are undoubtedly capable of being submitted to the test of direct experiment. Indeed, the researches of a recent observer (Fourcault) have been directed to the careful experimental investigation of the effects of suspended transpiration. We shall probably soon be able to lay these results before the readers of this Journal.

Recent Advances in the Physiology of Motion, the Senses, Generation, and Development. By WILLIAM BALY, M.D., F.R.S., Physician to Millbank Prison, and Lecturer on Forensic Medicine at St Bartholomew's Hospital; and WILLIAM SENHOUSE KIRKES, M.D. Being a Supplement to the Second Volume of *Müller's Physiology*. London, 1848.—Pp. 132.

THIS exceedingly careful and elaborate compilation forms a useful supplement to the second volume of Müller's Physiology, a work which, although not, perhaps, the most readable and generally useful in our language for the beginner in physiological science, must long maintain its place in English literature, as the most trustworthy and satisfactory guide for the advanced student. The same spirit of careful and impartial research, which formed the distinguishing characteristic of the original work, appears to have

guided the authors of the volume before us; the details of which are so varied and numerous, as to render it impossible for us to go into them at any length.

We hope that ere long Dr Baly will turn his attention to the subjects contained in the first volume of Müller's work. Some of these, such as Digestion and the Nervous System, require revision no less than those in the second volume. Meanwhile we have much pleasure in recommending the work of Drs Baly and Kirkes to our readers, as being at once well executed, handsome, and moderate in cost.

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVII.

MEETING XIII.—*Wednesday, July* 5, 1848.—Dr D. MACLAGAN in the Chair.

ON SEVERAL NEW GALVANIC BATTERIES. BY DR WRIGHT.

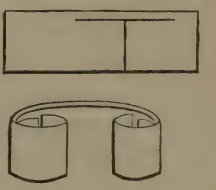
1. IN the *Monthly Journal* for July, I inserted a short notice of this new battery, as I wished to publish as soon as possible the principle on which it was founded, viz. *the inactivity of zinc in a mixture of concentrated nitric and sulphuric acids.* I now proceed to give an account of the mode of arranging the apparatus, sufficiently detailed to enable any person to construct it, as the battery is especially adapted for medical purposes, on account of its high intensity, and the facility with which it may be put together.

2. The battery essentially consists of a positive plate of zinc associated with a negative plate of the same metal covered with platinum, the former immersed in a solution of some neutral salt or caustic alkali, the latter in a mixture of nitric and sulphuric acids. The intermixture of the two liquids is prevented by the intervention of a porous cell, which may be formed of garden pot-clay, pipe-clay, or plaster of Paris. The cheapest and most convenient form of the apparatus is constructed in the following manner:—Take a common jelly jar two and a half inches in diameter, and line it with a strip of the thinnest sheet zinc two inches wide, from the edge of which a narrow strip or ear is cut, turned over the edge of the jar, and twisted to a copper wire (No. 20); within the zinc place a small porous earthenware garden-pot rendered water-tight by a plug of sealing wax, and within this again a roll of platinized zinc, to which a similar wire has been twisted. The battery is now complete; on charging it, pour a saturated solution of common salt or muriate of ammonia in the space without the porous cell and within it, and in contact with the platinized zinc the nitro-sulphuric acid.

3. The nitro-sulphuric acid consists of *nitric acid one part and sulphuric acid five parts.* In such a mixture zinc, the one of the most oxidizable metals, is as inactive as a plate of glass or porcelain; but a very slight deviation from the above proportion by the addition of nitric acid or water dissolves the charm, the metal is acted on, and the platinum is thrown off its surface, for which reason it is advisable to let the nitro-sulphuric acid have an elevation within the porous cell, higher than that of the saline solution without it.

4. I have now described the mode of constructing a single cell of the battery, which is well adapted for use in conjunction with the electro-magnetic coil machine; but when the instrument is to be applied to the decomposition of saline or other fluids, it is proper to use from four to six cells in series, the

zinc roll of one cell being in connexion with the platinized zinc of the cell next to it. In constructing a compound battery of this kind, the alternate cylinders of zinc and platinized zinc are cut from the same slip of metal by a T-shaped incision, and bent into rolls as in the figure, in which A indicates the zinc cut, and B the same slip bent into the proper form, the small cylinder being afterwards platinized nearly up to the top: the zinc and platinized cylinders at each extremity of the series are of course unconnected, and have wires twisted to them to serve as poles or conductors. I think it will be at once understood that screws or joinings are unnecessary in this battery, however extended the number of pairs in series may be.



5. The zinc is easily platinized, after having been cut and moulded into the requisite shape, by dipping it for a few seconds in a solution consisting of

Saturated solution of chloride of platinum,	ʒ ss.
Sulphuric acid, . . .	ʒ jss
Water, . . .	ʒ ij

it is then washed and quickly dried, and is found to have assumed a smoky tinge from the minutely divided platinum adhering to its surface.

6. For the administration of the galvanic shock and current, I use a small battery constructed in the following manner:—A plate of thin sheet zinc three inches by four is platinized to half its width, and cut into twelve slips, which are bent into the form of the letter U, the dark leg of which represents the platinized leg of the bent slip. Twelve of the glasses used in pewter inkstands are then filled with moistened table-salt, and a tobacco-pipe bowl, rendered water-tight by sealing-wax, is thrust into each of them, and instantly filled with the acid mixture (3), before the saline solution has time to percolate through it; the glasses are then ranged in a double line, and the zinc arcs placed in them, each having its platinized leg in the acid mixture, its unplatinized leg in the salt of the glass next in series; fine iron wires twisted round the slips at each extremity are conducted into basins of salt and water, from which the shock may be taken by immersing the hands therein. This small arrangement affords a stronger shock than a Cruikshank battery of fifty pairs of plates, accompanied with a strong sensation of heat in immersing and removing the hands from the basins.

7. Several forms of the platinized zinc battery were brought by me before the Medico-Chirurgical Society of Edinburgh, the apparatus used being a single cell (2), in conjunction with an electro-magnetic coil machine; a compound battery of four cells (4), the poles of which terminated in platinum plates immersed in dilute sulphuric acid, and the small compound battery (6). The sparks from the first of these arrangements were most brilliant, the second decomposed the water of the dilute acid with great rapidity, and the shock from the third was perfectly satisfactory to several of the members who experienced it, even after the battery had been charged more than three hours.[1]

8. In an economical point of view, this battery is much superior to any other; its intensity is four times greater than of that of Smee's battery, the arrangement now in common use, at about a fiftieth part of the cost, as it can be put together by any one having a little sheet zinc and a pair of scissors. The saturated solution of platinum is about 2s. per oz., a sixteenth part of which quantity is sufficient to platinize more than a dozen cylinders.

9. The cells should always be well washed after use, during which process a great part of the platinum is removed; it will therefore be advisable to dip the cylinders in the platinum solution before again using them.

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[1] The electro-magnetic coil machine used on this occasion is constructed on a new principle, and will be described in an early number of this Journal.

Fig. 1.

Fig. 2.

Fig. 2.

a. Sternum reflected.
b. Left Fœtal Heart.
c. Right Fœtal Heart.
d. Common Pericardium.
e.f. Common liver with suspensory ligament.
g. The Lung.
k. Common Diaphragm.
m.n. Umbilical Veins.

On Curable Headache, as regards its Pathology and Treatment. By Dr Seller.—This communication will be inserted entire in a future Number.

Singular case of Dilatation of the Ductus Communis Choledocus. By Dr Halliday Douglas.—In this case the ductus communis choledocus was so dilated as to hold half a gallon of fluid; during life it had been punctured. The case itself will be given in a future Number.

On the Exhibition of Chloroform in Dental Surgery. By F. B. Imlach, Esq. —This paper will appear in a future Number.

EDINBURGH OBSTETRIC SOCIETY.

SESSION VII.

Meeting V. May 10.—Dr Simpson, President, in the Chair.

BIRTH OF A DOUBLE MONSTER. BY DR LYELL. COMMUNICATED BY PROFESSOR SIMPSON.

Dr Simpson laid before the Society a case of the birth of a double monster, which occurred in the practice of Dr Lyell of Dundee, who had kindly forwarded the infants to him for examination. The monster belonged to Geoffroy St Hilaire's family of *Monomphaliens*, characterised by the union of two almost perfect individuals having a common umbilicus, and was of the genus *Sternopage*, in the same classification. It agreed with St Hilaire's characters of the genus in the junction or fusion of the two fœtuses, extending from immediately below the umbilicus to the apex of the sternum, (Fig. 1); in having a common pericardium (*d*, Fig. 2) containing two distinct and perfect hearts (*b c*, Fig. 2); in having four perfect lungs; but it differed from St Hilaire's generic description in this one particular, that the cavities of the pleuræ were not fused and thrown into one, but remained separated by a double serous layer, composed by the reflexion of the pleuræ. There was a single diaphragm (*k*), and a large liver (*e f*) common to both lying in the mesial line, and over which the two peritoneal coatings were reflected, one over each half, forming also its suspensory ligament in the mesial line above, and the septum between the two abdominal cavities by their union below. From the liver, at both sides of the serous dissepiment of the two peritoneal cavities, issued an umbilical vein—(*m n.*) These, with their corresponding umbilical arteries, converged to form the cord which issued single from the umbilicus, and at a length of about eight inches divided into two stems, each of about two inches long, which were inserted separately into a common and single placenta.

The history of the labour Dr Simpson laid before the Society in the following letter from Dr Lyell, received a few days after the woman's delivery:—

"On the evening of Monday the 2d November 1846, I was called about half-past eight P.M., to attend Mrs A——, in labour with her second child. She had her first child, a boy, twenty months ago; and then her labour was so quick that no doctor was sent for. On my arrival about nine o'clock, I ascertained that she had been ill about three quarters of an hour. On examination, I found the vertex presenting, the os uteri dilated to the size of a crown-piece, and very dilatable. She complained of the pains being strong; but as they were not at all expulsive, I ruptured the membranes. As this did not mend matters, I exhibited a dose of ergot, but it had no better effect. A hand was felt coming down along the side of the head. From the circumstances of the pains being so strong, and their being no appearance of the head being impacted, I began to suspect there were twins, and that the one child was, in some way, preventing the delivery of the other. I, therefore, thought it would be necessary to give her some assistance with the forceps. At twelve P.M. I proceeded to apply them, and succeeded in effecting the delivery of the head in a few minutes, but I could not make the body descend, and when I stopt pulling, the head was forcibly drawn back against the external parts. I tried pretty strong trac-

tion again, and the neck yielded as if the head had partially separated from the body. On throwing aside the bed-clothes, and examining the head, I discovered the presence of harelip and cleft palate in the child, and as it made several convulsive movements, I was anxious to effect the delivery as soon as possible, that its life might be saved. I brought down first one arm and then another, but still could not get away the body. The face looked towards the pubis. At this time I felt a third hand descending, and now there was no doubt of there being a second child. I introduced my right hand for the purpose of changing its position, but found it so wedged, lying across the brim of the pelvis, that its position could not be altered. The lower part of the body of the half-delivered child lay across the loins of the second, and was thus prevented entering the brim. I withdrew the right hand, and, after reflecting for a little, introduced the left hand along the spine of the first child to the nates, and succeeded in dragging its body into the pelvis, but all advance was again stopped. Anticipating the necessity of turning the second child, I seized and dragged its limbs along with the still undelivered limbs of the first child, and at last, after many efforts, succeeded in completing the delivery at half-past twelve P. M., the two infants revolving, as it were, upon their common sternum, around the symphysis pubis of the mother. From the first application of the forceps to the completion of delivery, from twenty to twenty-five minutes elapsed. The first child having the harelip was alive, and lived eighteen hours. The second was born dead. They were of full size (eighteen inches long), and, on the whole, well-formed. The mother made a good recovery. Mrs A—— has since been delivered of a healthy child at the full time."

The case, Dr Simpson observed, was interesting *obstetrically* from the accurate description of the mechanism of the delivery as detailed by Dr Lyell, there being few details yet on record of the mechanism of labour with double monstrosity. The practice adopted by Dr Lyell seemed the very best that could possibly have been adopted. The wonder was the frequency with which double monsters were born without almost a mother ever being lost. In most cases, the fœtuses seemed to overlap, and adapt themselves to the inequalities of each other; and, when turning or extraction by the feet was adopted, it was usually found that pulling by the feet of one body (after all the four were down) was preferable, as it allowed the one body to overlap the other, and the head of the first child, for instance, to be pulled down, and adapted to the neck of the second, thus greatly saving size and space.

The case was interesting *physiologically*, as it formed, he believed, the first recorded instance of any of the infants in a case of *Sternopage* monstrosity living beyond a few seconds after birth. In general the two hearts are too intimately fused together to allow of the survival of either of the infants after birth.—(*To be concluded in our next.*)

ELECTION OF DR HUGHES BENNETT TO THE CHAIR OF THE INSTITUTES OF MEDICINE IN THE UNIVERSITY OF EDINBURGH.

ON Thursday, July 13th, the Magistrates and Town-Council of Edinburgh met for the purpose of electing a Professor of the Institutes of Medicine in the University, vacant by the resignation of Dr Allen Thomson.

Dr Gunning and Dr Martin Barry having withdrawn as candidates, the Lord Provost rose and said,—I am happy to have it in my power to propose for this vacancy a gentleman who has shown his qualifications in no ordinary degree, by his writings and by his lectures, upon the branches of science which are to be taught in that chair. It would be a waste of time were I to refer to the many very high testimonials which he has obtained, not only from scientific men in Great Britain, but in all the great schools in the continent of Europe. We may consider ourselves exceedingly fortunate in having had a candidate so well qualified, not only to maintain, but to advance, the reputation of our University, and

I hope that this election will be unanimous. At one time I certainly was wavering in my own mind regarding the respective qualifications of Dr Barry and Dr Bennett, the gentleman I am now to propose. Dr Barry has brought forward testimonials of a very startling kind, and his own gentlemanly bearing, and his high character, were such that one might have been proud to have had such a professor in the University. However, I wish it to be distinctly understood that it was not the idea of the tests, whether they were to be put or not put, that had any weight upon my mind; for had I considered that Dr Barry was best qualified I should have proposed him, whether he would have taken the tests or not. It was my intention that we should not have mooted the question of the tests at all; and, had any gentleman come forward and made inquiry about that question, I should have stated that, at this stage of the proceedings, we had no opportunity of making any such inquiry. I do hope that candidates in future will not be deterred from offering themselves on account of these tests; because, if Dr Barry had been considered the best qualified, it would have been a great credit to himself to have had the majority of the electors on this occasion. I made up my own mind independently of this question; after having examined the various testimonials, and after making inquiry of medical gentlemen in Edinburgh, I was satisfied that it would be impossible to get any one so well qualified for that chair as Dr Bennett. It is unnecessary to take up your time upon this question: he has not only shown, both by his writings and his lectures, his thorough qualification for the situation, but we know that he is so industrious and so persevering, and his ardour and zeal are so great, that we have every reason to believe that he will go on making further discoveries in science, and will be a credit to the University, and will be the means of advancing its fame in the world. I have therefore very great pleasure in proposing, that Dr Bennett be elected to fill the vacant chair of the Institutes of Medicine.

Mr Melville had much pleasure in seconding the motion. He was thoroughly convinced that Dr Bennett would add to the reputation which the University had already acquired. They owed him a debt of gratitude for the benefits he had conferred on the academic school. He had no doubt that the fame he had there acquired would go with him to the University. With regard to the tests, this was not the time to put them. It was their duty to hold, that every person making such application will comply with the requirements which the law prescribes. He trusted that on all future occasions they would assume, that those who came forward were prepared to comply with the law.

Mr Gulland said, there could be no doubt that the mere circumstance of these tests existing had a tendency to prevent gentlemen from coming forward who would otherwise do so; and he therefore thought that they should urge government to bring in a bill for the immediate abolition of these tests.

Mr Ritchie just wished to express his admiration of Dr Bennett. He had attained his high standing in life by his own energy; he is a man who will work his way through life by his own individual talent, as he had placed himself in the sphere he now occupies by his own merit.

Dr Renton thought that justice should be done to all parties. In reference to Dr Barry, he would say that this board is fortunate in having such gentlemen to come forward to fill the chair of physiology. Dr Barry had two certificates from the highest medical authority in Great Britain; and although he did not possess the same amount of energy as Dr Bennett, he comes forward as an original thinker, a sound-headed man, one whose zeal is indefatigable, and he knew of no limitations to such a mind.

Mr Dick was satisfied that either of them would have done high honour to the University, while he considered that Dr Bennett was superior to Dr Barry. He would not have risen but from the fact, that in regard to the tests he was in the minority in the Hebrew chair question; but, had Dr Barry been elected to the chair, he would have considered it his duty to allow him to go into the University without any mention of the tests at all.

Dr Spittal said he had had various occasions of bearing testimony to the know-

ledge of Dr Bennett, and he highly congratulated the council on the admirable choice they had made. In regard to the claims of the two gentlemen alluded to, he not wish to enter into any comparison; but it was evident that Dr Bennett stood so prominently forward, as far to outstrip the claims of Dr Barry. Dr Bennett's progress and success in teaching he had watched with great interest, ever since he commenced his labours in Edinburgh. His claims as a teacher of practical medicine are of a very superior description; and as instruction in clinical medicine will form part of the duties of the future professor, he felt satisfied that Dr Bennett will revive the celebrity of the University in this very important department.

The Provost's motion was then unanimously agreed to.

Mr Clark said, that the appointment now so unanimously made, he was quite sure, could not be more gratifying for Dr Bennett to receive, than it was on the part of the patrons to bestow.

BOOKS RECEIVED.

1. A Treatise on the Practice of Medicine. By George B. Wood. M.D., &c. 2 vols. large 8vo. Philadelphia. 1847.

2. Lectures on Diseases of the Eye. By John Morgan, F.L.S., &c. Second Edition, carefully revised and enlarged with Notes, by John F. France, &c. London. 8vo. 1848.

3. A Practical Treatise on the Diseases Peculiar to Women. By Samuel Ashwell, M.D., &c. Third Edition. London. 8vo. 1848.

4. The Retrospect of Medicine. Edited by W. Braithwaite, &c. Vol. XVII. London. Small 8vo. 1848.

5. Memoranda for Young Practitioners in Midwifery. By Edward Rigby, M.D. London. 24mo. 1848.

6. The Journal of Psychological Medicine and Mental Pathology. Edited by Forbes Winslow, M.D. No. 3. London. 8vo. 1848.

7. The American Journal and Library of Dental Science. April 1848. Baltimore. 8vo. 1848

8. The Ethnological Journal. July 1848. Edited by Luke Burke, Esq. London. 8vo.

9. Eighteenth Annual Report of the Belfast District Asylum for the Insane Poor. 8vo. Belfast: 1848.

10. Hassall's Microscopic Anatomy. Part XIII.

11. Littel's Living Age. No. 201. 18th March 1848. Boston.

12. Harvey and Buchanan's new and improved Synoptical Table of the Diseases of the Human Ear, &c. 1848.

13. Zeitschrift für die Gesammte Medizin. Von Oppenheim. Band 37. Heft. 3.

14. Bibliothek for Læger, Tredie Række. af. H. Silmen. Kjobenhavn. Oktober 1847.

15. Report of the Pennsylvania Hospital for the Insane, for the Year 1847. By Thomas S. Kirkbride, M.D. Philadelphia. 1848.

16. The Nineteenth Annual Report of the Inspectors of the Eastern State Penitentiary of Pennsylvania. Philadelphia. 8vo. 1848.

17. Twenty-seventh Annual Report of the Bloomingdale Asylum for the Insane. By Pliny Earle, M.D. New York. 8vo. 1848.

18. History, Description, and Statistics of the Bloomingdale Asylum for the Insane. New York. 8vo. 1848.

19. American Journal of Dental Science. January 1848.

20. Outlines of Lectures on the Nature, Causes, and Treatment, of Insanity. By Sir Alexander Morrison, M.D., &c. Fourth Edition, with Plates. London. 8vo. 1848.

21. General Index to the British and Foreign Medical Review. Edited by John Forbes, M.D., &c. Vol. XXV. London. 8vo. 1848.

22. Practical Observations on the Administration and Effects of Chloroform, especially in its Application in Cases of Natural Labour. By J. H. Stattard, Esq. &c. London. 8vo. 1848.

23. A Plain Statement with Reference to Medical Reform. By Edwin Lee. London. 8vo. 1848. *This Pamphlet is distributed gratuitously to the profession.*

24. A Report on the recent Progress of Psychological Medicine, and Mental Pathology. By C. Lockhart Robertson, M.D., &c. Small 8vo. 1848.

We have to express our acknowledgments for several copies of the China Mail, and of other Newspapers.

ERRATA IN JUNE AND JULY NUMBERS.

In Dr Basham's evidence, June Number, p. 901, 19 lines from top, and

In the July Number, p. 62, 20 lines from bottom of page, for "absorption," read "deposition."

NOTICES TO CORRESPONDENTS.

Articles by Mr Joseph Bell and Dr Kelburne King are in type, and will appear in the next Number.

THE

MONTHLY JOURNAL

OF

MEDICAL SCIENCE.

No. XCIII. SEPTEMBER, 1848. No. 27. New Series.

Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*On the Seat of Headache in the Sympathetic Nerve, and on some of the Rules of Treatment, drawn from its connexion with Chronic Ill Health.* By William Seller, M.D., F.R.C.P.E., Extraordinary Physician to the Royal Infirmary of Edinburgh, &c.

When headache is dependent on disease of the membranes of the brain, the cranial bones, or the scalp, the pain plainly follows the usual law of pain. The cause of the pain has its seat in one or other of these several textures, and the consciousness of the sensation is referred to that seat. But when the cause of pain has its seat any where in the encephalon itself, it becomes a question what the organs are through which the consciousness of the sensation arises. Of the several parts composing the encephalon, some, and these form the largest portion, may be cut or punctured in living animals without any discoverable effects; others cannot be so treated without the occurrence of paralysis or convulsions; others again, when dealt with in the same manner, are the source of pain; but the consciousness of that pain is not referred to the parts acted on, but to the extremities of nerves originating in those parts. We should expect, then, that the effects of disease within the encephalon in man, would correspond to these results of experiments on animals. That acute inflammation, for example, should affect the greatest part of the encephalon, without giving rise to pain at all—that when the same invades a certain number of parts, paralysis or convulsion should occur without pain; and that when it seizes any of the remaining parts, pain should uniformly arise, referred, however, not to any part of the encephalon, but to parts supplied with sentient nerves from the encephalon.

No such rule, however, is observed. And if the facts recorded in the pathology of the brain be attentively considered, it will be found, I think, that headache has been remarked along with uncomplicated disease of almost every particular part of the encephalon.[1] Yet it remains a question whether the consciousness of pain be referred in these cases to the affected part. This question cannot be determined in a direct manner, because such a reference cannot be judged of with exactness in the encephalon any more than in the other organs, of the changes on which we have not been rendered conscious in the state of health. The advantage of direct observation is, therefore, unavailable in this instance.

There are, I think, only three conceivable modes of explaining the occurrence of headache in affections of the encephalon itself, consistently with the established laws of the animal economy. In one of these the assumption is made, that the reference of the consciousness of pain is not to the seat of the actual irritation, but to the extremities of the nervous filaments in the adjacent membranes or the scalp; in the two others, the reference is held to be made to the real seat of the cause of pain.

The first is at present the common view, though it does not appear to have met with a very searching investigation from physiologists. It is dependent on what is termed the reflection or radiation of sensation.[2] A typical example of this reflection of sensation occurs in toothache. The acute impression made on a few filaments of the fifth pair of nerves, exposed by caries of a tooth, being transmitted to the origin of these filaments in the encephalon, excites not merely the usual change necessary for the consciousness of pain, and its reference to their extremities in the carious portion of the tooth, but flows over on the adjacent parts of the encephalon, where other filaments of the same nerve take their rise. These being thus stimulated in the same manner as those of the affected tooth, give origin to the like sensation; and the consciousness of this accessory sensation of pain is referred to the whole cheek of the same side; and in this way is explained the diffusion of the pain. In like manner, in symptomatic headache, for example, the extremities of the par vagum in the stomach, or of the splanchnic or sympathetic nerve in the bowels, liver, or uterus, being irritated, the impression is conveyed to the origin of the affected filaments in the nervous centre; and being supposed to spread to the other parts of that centre, where sentient filaments, distributed to the dura mater or to the scalp, originate, gives rise to the consciousness of pain there. And similarly, according to this view, when the cause of pain is seated in the encephalon itself, the irri-

[1] See Abercrombie on Diseases of the Brain, cases 53, 54, 55, 56, 57, 58, 60, 69, 100, 106, &c.

[2] Todd and Bowman, Physiology, vol. ii. p. 368; also, Part Third, p. 115. Müller, Physiology, by Baly, vol. i. p. 697.

tation is supposed to spread to the region where nervous filaments of the dura mater, or of the scalp, originate; and thence the consciousness of pain is referred to those parts. The objections to this mode of explanation will be stated after a short consideration of the remaining views.

The second view assumes, that though the encephalon be generally destitute of sensibility in health, yet, like some other parts, it may become sensible under certain morbid conditions, and that this sensibility is independent of nerves, being altogether *sui generis*. Thus the hemispheres of the brain are in so far concerned in every sensation of pain, from whatever source, that though they may not be necessary, as some think,[1] to the mere consciousness of the sensation, yet they are necessary to the remembrance of the consciousness. But as in man, when awake and free from the influence of such agents as chloroform and ether, there is uniformly the remembrance of pain, the hemispheres of the brain must be admitted to be ordinary agents in every complete sensation of pain. When, then, a sufficiently powerful morbid cause operates on the substance of the hemispheres, there may be, according to this view, what is equivalent to a concentration into one of all the several ordinary steps or stages in the process of sensation; these are, 1, an impression on the extremity of a nerve; 2, the extension of that impression to the sensory tract of the nervous centre; 3, a primary physical change there; 4, a transmission upwards through the white filaments to the hemispheres; 5, a secondary physical change there; 6, a consciousness complete enough to be remembered. Or the second view simply amounts to this, that the hemispheres, being the essential seat of sensation, must be capable of sensation, at least when the impression is of sufficient force, even though the subordinate instruments in the ordinary process cannot come into operation. Of this view also hereafter.

The third conceivable view of this question is that here proposed for adoption. It is contained in the three following propositions: —1, That the susceptibility to pain, under certain morbid conditions—some temporary, some permanent—is no special endowment of the substance of the encephalon, but the result of the ordinary law of susceptibility to pain in the vascular solids. 2, That it is not the encephalon, either in its grey or white matter, or the nerves concerned in its proper functions, which are subject to the first effect of irritation in the production of headache, but the nerves subservient to its vitality as an organ of the animal machine. 3, That these nerves are the filaments of the sympathetic, diffused throughout the encephalon in company with the nutrient capillaries.

How far these views bear examination must now be considered.

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[1] Grainger on the Structure and Functions of the Spinal Cord, p. 11.

To go back to the first—the case of headache which most distinctly suggests the reference of the sensation to the cranial nerves, is the pain over the left brow in derangement of digestion. This pain is instanced by Todd[1] as a well-marked example of reflected sensation, the primary impression being conveyed to the nervous centre through the par vagum, and thence reflected to the extremities of the frontal branches of the fifth pair. In this explanation it does not appear why the highest branches, namely, the frontal branches of the ophthalmic portion, should be the channel of the reflected sensation, rather than the branches of the second portion and the sentient branches of the third portion, the points at which these originate being obviously nearer the part of the nervous centre disturbed by the impression conveyed from the stomach through the par vagum. Nor does it appear that there is any greater direct evidence in favour of this explanation which makes the case an anomaly, or at least exceptional, than for the supposition of the pain being really in the anterior part of the left hemisphere, consequent on irritation of the gastric filaments of the left vagus nerve, which keeps it within the general law. It is impossible to deny, on just grounds, that phenomena entitled to be termed *reflected sensations* do occur; but it is an error to regard these as the result of a law extensively applicable to the acts of the animal economy. They seem to arise under the unusual stimulation of sentient nerves, and differ widely in character from *reflected motions*. Without the constant operation of the great law on which reflected *motions* are dependent, the animal machine would quickly come to a stand. On the contrary, the phenomena falling under reflected or radiated *sensations*, are, as it were, erroneous acts of the system, comparatively rare, incidental, forming no part of the ordinary operations in its economy, the result almost uniformly of morbid causes. The conditions on which the unequivocal examples of reflected sensations arise, are obviously the intenseness of the impression made on the extremities of the nerve first affected, and the proximity in the nervous centre of the origin of the nervous filaments, through which the reflected sensation is conceived to be conveyed. But these conditions are not obviously present when irritation of the stomach is supposed to be transferred through the par vagum to the first portion of the fifth pair of nerves, and still less when irritation of the bowels, liver, or uterus is represented as exciting headache on the like principle. If there be affection of the encephalon at the origin of the fifth pair of nerves, then there may be pain of the scalp, independently of any radiation of sensation, or in accordance with the general law.

The second view need not detain us. It must be regarded merely as a last resort, in case the two other views should be dis-

[1] Todd and Bowman, Physiology, Part Third, p. 115.

proved. It violates the ordinary simplicity of physiological laws. Yet were it discovered that there is no such phenomenon as the reflection of sensation, and that no nervous filaments accompany the encephalic capillaries, while it distinctly appeared that the chief part of the substance of the encephalon becomes, under certain conditions, the immediate seat of the consciousness of pain, then we should be compelled to admit two laws of sensation, one in which nerves are essential instruments, and one direct, without the intervention of nerves. But no such necessity is likely to arise.

As regards the third view, which ascribes headache affecting the encephalon to the ordinary process of sensation, exercised by means of minute nervous filaments spreading from the sympathetic throughout its substance, it must be admitted that the existence of such nerves cannot be demonstrated. Nevertheless, the evidence falls little short of demonstrative. It is not doubted by our best authorities, that the sympathetic is distributed in the rest of the body coextensively with the vascular system. Our most recent accounts of the process of inflammation, to which all vascular parts are subject, assign an important share in that process to the nerves of the capillaries. What would become of these views did the capillaries of the encephalon possess no nervous filaments? If the conclusion be all but inevitable, that the sympathetic is spread elsewhere coextensively with the bloodvessels, nothing but a confusion of ideas as to the endowments of the nervous substance could lead to the belief of an exception in regard to the bloodvessels of the encephalon. But the force of this evidence, as far as it goes, being admitted, it may yet be said that the nervous filaments accompanying the capillary bloodvessels are not sentient but organic nerves—nerves not destined to become susceptible of pain, but for the maintenance of nutrition and a high vitality. The sympathetic has all the characters of a nerve adapted for presiding over organic rather than over animal acts. Yet it contains both motor and sensitive filaments. It is an incontrovertible fact, that parts supplied exclusively with nerves from this source—for example, the bowels—are susceptible of the most acute pain.[1] It is not unlikely that the general feeling of bodily wellbeing, and that of bodily discomfort, are dependent on opposite states of the vascular filaments of this system of nerves; and, indeed, Bouillaud has expressly ascribed the latter feeling to such a cause. It is not necessary for the object before us to settle whether the sympathetic be an independent portion of the nervous system, and what is the nature of its connexion with the cerebro-spinal axis. It is enough if it shall appear that there are sentient filaments in the sympathetic; and that

[1] See Swan on Diseases of the Nerves, 1834, p. 289; Grainger on the Spinal Cord, p. 136; Müller, Physiology, by Baly, vol. i. p. 742; and Todd and Bowman, Physiology, Part Third, p. 143.

these filaments, distributed along with the encephalic capillaries, are the immediate instruments of sensation in headache affecting the substance of the encephalon, whether the primary central acts be dependent on the cervical ganglia, or on the cervical portion of the spinal cord. The small amount of nervous substance contained in the plexuses surrounding the carotid and vertebral arteries, hardly forms any objection to this view, when it is considered that, compared to its volume, the encephalon, owing to its unusual supply of blood, and the general fluidity of its constituents, contains but a small proportion of matter in a solid state of organization, or in a form capable of having sensibility imparted to it.

It is a valid reason in favour of this view, that the same principle of explanation is applicable to what seems at first sight an anomaly in the observations made on the spinal marrow, and the trunks of the nerves of sense. The following is Muller's statement of this case— "An apparent contradiction occurs in the nerves of sense and in the spinal cord, namely, that there is sometimes pain in the part to which the irritation is applied, as well as in the extreme filaments, and that pain is not merely felt in all the parts which receive nerves from below the seat of a lesion of the spinal cord, but that the injured or diseased part itself is painful. * * * We are at present ignorant why sensations should at one time be felt in the peripheral parts; at another, in the spinal cord itself."[1] The solution of this difficulty is easy, if it be admitted that filaments of the sympathetic accompany the capillaries of the spinal cord and of the sentient nerves. When the sensory columns of the spinal cord, or the trunk of a sentient nerve, is subjected to any common stimulus, the consciousness of the consequent sensation is referred, in accordance with the ordinary law, to the extremities of the nervous filaments which have thus been stimulated, whether at their origin or in any part of their course. But when a more permanent stimulus operates at the same points, by which the capillary circulation at those points is much excited, then the sensibility of the filaments of the sympathetic distributed there is developed, and the consciousness of the sensation is referred to the extremities of these in the part acted on. Nor is it surprising that a considerable turgescence of capillary vessels should rather affect the nervous filaments spread on their own coats, than the filaments of the nerve which they nourish.

There is, moreover, an objection to the explanation of symptomatic headache by the reflection of sensation, which does not apply to the view here maintained. If a sensation be reflected in the nervous centre to other nervous filaments than those by which the impression was conveyed thither, we should expect that the primary sensation and the reflected sensation would be uniformly of the same kind. Hence, if intense pain of the head be produced by reflection to the nerves of the dura mater, or of the scalp, from a primary irrita-

[1] Muller's Physiology, by Baly, vol. i. p. 802.

tion of the nerves of the stomach, the liver, the bowels, or the uterus, the same kind of sensation—namely, intense pain—should at the same moment exist in one or other of these organs. This, however, is far from being uniformly the case. On the contrary, there is no difficulty in admitting that agents which do not immediately excite pain, may so stimulate the nervous filaments of one or other of those organs, as to disturb the capillary circulation at their origin sufficiently to produce headache in a direct manner.

A few points remain to be noticed which seem to confirm the view which I have been urging. The great cause of temporary headache is obviously local turgescence of the encephalic capillaries. And though the encephalon, owing to its approach to fluidity, be, as a whole, nearly as little compressible as water; yet, in consequence of that almost fluid character, local turgescence of capillaries is more readily admissible than in other organs, the capillaries around the affected part being proportionably compressed. Again, the encephalon being not one organ but a congeries of organs, each endowed with separate functions, upon the ordinary law of excitement under living acts, is liable, beyond other parts, to local determinations. And the encephalon, being the seat of impressions derived from every vascular point of the rest of the living frame, is subject to determinations and other transitory changes, not only from causes affecting directly its own vessels, but from any morbid cause that strongly operates on the nerves even of the most distant parts. Lastly, though causes operating directly within the head, as the effects of intellectual exertions, or of emotions of mind, do very often give origin to such determinations of blood, yet these must more generally result from irritation of the distant extremities of nerves; or, in short, transitory headache is more frequently of what is termed the symptomatic character.

The frequency of speedy relief to headache, when not dependent on serious disease within the head, by the sanative efforts of nature, with or without the aid of remedial measures, has led perhaps to an over-estimate of merely palliative means and temporary treatment, somewhat to the neglect of the principles of a radical cure. A paper of this kind does not afford scope for the discussion of the merits of the ordinary well-known remedies for headache, which I say are chiefly palliative.

The radical treatment, as concerns symptomatic headache, lies in such means as remove sources of irritation throughout the body, and such as so improve the general health as to obviate the renewal of the same irritations. We find headaches occurring under the most opposite states of the system, in the plethoric state, in the anemic, the chlorotic, the scorbutic, the petechial state, the nervous state, the state of increased, the state of diminished, the state of perverted secretion; and lastly, in states of vitiation of the great excretions of the system. Any one of these states, or at least a strong

tendency to any one of these states, may exist without the actual presence of a distinct disease, thus constituting what is conveniently termed chronic ill health. With various diseases chronic ill health combines so as to render the ordinary means of cure unavailing. Symptomatic headache, however, is not so much a disease united with chronic ill health, as a usual part of certain kinds of chronic ill health.

Chronic ill health is a more or less perverted state of the vegetative or assimilative functions, usually consequent on errors of diet and regimen, or on privations and subjection to unfavourable influences arising out of forced occupations. Mere common excesses are not the only errors of diet and regimen, it often happening in modern civilized life, that the habits adopted expressly for the preservation of health become the means by which ill health is perpetuated. It may be observed, that a valetudinarian cast of the constitution being suspected, often on slight grounds, and numerous peculiarities being imagined to exist, the sufferer immediately begins to adopt habits deemed corrective of these, in which he is very prone to deviate far from the standard of healthful regimen. Rules for the supposed preservation of health are seldom safe, when leading to usages much different from the practice of those who keep their health without rules. Between excess and deficiency in the amount of aliment there is but a small interval; and there can be no doubt, that while excess was the prevailing fault in former times, and still is with too many, there are not a few instances met with now in which an under quantity of aliment is taken to the prejudice of the health. The habitual dyspeptic is very apt to sacrifice his general health to the fear of disquieting his stomach, forgetting that dyspepsia can never be cured so long as the stomach participates in a general defect of nutrition throughout the system. Nor must it be forgotten how often a person may indulge in luxurious living without thereby obtaining adequate nourishment.

The fitness of the blood and of the several organs of the vegetative functions to perform efficiently their respective offices, depends on the completeness of the process of nutrition in each. But that process cannot be otherwise than imperfect in chronic ill health. In a chronic state of ill health, then, whether headache or any other source of suffering be the immediate object of attention, the first consideration is, by what means a commencement of improvement may be made, so that, by the gradual application of those, the sanative powers of the constitution may be brought at last to operate without embarrassment. For it is a mistake to suppose, that the physician has any greater direct power over the cure of disease than the surgeon has over the healing of wounds.

The leading principle for the treatment of chronic ill health, and for the permanent removal of the uneasy sensations attendant on it, is the exact adjustment of the food, drink, air, and warmth, to the present necessities of the system.

Chronic ill health may be connected with the plethoric state, in

which event the means of correction are best understood; though even here, perhaps, a nicer adjustment than what the ordinary antiphlogistic treatment affords might be made, by a careful study of each instance in accordance with the principle just laid down. In this case, as well as in the less sanguine and the anemic states of chronic ill health, there may be irritating matters to be evacuated, neutralized, or sheathed, before any progress can be made; for which purposes purgatives, even emetics, antacids, diluents, and demulcents, may be requisite. It is no doubt true, as these irritating matters are continually reproduced while this state of health continues, that evacuants in former times have often been too freely, and even detrimentally, employed, and that diluents and demulcents are of safer use. But it is only the frequent repetition of purgatives that can be hurtful; and the example of trainers for the ring may be regarded as conclusive in favour of the utility of such evacuants as a first step, when the health is to be improved. And though a feeling begins to rise in the profession against strong purgatives, owing to the abuse of these remedies in the earlier part of the century, it is undeniable that some of those, when used with moderate frequency, have the power at once of evacuating acrid matters, and of promoting a more healthy action and secretion in the abdominal organs. Of this description are croton oil, oil of turpentine, and colchicum. The liquor potassæ is the best of the antacids. To their diluent effect is probably owing, in a principal measure, the reputation, in health impaired by dissipation, of some of the less active thermal waters,—witness the Bath water, so much extolled by Heberden in such cases. And it is not impossible that the far-famed repute of the decoction of sarsaparilla for the correction of ill health, which I firmly believe to be well founded, may be owing to no higher quality than its demulcent effect.

I just now referred to the practice of trainers for the ring; and I would now instance their success in improving the respiration, and also the strength and resistance of the muscular system, and, in particular, the action of the skin, as an encouragement to attempt, hardly on similar, yet on parallel principles, the restoration of vigour to the vegetative functions, when impaired by the progress of chronic ill health. The case which comes under the management of the modern trainer, is commonly that of a naturally vigorous young man, whose wind has become impaired, his belly protuberant, his muscles flabby, by all kinds of excesses. A few doses of purgative medicine suffice to relieve the digestive organs from any immediate impediment. After being kept for some time on a regulated diet of plain food, with alternate exercise and rest, he can draw a fuller inspiration, hold his breath longer, and recover it sooner when lost; the muscles become firmer, and contract with force; he is less easily stunned by a blow, and the skin becomes clear, smooth, well coloured, and elastic.

It is, however, only the example of the trainer, not his rules, that we are to follow, in chronic ill health. Every case will require a special direction. It has often been a subject of surprise to medical men, that mineral waters should be of so much efficacy in the restoration of health; but the wonder ceases, when it is considered that, independently of any thing more in the water than a mild diluent quality, the residence at a watering-place, under proper regulation, is nothing else than that kind of training here recommended. A watering-place may be dispensed with, if we can command the same kind of management elsewhere.

For particular morbid conditions, the common well-known rules of treatment are available, too much interference being avoided. In every case where there is the least obscurity as to the nature of the headache, the head should be kept shaved, and cold applied as often as seems to be necessary, to avert unsafe determination of blood. Alterative remedies, as causing least disturbance, are generally best adapted for the ordinary treatment. Under the head of alteratives falls cod-liver oil, which has an inexplicable effect in the promotion of nutrition.

When the excretions assume an anormal character, various well-known remedies are at least of temporary benefit. But neither the bile, the alvine evacuation, nor the urine, can deviate far from the healthy state, without a previous fault in the quantity or quality of the food or drink, in the supply of fresh air, or in the primary processes concerned in the preparation of the aliment; and therefore the first and principal attention, even in these cases, is still due to the means of promoting a healthy nutrition.

ARTICLE II.—*Remarks on Certain Displacements of the Unimpregnated Uterus, with Cases.* By JOSEPH BELL, Member of the Faculty of Physicians and Surgeons of Glasgow, Lecturer on Botany, Anderson's University, Glasgow.

(*Read at a Meeting of the Glasgow Medical Society, Nov.* 16, 1847.)

THE unimpregnated uterus is subject to various displacements. Descent, prolapsus, and procedentia, are the most common forms; next in the order of frequency we have retroversion and anteversion. Less often we find the organ bent upon itself, constituting either anteflexion or retroflexion.

In the following remarks I intend briefly to allude to the causes, the prominent symptoms, and treatment of the four latter forms of displacement:—

1. RETROVERSION.—This condition I have reason to believe very frequently exists. The organ is quite altered in its position—the fundus lies in the hollow of the sacrum—and the os uteri is turned upwards and forwards, either resting upon or above the symphysis pubis.

Causes.—Parturition may be enumerated among the causes of retroversion of the unimpregnated uterus. I have known the affection very frequently follow child-birth; indeed it is by no means difficult to conceive how this malposition will be likely to occur when the female resumes her avocations within a few days after her child is born, a practice very common among the lower and working classes of society. The uterus remains enlarged and the vagina relaxed for several weeks after delivery; the fundus being the heaviest portion, it must necessarily fall in the direction where there is least resistance, viz. the hollow of the sacrum; a distended bladder tending not only to cause but to aggravate the displacement. During seven years of my professional life, I had an extensive midwifery practice among the wives of the working people, and I have found these females very subject to this displacement. It could almost universally be traced to their having left their beds soon after delivery, and to the resumption of their domestic duties.

It is needless to remark, that parturition cannot be the cause of this malposition in those who have never been pregnant. In instances occurring in the unmarried, I have found the uterus either enlarged or its walls occupied by tumours, or the patient had laboured under dysmenorrhœa for years.

I am inclined to consider this affection as a cause not only of retroversion, but also of the other varieties of displacement. Dysmenorrhœa, when protracted, always produces congestion and enlargement of the uterus, conditions tending to change the normal position of the organ.

I have met with a few cases in which retroversion followed an acute attack of metritis; the inflammation had probably subsided into a chronic form, and had gradually produced enlargement, and consequent displacement.

Some may be inclined to view the enlargement of the uterus which almost invariably exists in retroversion, as the result of the altered position, and not the cause. I have no doubt but that the abnormal change does cause congestion and enlargement; judging, however, from my own observations, I am convinced that the organ is generally enlarged more or less before it becomes displaced. These different opinions, however, have no great practical bearing, as we will have occasion hereafter to observe.

Symptoms.—The symptoms may be enumerated in a few words. Difficulty of evacuating the contents of bowels and bladder, particularly the former, pains and a feeling of weakness in loins, often referred to lowest part of sacrum, a sensation of weight about perineum, dragging sensations about iliac and epigastric regions, pains shooting down thighs. These symptoms are aggravated on exertion, on standing, walking, or riding; the patient is often unable to take the slightest exercise without experiencing much suffering; a vaginal discharge often exists, generally thick, not abundant, sometimes coming away in cheesy or curdy masses.

General derangement of the health soon takes place, the patient becomes hysterical; there is irregularity of the menstrual discharge, both as to period and quantity.

On vaginal examination the finger comes in contact with the fundus uteri lying in the hollow of sacrum, the cervix can be traced forward to the symphysis pubis, against or above which the os uteri is detected.

Diagnosis.—The only affections with which retroversion can be confounded are (*a*) retroflexion, and (*b*) tumours in the recto-vaginal septum, and in the posterior wall of uterus.

(*a*).—The diagnosis between retroversion and retroflexion will be pointed out when we describe the latter displacement.

(*b*).—Besides the form of the tumour, and its connexion with the cervix and os uteri anteriorly, the uterine sound will easily enable any careful practitioner to distinguish retroversion from pelvic tumours (whether ovarian or not), however closely they may be applied to the uterus. If it be the fundus uteri which occupies the hollow of the sacrum, the sound will be found striking against the finger either through rectum or vagina. When using the sound in such cases, I usually place the index finger of left hand on the tumour through the rectum, and thus the other hand has more freedom to direct and use the instrument than when the finger is introduced into vagina. Besides this, in investigating displacements of the uterus, much valuable information will be obtained from the examination being made through rectum as well as vagina. You can in this way examine the posterior surface of uterus, and obtain an accurate notion of the extent of displacement, the size of the organ, its relation to the neighbouring parts, and whether or not it has become abnormally adherent.

As the treatment of the uterine displacements which are to be considered in this paper differs very little, I shall defer my remarks on it until the other forms have been described.

2. ANTEVERSION.—In this misplacement the fundus uteri lies forward either on the symphysis pubis or under it, the os and cervix being turned backwards and upwards towards the promontory of the sacrum. Lisfranc states, that anteversion is infinitely more frequent than retroversion. Boivin and Dugés affirm that it is one of the most common affections to which the uterus is subject.

As regards my own experience, I have met with very few cases. Many instances of considerable obliquity of the uterus have come under my observation. The uterus in its normal situation has its fundus placed towards the pubis, and the neck and mouth are turned backwards, the organ thus having the direction of the superior pelvic axis. Now, this position very often presents a considerable amount of obliquity; but this does not constitute anteversion, which appears to me to consist in that change in the direction of the uterus, when the os uteri is not only directed backwards but upwards towards the

promontory of sacrum, and the fundus turned downwards towards the anterior wall of vagina.

Causes.—I am convinced that anteversion sometimes arises from want of proper rest and care after parturition. Dysmenorrhœa I have known to precede and accompany the displacement.

The only instance which I have had under my care in the virgin state, irregular and painful menstruation had always existed.

Levret, Desormeaux, Lachapelle, and others, hold the opinion, that chronic metritis generally, if not always, precedes anteversion.

Symptoms.—These do not differ much from those of retroversion, some are more prominent, and others less so; in general, dysuria and retention of urine are more severe and frequent than in retroversion.

Diagnosis.—This is so easily formed by a vaginal examination, and by the use of the sound, that it would be a loss of time to enter into details. There is sometimes considerable difficulty experienced on the introduction of this instrument, in consequence of the os uteri being turned upwards and backwards. By introducing a finger into rectum, I have been able to bring down the os uteri, so as to place the sound in the uterine cavity with facility.

Some French practitioners recommended a windowed spoon (cuiller fenêtrée) for this purpose.

After this brief sketch of the causes, symptoms, and diagnoses of the "versions" of the uterus, I shall, in the next place, consider the "flexions" to which the organ is subject.

1. Retroflexion.—Though many writers seem to doubt the existence of this malposition, yet I am convinced that it often occurs. The reason why the writers alluded to have not met with the affection, must be ascribed to the imperfect nature of their investigations. Independent of my own observations, the numerous cases recorded by many acute and careful observers, appear to me to settle the question of the existence of the displacement beyond all possible contradiction. As to its relative or positive frequency, that is quite a different subject, and cannot probably at the present day be satisfactorily discussed. We require additional observations.

The best description of the displacement is given by Denman—it is, "such an alteration in the position of the parts of the uterus, that the fundus is turned downwards between the rectum and vagina, whilst the os uteri remains in its natural situation. An alteration which can only be produced by the curvature of the uterus in the middle, and in one particular state, that is, before it is properly contracted, and when a woman is delivered."

Causes.—All the cases which I have seen, except two, could be distinctly connected with improper freedom exercised shortly after delivery. In several instances the patient's sufferings did not fully develop themselves immediately, often a very considerable period elapsed before much inconvenience was experienced. Denman considered that the disease only originates immediately after parturition. Dr Ash-

well is of a different opinion, and has recorded two interesting cases in support of his views. Boivin and Dugés have recorded two cases occurring in young females. Amenorrhœa was the principal cause to which these authors ascribed the affection. It is singular that both of these patients died from hypertrophy of the heart. In one, the cervix uteri was obstructed by chalky matter, and, in the other, the same deposit was found in the Fallopian tubes.

I have met with one case in the virgin state, and another in which an attack of metritis preceded the displacement; but, as I have already observed, these are the only two instances unconnected with parturition that have come under my notice.

Symptoms.—The symptoms differ very little from those of retroversion. The evacuation of the contents of the bowels is attended with equal difficulty and uneasiness in retroflexion as in retroversion. The difficulty of voiding urine is not so much experienced in the former as in the latter. I have met with two instances, however, in which this was the most prominent symptom, being indeed the principal complaint for which medical aid was requested. In these cases the uterus was much enlarged, the os being greatly augmented in size, fissured, ulcerated, and pressing against the neck of the bladder. The sensations of dragging about epigastrium and sides, pain, &c. &c., are also experienced in retroflexion.

When a vaginal examination is made, the finger comes in contact with the os uteri low down in vagina, being nearly in its natural position (generally inclined a little forwards), it is often fissured and ulcerated or abraded; behind it, and lying upon the rectum, the fundus is found in the form of a large tumour. By running the finger backwards from the cervix, the acute angle at which the flexion occurs, is easily detected. The finger through the rectum can be readily passed over fundus, by depressing which the os uteri is tilted upwards and forwards.

Diagnosis.—There cannot be much difficulty in distinguishing retroflexion from retroversion. The position of the os uteri is of itself almost sufficient. In the former it is nearly natural, in the latter it is carried forward and upwards.

From tumours situated in the recto-vaginal septum (of whatever character they may be), also from those in the uterine wall itself, the diagnosis is easily made by the use of the uterine sound, the apex of which can be distinctly felt striking against the fundus of the uterus in its abnormal situation. Sometimes both retroversion and retroflexion are complicated with tumours in the posterior wall of uterus; but the use of the sound, combined with the uneven surface of these tumours, will enable us to decide when such a complication exists.

2. ANTEFLEXION.—This misplacement stands in the same relation to anteversion as retroflexion does to retroversion, the uterus being bent upon itself to such an extent, that its posterior wall becomes anterior, and lies behind the symphysis pubis, the cervix and

os remaining in their normal position. Dr Ashwell states that the point of flexion is usually a little above the union of the cervix with the body of the uterus, and that the degree of curvature determines the slightness or severity of the affection. In the two instances which have fallen under my observation, the flexure took place at the point mentioned by this excellent writer. Anteflexion is the most rare of the malpositions to which the uterus is liable. Very few cases have been recorded.

Causes.—In one of the cases which I had under my care, the apparent cause was a very speedy delivery, and perhaps irregular uterine contractions afterwards. The patient was standing on the floor when seized with a pain, and, before she reached the bed, the child and placenta were expelled. She suffered intense after-pains, and shortly after delivery she began to move about the house, when dysuria and other symptoms commenced; this led to a vaginal examination, which detected the malposition. In the other case, the patient had a miscarriage; this was followed by an attack of inflammation of the uterus, and ultimately misplacement.

Some authors enumerate among the causes of anteflexion, irregular contractions of the uterine walls after labour, and chronic inflammation. Others ascribe it to direct causes, such as falls, straining at stool, or in the evacuation of urine. I am inclined to think that the latter class of causes would not be likely to produce the affection, unless some abnormal condition previously existed.

Boivin and Dugés have referred it, when occurring in young unmarried females, to rapid and disproportionate growth of the uterus at the period of puberty. This seems to me to be a very powerful cause of the unnatural position; if the posterior wall grow more rapidly than the anterior one, either anteflexion or separation of the two walls must occur, and, as the latter is impossible, we must necessarily have the former state produced. This also seems to me to explain how inflammation of the uterus, when that action is confined either to one wall, or a portion of it, produces flexion of the organ. The satyr in the fable expresses his surprise that the man should blow heat and cold alternately with the same breath; it seems equally strange to us, how inflammation should produce two such opposite conditions as hypertrophy and atrophy in organs subjected to its influence. Singular as this may appear, yet such occurrences are most frequent, and may be ascribed to differences existing as to the seat and nature of the deposit effused, and also to the effects produced by such effusion on the processes of nutrition in the organ affected.

With reference to our present subject, it matters not whether the inflammation has produced either hypertrophy or diminution of the whole or part of one of the uterine walls, flexion of the organ must take place. For example, if the posterior wall be enlarged, the fundus uteri will be turned anteriorly; the anterior wall forming the concave, and the posterior the convex side of the curve. The same result must take place if the anterior wall become atrophied. On

the other hand, when the anterior wall becomes enlarged, or the posterior contracted, retroflexion will be produced.

Thus all cases of partial, or of what might be called parietal inflammation of the uterus, if not cured, must necessarily be followed by flexion of the organ; the direction and severity of the flexion being determined by the seat and extent of the hypertrophy or atrophy.

Symptoms.—These differ very little from those which attend the displacements already described; we have the same bearing down pain, and sense of fulness about vulva and perineum, pains in loins and thighs, difficulty in voiding the contents of bladder and rectum, particularly the former, but this is not always the case. Dr Walshe mentions an instance in which no inconvenience was felt in voiding the urine, but there was great difficulty at stool. Legrand and others have met with cases in which no difficulty attended the evacuation either of bladder or bowels.

Diagnosis.—It must be evident that no decided reliance can be placed on the symptoms without a vaginal examination. Indeed, this is a *sine qua non* to enable us to form our diagnosis.

The os uteri is found in its normal place, and by running the finger along the anterior surface of the cervix, the chink or angle made at the point of curvature is felt, anterior to which the fundus is found impinging upon the neck of the bladder. The uterine sound enables us to distinguish the flexed uterus from all kinds of pelvic tumours, and thus gives certainty to our diagnosis.

We cannot speak too strongly of the great advantages derived from the use of this instrument in the elucidation of uterine diseases. If Dr Simpson should never add another improvement to medicine, the invention of the sound would entitle him to the best thanks of the profession. By the sound we can now form correct notions of a large class of important affections, which were otherwise involved in obscurity and uncertainty.

Before Dr Simpson made his invention known, I was in the habit of employing in the examination of uterine diseases an elastic bougie made upon a steel wire; but this was vastly inferior to the simple and ingenious instrument of Dr Simpson.

The speculum vaginæ is not required in the investigation or diagnosis of these misplacements of the uterus; the "*touch*" with the use of the "*sound*" being quite sufficient. In flexions of the uterus the speculum will be apt to mislead. We can only bring the os uteri into view, and as this retains nearly its natural position, we would have no reason to apprehend misplacement. Again, the os uteri being generally indurated, red, and even ulcerated, in these abnormal positions, if we use the speculum alone, we would be apt to conclude that the tumefaction, redness, and ulcerations were the sole cause of the patient's sufferings. I allude to this point, as I have known several mistakes to arise from the use of the instrument in

such cases. This could not possibly have occurred had the examination been made with the finger.

Treatment.—The treatment of the four misplacements under consideration may be divided into two parts: I. The Medical. II. The Mechanical.

I. Medical Treatment.—It must appear self-evident to every person, that either when enlargement of the uterus or inflammation exists, these conditions must be removed before we can expect to cure the patient.

It may be said that the enlargement and inflammation are the results of the malposition, and not the cause; consequently it would be more rational to replace the uterus, and afterwards treat the effects. In answer to this, I would submit the following observations:—1*st*, The relation here assumed does not always exist; indeed, I think that it very rarely happens. 2*dly*, Though you *can replace* the uterus, yet it will very soon relapse into its abnormal position, unless the enlarged condition be removed. 3*dly*, The uterus frequently *cannot* be restored to its proper place until it has been reduced in size. 4*thly*, By removing congestion and hypertrophy, the organ will very frequently either spontaneously resume its natural site, or pregnancy occurs, and subsequent to parturition no relapse takes place. 5*thly*, When you have reduced the hypertrophied condition, if the uterus does not unassisted take its normal situation, manual interference will then be more successful. Hence, in a practical point of view, it is of very little importance whether congestion, inflammation, or enlargement has preceded or followed the displacement. Our treatment ought in either case to be conducted on the same principle; namely, the employment of means to remove the results of congestion and inflammatory action, previous to any effort being made to restore the organ to its normal position.

Judging from my own experience, and from the statements of several authors, especially of Boivin and Dugés, I have little hesitacion in affirming, that when judicious means are used, in conformity with the principle here adverted to, few cases will require mechanical assistance.

The following is an outline of the treatment which I have found most useful:—

1. Confinement to the recumbent position, either on a sofa or mattress. Rest I conceive to be a matter of the greatest importance. Exercise always causes, not only an aggravation of the patient's sufferings, but I am convinced of the affection also, at least very essentially protracts the recovery of the patient. A consideration of the abnormal position of the uterus must render obvious the truth of these observations.

2. Regular condition of the bowels; this requires strict attention. In retroversion and retroflexion, constipation is particularly injurious. The impacted feculent matter in the large intestines must by

its pressure increase the extent of the misplacement, and as these hardened matters make their exit slowly through the rectum, a very considerable amount of irritation and congestion must be produced both in the rectum and uterus; in the former it is made evident by the large quantity of coagulated mucus (not unlike macerated maccaroni) which is voided at stool; besides this, the patient's sufferings are greatly augmented.

The laxative which I have found most useful is either olive oil, or an electuary composed of sulphur, cream of tartar, and molasses. Of the former I order a large table-spoonful night and morning, if the oil be free from rancidity—it not only keeps the bowels easy, but it also improves the general health. In several cases the advantage of this over many other laxatives was most striking. Enemata generally prove injurious. I have not employed any thing of this nature for a considerable period.

3. Local depletion is of great use, particularly when there is much tenderness of the uterus, or severe pains about loins and thighs; in such cases local bleeding is almost indispensable. The application of a few leeches to vulva, vagina, or os uteri, once or twice each week, will prove most beneficial. Cupping the loins is also an excellent remedy.

4. *Mercury.*—When the uterus is much enlarged and indurated, my chief reliance has been on mercurials.

I have seldom found these fail when properly employed. At one time I prescribed small doses either of the mercurial pill, or of hydrargyum c. creta, morning and evening, until the gums became tender. Since the iodide of mercury has come into general use, I prefer it to any other preparation. I order it in half grain doses every eight hours, made into a pill with conserve of roses and extract of hyoscyamus.

In my hands, its action has proved most beneficial in reducing the size of the uterus. Salivation has sometimes resulted from its use, and oftentimes it has produced considerable intestinal irritation, but this inconvenience was obviated by diminishing the dose. After the mercurial action has been established, I have found much benefit from the hydriodate of potass, given in combination with some mild vegetable tonic, such as colomba or cascarilla.

By this method of treatment the enlargement and inflammatory action of the uterus rapidly subsides, and the organ resumes (in many cases spontaneously) its natural position,—and if no adhesions have been formed, a little manual assistance is all which is necessary for its replacement. Often when the uterus becomes reduced in size, the patient ceases to experience any unpleasant sensation or inconvenience,—the symptoms and complaints disappear in proportion, as the uterus decreases in size. This leads me to believe, that if the uterus were merely displaced, and not augmented in size, the patient would not feel any great inconvenience from the accident; at least I am certain, not to the same extent as she must do when

hypertrophy exists. Not having met with any case uncombined with hypertrophy, I confess myself unqualified to discuss the subject any further than to repeat the remark, that in several cases of retroversion in which the organ was greatly hypertrophied, and in which the patient suffered exceedingly, when the hypertrophy was removed, all the symptoms vanished,—and though the uterus still remained in its abnormal situation, yet the patient was able not only to go about without any inconvenience, but had no return of those feelings which previously had rendered her unable to take any exercise. Now, we would think if the malposition was the cause of the inflammatory action, and consequent hypertrophy, that in such cases a relapse of this condition would have occurred. So far as my experience extends, I have not observed any such thing taking place.

Improvement of the general health I hold to be of the utmost importance in the treatment of displacements of the uterus. Profuse menstruation must be checked,—the action of the stomach, liver, kidneys, and skin promoted; sponging the surface with a solution of salt, and the tepid bath medicated with salt and mustard do much goood. If vagina be much relaxed, the alum hip bath is useful. In a few words, we should have recourse to every means that is calculated to improve the tone of the general system,—this ought to be kept steadily in mind in the treatment of every form of uterine disease, as well as in those at present under consideration.

In all local diseases, especially of long duration, occurring in impaired constitutions, whether that impairment has preceded or followed the local affection, no local treatment will succeed if we do not at the same time improve the general health.

II. Mechanical Treatment.—This may be divided into manual and instrumental. Several authors recommend the uterus to be replaced by the hand introduced into vagina and rectum; for a description of these manipulations, I refer you to the works of Ashwell, Boivin and Dugés, &c.—This mode of treatment may be useful when the displacement is recent, and uncomplicated with hypertrophy, especially if the displacement has occurred immediately after parturition.

With regard to the use of instruments, I can say little from my own experience; various kinds of pessaries have been proposed—none probably so ingenious as those invented by Dr Simpson.

These instruments, *especially the latter, may be serviceable* in retroversion; but I doubt their efficacy in cases of anteversion, and the flexions of the uterus. If inflammatory action exist, I would be afraid that the presence of these instruments would excite this process, and thus aggravate the case. It is but justice, however, to mention, that Desormeaux and others consider that the irritation caused by the use of the common pessaries tends very much to cure the enlargement of the uterus, by increasing the action of the absorbents. If there be any truth in this opinion, Dr Simpson's in-

struments must prove useful in two ways; that is to say, by removing hypertrophy, and maintaining the uterus in its proper place. If no bad consequences result from the employment of the style, and if it retain the uterus in its proper position until enlargement be removed, I consider that the instrument will be a most valuable acquisition in the treatment of retroversion, especially as the patient will be thus enabled to avoid the confinement which is otherwise indispensable—and which tends more or less to impair the general health in young females.

The sponge pessary I have often introduced, after the uterus had regained its normal size:—I have never found its use to be required beyond a few weeks; in some cases it was discontinued in a few days, in consequence of the irritation which it caused.

Probably my good fortune may have been owing to the mildness of the cases which have come under my care. I would remark, however, that in more than one instance, where mechanical means had been previously employed without success, the medical plan of treatment which I have recommended proved most effectual.

I may mention, that Boivin and Dugés recommend antiphlogistic treatment in all cases of flexion and anteversion of long standing. Levret, Désormeaux, Lachapelle, also treat these displacements by antiphlogistics, the opinion of these authors being that chronic metritis always exists.

I am happy to add the testimony of Dr Beatty of Dublin, in confirmation of the mode of treatment which I have advocated. Since I commenced to write this paper, I have read with much pleasure his excellent article on retroflexion of the uterus (in the *Dublin Quarterly Journal.*) He narrates two cases successfully cured by treatment conducted on the same principles, and by similar remedies as those which I have mentioned.[1]

In submitting the following cases to the consideration of the reader, I would observe that I do not wish him to regard them as *proofs* of the curative power of the mode of treatment adopted, but simply as the record of the effects which have followed that treatment, not only in the few cases which I intend to quote, but in a very considerable number of the same character which I have had under my care. The cure of any disease is perhaps as complex as its cause, that is to say, depends on the conjoint action of several conditions or agencies. Hence it must be always a difficult question in medical reasonings to determine the exact share or

[1] Since this paper was read, several valuable communications have appeared on the same subject in the various Medical Journals. Dr P. Smith (*the Obstetric Record*, p. 35), Dr Hensley (*Provincial and Surgical Journal*, Jan. 1848), Dr Simpson (*Dublin Journal*, May 1848), and still more recently Mr T. S. Lee (*Medical Gazette*). Dr Simpson has appended to his valuable contribution an excellent bibliographical note, which must prove very useful to those who feel interested in these affections.

relation which any mode of treatment holds in the cure of disease; consequently, cases in which recovery occurs are more to be regarded as *illustrations* than as *proofs* of the effects of any plan of treatment. To view them in a stronger light will lead to error; indeed, cases may thus become most detrimental to the progress of medical science. On the other hand, I would beg to dissent from that scepticism which denies all relation between medicinal agents and the cure of disease. When we find affections yielding almost invariably under the employment of certain remedies, I contend that it is irrational to deny that these agents have no share in effecting the cure, because similar cases so treated have terminated fatally, or got better under different means, or without any at all. It surely does not follow, that the results of our treatment ought not to guide us in the management of disease, because we cannot explain to the satisfaction of one who shuts himself up in the dark folds of his cold scepticism, how certain agents occasionally fail to effect a cure in diseases, in the treatment of which a successful result generally follows their use. Such a mode of reasoning is still more fatal to the advancement of medicine than the former. A medium course is the best. The combination of several circumstances may be necessary to effect a cure in every case. Some of the conditions necessary to a successful result we too often overlook,—others are beyond our powers to appreciate, and some we cannot render available. Thus, the constitution of the patients, their age, the mental and physical circumstances under which they have been, and are placed,—the stage of the disease, its complications, &c. &c.,—all tend to influence the result of any medicinal treatment. Thus the process of cure, as I have already observed, is as complex as the cause from which the disease originated; consequently we err most seriously when we ascribe the cure to our "*physic*" alone. I have made these remarks, so that I cannot be considered as demanding a greater amount of consideration for the cases which I shall now quote, than that to which they are justly entitled. I place them before the reader as a faithful record of certain occurrences following a mode of treatment, in a class of cases which some talented medical men seem to consider unmanageable by medicinal agents, and to be cured principally, or alone, by instrumental means.

CASE I.—*Retroversion of Eighteen Months' duration—Cure by Rest—Mercurials—Leeches.*

July 10, 1841. Mrs A——, of a sickly emaciated appearance, mother of three children—complains of general derangement of health, weakness, pain of back referred to the sacrum, dragging sensations about iliac regions, difficulty and pain in voiding the contents of the bowels and bladder. A thick cheesy discharge from vagina—bowels constipated—globus and other hysterical symptoms. Complaints commenced shortly after the birth of her last child, eighteen months ago. Had excellent recoveries after former deliveries, being able to resume her domestic duties on the second or third day after birth of child—on the last occasion had more than the usual amount of exertion in con-

sequence of a change of residence, which she had to effect on the third day after delivery.

On making a vaginal examination, the uterus was found retroverted, the fundus lying low down; it was painful when touched, and considerably enlarged.—*Habt. Hyd. c. cret.* gr. ijss.—*Extr. Hyoscy.* gr. ij. *ex form pil. t. d.*—*Admov. Hirud.* vj. *ad Memb. inter. Vagin.*—*Habt. ol. Olivar* ʒj *b. d.*—*Quiescat in lectu.*

This patient's gums became tender at the expiry of eight days, when the mercury was discontinued. The leeches were repeated, and she was ordered to use the following mixture three times each day:—R. *Hydriod. Potass.* gr. iiss. *Tinct. Cardamom. co., T. Hyoscyami, nigr.* aa. ʒss. *Infus. Colombæ,* ʒij.

When I examined her on the 20th August, the uterus had resumed its natural position and size, and was free from all tenderness—she had no complaint, and had regained flesh very considerably.

I might quote many cases very similar to the above, but this I deem would be unnecessary. The one now narrated affords a very fair specimen of the result of the plan of treatment which was adopted.

I may mention, however, that in some cases connected with dysmenorrhœa, and also with tumours in the posterior wall of the uterus, the organ in the former class of complications often relapsed into its displaced position until the dysmenorrhœa was cured, and in the latter class, viz. where tumours existed, the uterus, though replaced, generally relapsed. The first case of this kind which comes under my care, I shall make trial of the style pessary of Dr Simpson, in order to prevent, if possible, the repetition of the displacement.

CASE II.—*Retroflexion of Three Years' duration—Treatment by Local Depletion —Mercurials—Tonics—Cure—Subsequent Pregnancy.*

September 6, 1843. Mrs R—, æt. thirty years, complains of severe pain at lower part of back, pains through abdomen, dragging sensations about iliac and hypogastric regions, a feeling of heaviness about perineum, leucorrhœal discharge profuse. Her complaints are much aggravated by exercise—indeed, she is almost unable to walk; has much difficulty in making water, as also in defecation; stools contain a large quantity of coagulated mucus, not unlike maccaroni.

On making a vaginal examination, the uterus was found in a state of retroflexion, and much enlarged; the fundus pressing down on posterior part of perineum; the cervix and os much augmented in size, and turned somewhat forward to pubis; the os dilated, and its limbi hard, and covered with superficial abrasions. In consequence of the amount of hypertrophy which existed, the uterus could not be moved to any extent; in fact, it completely packed up the lower part of pelvis.

Her complaints commenced a few days after birth of last child (three years ago). She had walked from one bed-room to another on the second day after delivery, and, when in the act of walking, she found "something give way or fall down in the right side." This was followed by difficulty of evacuating the contents of bowels and bladder, and febrile symptoms. Was treated for "*inflammation*" at the time. Has not enjoyed good health since; suffers much from profuse menorrhagia at the menstrual period: it was in consequence of this that my attendance was requested.

The attack yielded to gallic acid. After this she was ordered *Hyd. c. cret.* gr. iij. *Extr. Hyoscyam.* gr. ij. *in form. pil. b. d.* et *Habt. ol. Olivar* ʒij. *om.*

nocte. Absolute rest was at same time enjoined. At the end of a week from my first examination of the case, not finding the tenderness yielding, she had eight ounces of blood taken from the loins by cupping; this was repeated twice at intervals of a few days. On 21st September the mercury was discontinued, in consequence of tenderness of gums. She then had the hydriodate of potass and Infus. Colombæ mixture. On the 10th November the uterus was found reduced nearly to its normal standard, but still retroflexed. She would not submit either to manual or instrumental interference for its adjustment, as all her complaints and uneasy feelings had disappeared.

She became pregnant a few weeks afterwards, and was safely delivered at the end of her full period. After delivery I caused her to keep her bed for the period of three weeks; a very necessary precaution to prevent relapses, and one which I always adopt. In the present instance no tendency to relapse occurred. I may mention that this patient has had two children since.

CASE III.—*Retroflexion of Long Duration—Mercurial Treatment—Fundus Uteri replaced by Hand—Cure.*

Sept. 11, 1846. Mrs S—, æt. thirty-two years; tall, and of dark complexion; complaints very varied—all of an hysterical character. She has pains every where, but fixed nowhere for any length of time. Appetite much impaired; some debility; want of sleep; pulse 78, weak; tongue furred; bowels regular; catamenia regular; a thick vaginal discharge exists. On a vaginal examination the os uteri was detected nearly in its proper position, but the fundus was lying between it and the rectum, in the form of a large tumour. At the point of junction of the neck with the body of the organ, the finger easily found the angle or chink at which the flexion had taken place. The os uteri felt indurated; and, by the aid of the speculum, several superficial abrasions and fissures were detected in its limbi.

About six years ago she had her last child. Two days after birth the infant was seized with convulsions; this caused her to rise frequently from her bed. She was then seized with what was called inflammation, since which she has never been well, but lately has become much worse, more nervous, dyspeptic, and annoyed with pains shooting through the pelvis. She has been for some time under *tonic* treatment for her weakness, but without any advantage. She was ordered to keep her bed, and to take—*Hydrarg. Iodid,* gr. ss.; *Ext. Conii. Ext. Hyoscy.* aa. gr. i.; Misce et fiat *pil. omn. mane et nocte sumend.*

Her pills had to be suspended on the 18th in consequence of diarrhœa, but were resumed on the 20th, and continued, with occasional intermissions of a few days, to 20th November, when the following note of her case was recorded:—Is greatly better in every respect; uterus much reduced in size, and easily moved in every direction. She was placed on her elbows and knees; a large rectum bougie, with a piece of sponge tied to its end, and covered with oil-silk, was well oiled and introduced into rectum, in the same manner as used by Dr Ashwell,[1] the fingers of the right hand at same time being introduced into vagina. By pressing the os uteri backwards with these fingers, and the fundus uteri forcibly upwards by the bougie, the organ was with some difficulty placed in its normal position. A piece of sponge, covered with oil-silk, was introduced into posterior part of vagina. This was discontinued at the end of two weeks. No relapse has taken place. The patient now enjoys the best of health.

CASE IV.—*Retroflexion—Great Enlargement of Uterus—Relief—Relapse.*

June 10, 1843. Mrs G—, æt. forty-five years, mother of a large family, has had profuse menorrhagia during the last two days. She has had similar attacks during the last two years and a half. The discharge on

[1] On Diseases of Women, 2d edit. p. 618.

the present occasion was restrained by gallic acid and cold applications. On inquiry, I found that her health had been much impaired from the birth of her last child; a few hours after the delivery of which, in consequence of the clothes of another child having caught fire, she arose from her bed suddenly, and was seized with inflammation the day afterwards. From this time she never fairly recovered. Has been much troubled with pains through the pelvis and lower part of the back, with difficulty of voiding urine; and she suffers much when at stool. Her menstruation is most painful and profuse. On making a vaginal examination, the uterus was found to be not only retroflexed, but enormously enlarged. The os uteri covered with deep fissures. She was ordered to keep her bed, to take the iodide of mercury, and to use the *Krameria triandra* as a tonic. At the end of two months her complaints had completely subsided. The uterus reduced to its natural size, but resisted every effort to restore it to its natural position. She felt no inconvenience for several months. In the end of August she had a severe attack of dysentery, after the subsidence of which her old complaints returned; the uterus becoming much enlarged and painful. She was again put under the previous treatment, and in a few weeks the size of the organ was reduced, but no replacement could be effected. She experienced no inconvenience from the malposition, and is at the present time (November 1846) in the enjoyment of good health.

I have met with several instances, besides that of Mrs G—, where no inconvenience was felt by the patient when the enlargement of the uterus was removed; the organ still remaining flexed. In the present case, the want of success attending the attempts to restore the organ to its natural position, arose from adhesions having been formed between it and the neighbouring parts.

CASE V.—*Retroflexion of Long Standing—Mercurial Treatment—Pregnancy—Cure.*

Nov. 23, 1846. Mrs J—, æt. twenty-eight years, of low stature, dark complexion, said to have been labouring under uterine disease during the last four years. Her complaints were very similar to those so often enumerated. On making a vaginal examination, the uterus was found to be retroflexed, and much augmented in size, so much so that it could scarcely be moved in vagina. Her general health is much impaired. She has undergone much treatment of various kinds. I ordered her to keep constantly either her bed or the sofa, to maintain an easy state of bowels by olive oil, and ordered her the iodide of mercury and vegetable tonics. These remedies were followed by great improvement of the general health, subsidence of her complaints, and reduction in the size of the uterus. Early in January 1847, the organ could be easily moved about the vagina, and was free from pain, but continued retroverted. I wished to endeavour to restore the fundus to its place, but the patient informed me that the menstrual period had been passed for a fortnight. Under these circumstances, pregnancy being probable, no interference was made. Pregnancy had occurred, and she was safely delivered of a fine female child, at full period. I caused her to keep her bed a full month after delivery. No relapse has taken place.

In this instance, as well as in Case II., the cure may be ascribed to pregnancy. I have seen another case lately, in which a similar result has occurred. The enlarged condition of the uterus seems to prevent impregnation, or at least gestation; but by the removal of the diseased condition, the organ seems quite capable of performing its functions, independently of its flexion. Abortion, however,

frequently takes place, and in one of these cases the woman was often threatened with this occurrence.

Case VI.—*Anteversion—Mercurial Treatment—Rest—Cure.*

On the 20th May 1842, I was requested to visit Mrs W—, æt. twenty-six years, in consequence of her inability to void urine. On inquiry, I found that about eight months since her first child was born; she had a good recovery, was able to leave her bed on third day after delivery; soon after she experienced difficulty in voiding urine, pains shooting through from pubis to back; these she ascribed to jumping suddenly from a chair. The pains and dysuria have continued to annoy her much; indeed the latter has gradually increased in severity, and now there is complete retention. Her general health is much impaired.

On instituting a vaginal examination, the fundus uteri was found lying behind the symphysis pubis, the os tilted up to the promontory of the sacrum. With considerable difficulty it was brought downwards (by introducing the two first fingers of the left hand into the rectum), and the urine withdrawn by the catheter. The uterus was then completely restored to its proper position. On the 22d, I was again sent for under the same circumstances. Similar manipulations were adopted with the same result; but in order to maintain the uterus in its normal situation a sponge pessary was introduced. This could not be worn; a ring one was next applied, but it had no effect in preventing the displacement. (June 1st), She was now ordered to keep her bed, and to take hydrarg. c. cret. gr. ij. every six hours. Care was also taken to prevent accumulation of urine. On the 28th the uterus was reduced in size and had resumed its proper place, and no relapse afterwards took place.

In this case, the advantage of removing the congested condition of the uterus was very obvious. Every thing previously done was entirely useless. If I had placed her under the mercurial treatment earlier, she would have been saved much pain and annoyance; but I was led, from the ease with which the organ could be replaced, to think that the pessaries would be sufficient to retain it, and that it would not be necessary to subject her to confinement to bed, and the risks which must always more or less attend a mercurial course. I will never allow such considerations again to influence the treatment.

Case VII.—*Anteflexion—Enlargement—Mercurial Treatment—Relief—Pregnancy—Cure.*

May 2, 1846, Mrs M'L—, æt. twenty-seven years, mother of one child, complains of difficulty of voiding urine, pain in hypogastrium and left iliac fossa, a sense of weight and fulness about the pubis, and pains shooting down the thighs; bowels regular; catamenia regular, sometimes scanty, and again profuse; complaints commenced last January, when she had an abortion, followed by an acute attack of metritis; before marriage she suffered much from *dysmenorrhœa membranosa.*

On making a vaginal examination, the os uteri was found in its usual position, (but low down in the vagina), the limbi were large and indurated; the fundus was lying behind the symphysis pubis, large and firm, painful to the touch: the point at which the organ was flexed upon itself was at the junction of the cervix with the body.

She was ordered to maintain the recumbent position, and to take half a grain

of iodide of mercury three times daily, to have six leeches applied above the pubis once weekly. A month after the commencement of this treatment the uterus was very much reduced in size, but still remained flexed, though to a much less degree than before. All her complaints were gone; indeed she considered herself quite well, and would not submit to any manual treatment. In a few months she became pregnant. During gestation she had frequent threatenings of abortion, indeed was obliged to keep her bed the greater portion of the time; but with care she reached her full period, and was safely delivered of a healthy female child. She had no relapse of the displacement, and has enjoyed excellent health since her confinement.

Pregnancy in this instance had the effect of restoring the uterus to its normal position; the treatment, however, bringing the uterus into a condition capable of performing its functions.

ARTICLE III.—*Case of Fibro-Cartilaginous Tumour of the Femur.*

By KELBURNE KING, M.D., Port-Glasgow.

ABOUT the end of last March, J. A., aged seventy-three, consulted me regarding a feeling of pain and weakness of the upper part of the left thigh, which had continued for about a fortnight, and though unattended by any swelling or other outward appearance, rendered him unable to lean his weight on the affected limb, and consequently prevented him from taking out-door exercise. While he sat he experienced no inconvenience, but on attempting to walk, he felt as if the movement of the limb was arrested by a painful feeling in the upper and inner part of the thigh, corresponding to the insertion of the adductor longus muscle. This pain was very severe after going to bed, but was not then confined so much to one spot, being referred sometimes to the knee, sometimes to the leg, and sometimes affecting the opposite limb. It often lasted during the greater part of the night, preventing him from sleeping till towards morning. Previous to this attack, he had enjoyed good general health, with the exception of occasional slight derangement of the liver and stomach. At the commencement of last spring, he had for a short time complained of similar pain in the same part—he had taken little notice of it, and it had gradually disappeared.

On examination, nothing abnormal could be detected. There was no swelling of the part; pressure produced no pain. The thighs were muscular, the affected one as much so as the sound. In short, there was neither pain on pressure, nor swelling at any part of the thigh or hip. His complexion, however, was bad, having a dirty yellowish aspect; his tongue was red at the point and foul behind—his pulse full and rapid, usually about ninety. His appetite was but little affected, and, though his bowels inclined to be costive, he asserted that his health was perfectly good if he could only get the use of his leg.

Under the impression that his complaints were referrible to rheumatism, accompanied with derangement of the chylo-poietic viscera, stimulating applications were made to the thigh, and he was put on a gently alterative course of medicine. At first some benefit seemed to be derived, and in the middle of April he was so much better as to be able on one occasion to take a short walk in his garden. The improvement, however, was not permanent. He soon began to fall off perceptibly in flesh and strength, his pulse increased in rapidity, his look became more anxious, and his sufferings at night more severe, so that now he obtained rest only by means of opiates. Dr Watson of Glasgow saw him along with me towards the end of April; but at that time and for some weeks after, the erratic nature of the pains, and the absence of any appearance of disease in the femur (though carefully looked for), led us to consider his complaint to be of the nature of nervous rheumatism. This, together with depressing circum-

stances in his private affairs, and his advanced period of life, seemed to us sufficient to account for the general giving way of his health. After this, colchicum, hydriodate of potash, cod-liver oil, &c., were successively employed, but without procuring relief. The pain increased in severity, occurred frequently in violent paroxysms, and was not limited to the place originally most constantly complained of, being greatest sometimes at the knee and sometimes as if shooting from the back. He became totally unable to put the foot to the ground, and was constantly confined to bed. On the 15th May a careful examination of the limb was made by Dr Watson, but with no better result than before. On the 18th he suffered a succession of the severest paroxysms he had yet experienced. He could not say at what point the pain was greatest, referring it indifferently to the knee, to the hip, and to the shaft of the bone. Relief was obtained only after large doses of opium. During one of the paroxysms he declared, that he felt as if a shot had passed up the affected limb, and shortly after repeated the same remark with exclamations of alarm. The next day the pain had subsided. On visiting him, I found that his attention had been attracted by a swelling at the upper and outer part of the thigh. This, on examination, proved to be a hard osseous tumour, about the size of a man's fist, situated below and in front of the great trochanter. The limb was considerably shortened, the knee bent in beneath the other, the foot inverted; altogether very much in the position caused by dislocation on the dorsum ilii. Any attempt to move the limb caused him extreme pain, and crepitation of an indistinct and, as it were, muffled character, could be detected by the hand placed on the top of the tumour when such motion was made.

For some time previous to this remarkable change in the symptoms, I had been led to suspect the existence of deep-seated, most probably malignant, disease of some organ—which I could not determine,—by the anxious expression and saffron colour of his countenance, the constantly excited pulse, the quick progress of emaciation, and the rapid falling off of his strength; I felt convinced, after examining the tumour, that it was caused by fracture of the femur, and consisted of the upper end of the lower fragment drawn upwards and outwards by the action of the muscles. The question then came to be, how did this fracture occur? It was certainly not produced by external violence, as he had not even been out of bed for some days. It was necessary, therefore, to look for an explanation to internal disease, and, under all the circumstances, it appeared to me, that the pain which had so long existed, and whose precise seat and cause had been so obscure, had proceeded from disease existing in the medullary canal of the femur, which, at first confined, had gradually overcome the resistance offered by the bone, and now by breaking through the shaft caused fracture. This was the opinion of Dr J. R. Spiers of Greenock also, who saw the case with me the same evening. We did not think it desirable to disturb the patient by attempting to extend the limb; and our attention was after this directed to sustaining his strength by cordials, regulating his bowels, and preventing the formation of bed-sores. There was never after any violent pain. He died on the 1st June.

Two days after death I made, with the assistance of Dr Reid of this place, a post-mortem examination of the thigh. An incision was made on its outer side, from the great trochanter to the knee. The muscles, though soft, were healthy. There was no extravasation of blood, nor any appearance of disease until the bone was reached. This was found fractured about two inches below the small trochanter, in an oblique direction from behind, forward and downward. The edges of the fractured surfaces were thin, rounded off at the anterior aspect of the bone, and brittle. The diameter at the seat of fracture was slightly increased, as if expanded from within. From the cavity of each fragment protruded a firm fleshy mass of a reddish colour. On following these they were found to be connected, both above and below, to the cancellated structure of the bone, by a firm pedicle of a fibro-cartilaginous nature. Their opposite surfaces corresponded exactly, leaving no doubt that originally they had constituted one mass, which had been divided by the weight of the limb;

the moment of separation being, most probably, that when he exclaimed that a shot had passed upwards through the limb. On placing the broken surfaces in apposition, a circular portion was deficient from the anterior part of the shaft, as if eroded by the pressure of the tumour; and the anterior aspect generally was thinned away more completely than the posterior.

On cutting into the tumours they were found of a greyish colour, having the appearance and consistence of cartilage, except towards their opposing surfaces, where they were of a red colour, and somewhat softened. This, however, in my opinion, was produced by the rupture, and did not depend on malignant action. The size of each was about that of a walnut. It should be mentioned, that though surrounded by bone, they were not attached to it except by the pedicles mentioned before. There was no attempt at production of new bone, or reparation, in any shape; nor was there the slightest appearance of inflammation, suppuration, or other consequence of injury.

We were not permitted to examine any other part of the body.

Remarks.—The foregoing case appears to me interesting in various points of view. In the first place, it differed from ordinary cases of osteo-sarcoma in the absence of swelling. In this disease the bone usually undergoes a species of degeneration and disorganization, becoming expanded in its tissue, and softened to various degrees of consistence from cartilage to gelatinous substance. Here, however, it appeared rather as if the bone yielded to the pressure of a growth foreign to itself, taking its origin, indeed, within its medullary sheath, but still producing upon it no effect except the gradual absorption caused by its continued enlargement. It did not appear from the post-mortem examination, that any morbid changes had taken place in the osseous texture. It was neither softened nor expanded by disease; it was simply thinned away, and forced slightly outwards by mechanical pressure applied from its centre.

Again, there resulted from this peculiarity in the nature of the case, a striking obscurity in the symptoms. Great pain, no doubt, was referred to the part, as is usual in osteo-sarcoma; but it was not constant in the same spot, nor even in the same limb, so that its erratic nature, increase on getting warm in bed, and the previous history of the case, combined with the absence of swelling of the bone, pointed rather to chronic rheumatism than to local lesion.

I would direct attention also to the rapid progress which was made in the latter stage of the complaint. A year before I saw him he had experienced pain and weakness of the thigh; but he paid little regard to it, and indeed felt little or no inconvenience from it till two months before his death. During the first period of its duration, it must have been dormant and inactive; but after that time its rapid progress, and the severity of the symptoms, indicate an amount of activity characteristic more of malignant than of benign disease. It is to be regretted that the tumour being incautiously put into spirits, advantage could not be taken of the powers of the microscope to throw light on this subject. Its physical characters, however, as evident to the senses, were those of the simple fibro-cartilaginous growth.

I have only to remark further, and it is my principal reason for

putting this case upon record, that had the nature of the disease been known at an earlier period, the treatment adopted would have been very different. I will not stop now to inquire, whether in that case it might have been got rid of by the use of the trephine and cutting pliers, or whether the more serious measure of amputation would have been required. Had the expansion been so great as to have been felt when I first saw him, or soon after, I think a successful attempt might have been made with the former instruments; so much is clear, that the treatment pursued could not exercise any beneficial effect on the disease which actually existed, whilst, had we possessed the means of making an accurate diagnosis, a chance of relief would have been afforded to the patient.

ARTICLE IV.—*Observations on the Employment of Chloroform in Dental Surgery.* By FRANCIS BRODIE IMLACH, Dentist, L.R.C.S.E.

(*Read before the Medico-Chirurgical Society, Edinburgh, July* 5, 1848).

EARLY in November last, I extracted the first tooth ever drawn from a patient placed under the anæsthetic influence of chloroform. Since that period I have, in my practice as a dentist, used it now above 300 times, and without one single accident or drawback of any kind. And I have at this time been induced to lay before the Society the results of my experience in regard to its use, from having heard repeated accounts of the alleged total failure of this agent in dental surgery; and that sometimes in the hands of men of extensive practice.

Let me merely premise, that I have tried chloroform manufactured by different chemists; but that supplied by Messrs Duncan and Flockhart is what I have found most pure, and most easy of inhalation. It is therefore that which I now constantly use.

I do not know if I have seen all the instruments that have been invented for the purpose of administering chloroform; but I have seen a very great number indeed, many of them very ingeniously contrived, chiefly with the object of preventing the loss by evaporation of chloroform; but in general they are so arranged, as to fit over the nose and mouth of the patient, and prevent his breathing a particle of air before it has passed through the apparatus—a point in which, I believe, there rests an insuperable objection to the use of all ordinary instruments; and I have not seen a single one that I could be induced to use in preference to the method first recommended by Professor Simpson, namely, that of a handkerchief.

It has the recommendation of being perfectly simple, convenient and easy of application, does not frighten the patient; and, what is best, it most effectually combines every requisite for perfect administration.

Some dentists among us are in the habit of placing two or three

plies of cotton wadding within the folds of the handkerchief, for the purpose of preventing the chloroform from evaporating so rapidly as it otherwise would do, and keeping what they do employ concentrated in one small spot. Certainly they may, in this way, succeed in rendering their patient insensible with a minimum quantity, provided that he is obedient enough to breathe in the exact way that is desired. If, however, the patient is not very manageable, he will not rapidly become anæsthetic by breathing from so small a surface; and then the operator, finding it difficult enough to make him insensible, is induced to place the handkerchief nearer and nearer the face, and perhaps close upon it. Then, here is a sufficient reason why the patient should begin to show what are called bad symptoms, not because the chloroform is bad, or the patient a difficult subject, but because he feels a great sense of choking, and very naturally tries to rid himself of that sensation by rolling the head backwards and forwards, speaking, throwing up the arms, trying to get from off the chair, or, in fact, by any other means that his half-drowsy sensibility can stimulate him to. In a very nervous patient, the mere touching him with the handkerchief whilst he is inhaling, is apt to excite, by recalling his sensibility to what is being done; and, as the first stage of chloroformization is a state of excitement, great care requires to be taken so as to get a patient of this sort past this stage as quickly as possible; and, having him safely past it, you may then touch him freely without any danger.

Perhaps the Society will allow me to enumerate a few simple and easy rules which I have always found useful, and to which I confidently attribute some of the success which has always attended the exhibition of chloroform in my hands.

Firstly. I settle the patient in an easy and comfortable position, with the head supported by some firm object. I am also in the habit of requesting him to close his eyes, in order to avoid the irritation of the chloroform vapour, and to prevent any accidental mental or physical excitement, as from the sight of an amusing object, or the glare of too bright light. Of course, all noise of every kind is strictly prohibited during the inhalation, and I think it advisable to warn him against being alarmed by extraordinary noises, as ringing in the head, or by flashes of light before the eyes, or a feeling of giddiness, or vertigo.

Secondly. As to the dose. I am always in the habit of pouring a large quantity upon the handkerchief, and diffusing it over a surface larger than will cover the nose and mouth. The quantity I never measure, as I judge by the effects and not the dose; but I believe I seldom begin with pouring, at first, less than three or four drachms upon the handkerchief. In fact, I pour upon it a quantity sufficient to moisten completely the required surface. Many of the supposed bad effects, doubtlessly, result from giving a small, imperfect, and consequently mere exciting, dose.

Thirdly. The mode of administration. The chloroform is poured

upon a thin white cotton handkerchief. An old one answers the purpose remarkably well. This I hold at first at the distance of two or three inches from the patient's mouth, and allow him thus to take two or three deep inhalations. I then approximate it more closely to the face, but never bring it into close contact. In this position the handkerchief is continued till the anæsthesia supervenes. In judging of this point, I depend upon no single circumstance, and the indications are of such a varying nature as cannot be described, but are easily perceived after some experience in the practice.

Fourthly. The moment the handkerchief is removed, I apply the instrument (kept ready in my hand), and at once proceed to extract the tooth. If several teeth are to be extracted or punched, or if I require to change the instrument in use, it may be necessary to recommence the inhalation for a short time. This readiness at the proper time, and the rapid and immediate extraction, I consider the chief secret of success.

Fifthly. The great mass of patients, on becoming insensible, have their jaws so clenched, that there is no possibility of opening the mouth, and the operator keeps working away, tugging at the under jaw, or pressing upon the angle of the jaw externally with his knuckles, trying to force open the mouth. This, again, is easily remedied, by the very simple method of never allowing the patient to close his jaws at all, by placing a small gag of ivory or gutta percha between the teeth, before administering the chloroform. No patient objects to it, and it causes him no inconvenience whatever.

A patient of my own, who is in London at present, went a week or two ago to one of the first dentists there, to have three or four stumps extracted. He placed nothing between her teeth, had her insensible three or four times, but never succeeded in getting out any of the stumps, as sensibility always returned before the jaws could be forced open. This is a circumstance that takes place day after day, and is the cause of many dentists not using it at all in their operations. Now, such a palpable oversight as this is set down as one of the difficulties of using chloroform, when to me it seems to be one of its principal advantages; namely, that you can have your patient's mouth kept open whilst you extract one, two, or half a dozen teeth if required, and then you slip out the gag, and the patient is left to recover his sensibility, without your having almost required to touch him except in seizing the tooth. Certainly, the patient can be made to inspire chloroform till the muscles of the jaw become relaxed; but then, this causes him to inspire a vast deal more chloroform than what is requisite for keeping him insensible whilst extracting the tooth, and this plan also requires a proportionately longer time before he recovers his sensibility. I have seen a patient begin to inspire, become insensible, have the tooth extracted, and return to perfect consciousness in little more than a minute.

The other day I was called out, along with Professor Simpson, to see a patient, a young lady, who, we were told, could not be made

insensible previously to having a tooth extracted, and would not submit to the operation without anæsthesia. The gentleman who had seen her before we were called in, had been half an hour trying to put her over, and had not succeeded. Professor Simpson poured some chloroform upon the handkerchief, held it to her face, but, mark what she did to prevent herself becoming insensible—she first kept in her breath as long as she could, then took short inhalations, so as to take as little fresh air as possible into her lungs, and, whenever she felt herself pressed for want of air, she suddenly tossed her head aside, pulled away the handkerchief, and immediately took two or three full inhalations, so as to refresh herself, and fill the lungs with pure air; and then she was willing to allow the handkerchief to be replaced before the face. Dr Simpson at once discovered the trick; and the next time that she attempted to repeat the same actions, he prevented her pulling the handkerchief down, and followed her face with it as she turned it from side to side, thus compelling her to inhale the chloroform whilst appealing to her mother, who was present, to be allowed to speak for a moment or two. Of course she was allowed to speak as much as she pleased; but it was only for a few seconds that she did so, as the vapour inhaled during the act of speaking, was sufficient to affect her rapidly, and in the course of a minute or two at most she was completely insensible, and the tooth taken out.

Now, I required to see this young lady again professionally, and she attempted the same trick with me; but I acted as before, held her against her inclination firmly in the chair for a few seconds, and she was put over as rapidly as formerly. In a case of this sort, the patient inhales a great deal more than is requisite to make an obedient patient insensible, and on this account it is always well to pour an excess of the chloroform upon the handkerchief. I have remarked some of my patients become sick upon recovering from a dose of chloroform; but almost all those cases arose from an overloaded stomach at the time of the operation.

I have also seen them sick where, on inquiry, I found that they had tasted nothing for four-and-twenty hours previous; but then the whole system was deranged, from the fasting, irritation, and want of sleep consequent on a severe attack of toothache.

I have seen a few cases, perhaps three or four, in which the patients have been slightly hysterical; but, on the other hand, I have not had a single patient faint in my house after any operation performed under its influence, whilst before, I often had them lying on my sofa in this state, or in a state approaching to it. Besides all this, the patients used to come into the dentist's room with their faces pale, their lips quivering, their mouths parched, and with every symptom of terror depicted on their countenances. Now, on the other hand, they seem to think it a good joke to have a tooth taken out; and many a tooth I have taken out because the patient could get it done without any pain. Again, I have seen the circumstance

of the respiration ceasing for a few seconds, happen in several instances, and I now always wait till it recommences, and allow the patient to take again a few inhalations before operating.

There is another class of patients who are apt to display symptoms that might cause a little difficulty to the operator, in so far as to render him not able to distinguish whether they are insensible or not—I mean sleep-walkers. I have administered it to several of them, and in most of these cases the patients whilst insensible had the same appearances, and the same remarkable character of gesture and countenance, as are commonly attributed to sleep-walkers. I always find that the eyes of somnambulists gradually open as the chloroform takes effect, and that, when they are insensible, the eyes are fixed and wide open. Generally the patient attempts to get up, and if you are not a little dexterous, and catch hold of the tooth without touching, and consequently irritating, him, there is a great probability that the head, body, or arms, will be moved in such a way as to prevent you from operating. Now, in cases of this sort, do not touch the patient, or try to steady his head, for, if you do, you assuredly will not succeed in your object; let him alone, and in the course of a very few seconds he will become quiet again. Then seize the opportunity to place the handkerchief nearer his mouth, so as to make him a little deeper, and then, by avoiding the previous error, there is little doubt but that he will remain quiet long enough to have his tooth extracted.

I may here state, in addition, that I believe an over-dose of chloroform, and the consequent perfect relaxation of all the muscles of the mouth and tongue, is a great impediment to easy operating. The tongue in this condition doubles up like pulp, or a bladder, around the finger or instrument, and prevents the surgeon seeing and seizing the tooth.

Inattention to the great variety of temperaments that are met with, and the neglect of the little niceties of manipulation requisite for what may be called the polite administration of chloroform, is one great reason why so many dentists fail in managing their patients easily and successfully. I have often heard of patients not being of a habit fit for the administration of chloroform; and I have often heard of patients who, it was said, could not be brought under its influence; but I have never met with any of this sort. I failed several times to render patients insensible when I first commenced the use of it; but I am convinced that it was not from the patient's habit, but from my not being aware that it affected people in such a great variety of ways; and I am also convinced that, if other gentlemen fail in easily rendering their patients insensible, it is from the very same reason that perplexed me, or perhaps from employing an improper article, and not from the temperament of the individual.

It is well known to the Society, that in the practice of the dentists of Edinburgh, chloroform is habitually and generally employed. I

believe, however, that very few gentlemen of my profession in the larger towns in England use chloroform before extracting teeth, from the difficulty of obtaining a pure article. In Liverpool especially, I understand what is manufactured is so bad (causing nausea, tightness of breathing, headache, and other disagreeable symptoms), that the use of it is almost abandoned by the profession; and I am not surprised at such a result, for the greater quantity of chloroform manufactured there is made with naphtha, the smell of which distinctly remains in the preparation. I have sent up a good deal of what is made in this town to be tried there, and I am informed that it is a totally different article from what they have been attempting to use, and that the effect of ours is such as they were led to expect from the accounts published by Dr Simpson.

Now, I hold that chloroform is not accountable for patients being made sick and disgusted with all those mixtures that bear its name; and till gentlemen are careful, and do not use any except the pure article, we shall have many accounts of patients being riotous, sick, and ill, and, consequently, of medical gentlemen temporarily abandoning the use of it.

One very curious effect that I witnessed whilst administering chloroform to a patient whose intellect was very much impaired from some affection of the head, so as to leave her imbecile, was, that whilst under the influence of it, her eyes being open, her countenance lost the vacant expression habitual to it; and she looked round in my face with an intellectual expression, so calm and so rational that I was perfectly startled. Whilst gazing at her, however, the effects of the chloroform passed off, and she awoke with the same unmeaning laugh and vacant gaze that she had before. I mentioned this circumstance to my friend Dr Simpson the day that it occurred, and I thought of trying what effect the habitual use of the chloroform might have upon the patient by stimulating the brain so as to assist the cure; for this patient, I was informed by the parents, is gradually, though slowly, recovering the use of her faculties; and we do not know but that, by stimulating the brain, it might materially assist in the recovery; but I must leave this for others better acquainted with affections of this sort than I am, and recur to my own department.

As to the method I follow whilst my patients are recovering from the anæsthetic state, it is this—At first I allow the patient to remain quiet for half a minute or so, so as not to excite him—then the mouth being open (for it generally remains so after the gag is removed) I put a napkin quietly to it, to prevent the blood running down and destroying any part of the dress, and then merely fan the face briskly for a short time. The patient then opens his eyes, laughs, says, "Is it out?" or "That's very odd!" and the whole scene is finished by his getting up and saying, that he will never suffer toothache again when he can get quit of it so easily. The advantage of the use of chloroform to the operator is as great as to

the patient, as he can take more time to his operation than he formerly used to do when he knew that his patient was in an agony of anxiety, with all his courage screwed up to keeping his mouth open, his head steady, and his hands convulsively clutching "the horrid chair." Now, on the other hand, the scene is changed; and patients have even professed themselves to me to be glad to have toothache for the apology of getting a dose of chloroform.

In conclusion, I would beg to remark—

1st, That out of above 300 cases of dental surgery in which I have employed chloroform, I have never seen the least deleterious effect from its use, but the reverse.

2d, I have seen or traced no after bad consequences of any kind whatever.

3d, I have seen no case in which I have been afraid to give, and where I have not given it quite successfully.

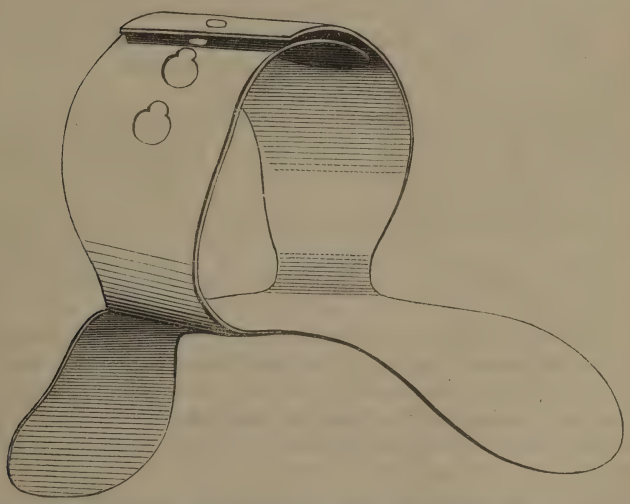
4th, It saves the patient's present physical suffering and previous struggles of feeling.

5th, It enables the dentist to perform his work with more satisfaction, certainty, and success.

6th, Patients, who have once had a tooth drawn under the influence of chloroform, invariably demand the repetition of the chloroform on requiring again the same operation.

ARTICLE V.—*Note on the Mouth Speculum.*

By ALEXANDER FLEMING, M.D.



I FIRST saw this useful instrument about two years ago in the wards of M. Rayer, at La Charité, Paris. It consists, as the annexed

wood-cut shows, of a circular band of metal (German silver) to keep the mouth open, while a broad horizontal plate, passing backwards, serves to depress the tongue. It facilitates greatly all operations on the mouth and throat, and is especially useful in children. By its aid, ulcers on the fauces may be cauterized much more accurately and more efficiently than without it. It is made of two sizes, for children and adults; and each speculum can be varied in size, to a certain extent, to adapt it to individual cases. Charriére of Paris has constructed a mouth speculum on the principle of his speculum vaginæ. It is considerably more expensive than the simpler one here shown; but has this advantage, that the same instrument can be enlarged or diminished in size to suit all ages. Specimens of both forms may be seen at Mr Young's, cutler, Edinburgh. As the mouth varies much in length in different persons, I have suggested to Mr Young, that in future he should make the tongue portion of two pieces, which, by a simple mechanism, may be so arranged as to admit of that portion being lengthened or shortened, as occasion may require.

Part Second.

REVIEWS.

On the Nature and Treatment of Stomach and Renal Diseases; being an Inquiry into the connexion of Diabetes, Calculus, and other Affections of the Kidney and Bladder, with Indigestion. By WILLIAM PROUT, M.D., F.R.S., Fellow of the Royal College of Physicians. Fifth Edition, revised. London: 1848. 8vo, pp. 595.

Urinary Deposits, their Diagnosis, Pathology, and Therapeutical Indications. By GOLDING BIRD, A.M., M.D., F.R.S., Fellow of the Royal College of Physicians, &c. Second Edition. London: 1846. 12mo, pp. 356.

Lectures on the Urine, and on the Pathology, Diagnosis, and Treatment of Urinary Diseases. By JOHN ALDRIDGE, M.D., Lecturer on Chemistry at the Medical School, Park Street. Dublin: 1846. 8vo.

Annuaire de Thérapeutique, de Matière Médicale, de Pharmacie et de Toxicologie, pour 1841, 1842, 1846 (*Supplément*), *et* 1848. (*Mémoires sur le Diabète Sucré ou Glucosurie.*) Par le Dr A. BOUCHARDAT, Agrégé de la Faculté de Médecine de Paris. Paris: 1848. 32mo. *Therapeutic Annual, &c.* By Dr A. BOUCHARDAT. (*Memoirs on Diabetes.*)

SOME of the works placed at the head of this article are probably so well known to our readers as to render a detailed criticism or

analysis a superfluous task on our part. This is particularly the case with the excellent work of Dr Prout, which has now attained such a European celebrity as to have scarcely any claim for notice from the reviewer; and the appearance of a fifth edition of which is a sufficient evidence of the estimation of the public. But the additions of late years to the sum of our knowledge of urinary diseases, are so interesting both in a scientific and practical point of view, that we have seized the present opportunity of bringing before the notice of our readers some of these, which scarcely appear to have received the consideration they deserve, either in Dr Prout's treatise or in any other systematic work that we have seen on the subject. We shall, therefore, occupy the greater part of this article with an analysis of such of these researches as appear most important; and to this end we shall dismiss, in as short a space as possible, the critical part of our office, by noticing in a few words the characteristics of the works before us.

In the present edition of Dr Prout's book, the body of the work presents little or no difference which can be justly called *original*, from the edition of 1843. The principal additions are in Chapter III. of Book First, where the views of Drs Johnson and Simon, on the pathology of certain renal diseases, are incorporated into the work, chiefly by means of large extracts from their papers, illustrated by some of the plates from the former author's memoir. A number of facts are also added to the account of the chemical constitution of the urine, selected chiefly from the memoirs of Golding Bird and Bence Jones, and from Liebig's late researches on the chemistry of food, which the author seems to value more highly than most of that chemist's productions. In the notes throughout the work there are numerous additions, the principal object of which is the support of the author's original views in points in which they appear to have been superseded or endangered by the researches of more modern chemists. Indeed, the effect of Dr Prout's acquaintance with such researches in modifying his own views, may be expressed in his words as applied to Liebig in the preface to the fourth edition:— "Some of the views" (of Liebig) "are the same as I have long advocated. Others are directly opposed to mine, and seem to me neither susceptible of proof, nor even probable."

From the whole tenor of the present edition, as well as the last, of Dr Prout's work, it is evident that the increasing cares of practice, and constant mental occupation arising from this cause, have driven him in a great measure from the field of experimental science, of which he was formerly so distinguished and successful a cultivator, into a somewhat too narrow and exclusive dogmatism. It is not possible for us to conceal from ourselves, that his theories have assumed in his mind that fixed position which is referred to by Bacon as one of the chief sources of the *Idola Tribus*, or *delusions of all humanity;* viz. where facts are assimilated to the theory, instead of the latter being modified by the facts; and where the mind, easily moved and

excited by affirmative instances, allows the negative to drop without sufficient investigation. But while our duty obliges us to ascribe to a truly great man some of these common errors of humanity, let us not omit to say, that those theories, as well as the most elaborate and praiseworthy experiments on which they were originally founded, have in many of their most cardinal points been corroborated by further experience, and remain not only among the noblest ornaments, but also among the most valuable lights of the present age. While we cannot accept Dr Prout's work as the expression of the *whole* of modern science on the subjects of which he treats, we are still of opinion that the student will find in it not only a collection of most instructive practical facts, but a mass of sound and irrefragable principles, in relation to the processes of secretion and assimilation, such as, amid the fluctuating opinions of modern writers, he will not easily acquire from any other source.

We have on a former occasion referred to Dr Golding Bird's treatise in terms of the highest praise; and constant reference to it since that period enables us to renew our testimony to its general accuracy, and to recommend it as by far the best guide to the examination of urinary deposits.

The work of Dr Aldridge has been for some time before the public, and we regret that our intention of noticing it has not been sooner carried into execution. It consists of a series of six lectures, giving a succinct and clear summary of the most important facts in reference to the chemistry of the urine, and the pathology and treatment of urinary diseases. The work is also characterised by many very original and striking views, some of which will be noticed hereafter. It is exceedingly creditable to the author, that while chemical details have occupied so much of his attention, he has not allowed the pathological anatomy of the urinary organs to fall into that secondary place which it occupies in the works of many chemical writers. In this respect the lectures of Dr Aldridge are diametrically opposed to the work of Prout. The former author is more of what we might call a *solidist*, the latter is essentially a *humoralist*. The former wishes in every possible case "to connect the functional disorders with the organic lesions on which they depend," or at least always "to define the groups of symptoms to which may be philosophically given the name of a distinct disease." He accordingly tends to look upon all urinary derangements as the result of some definitive lesion of the glandular or excretory apparatus, or some recognized morbid constitution of the system, such as the rheumatic, gouty, scrofulous, &c., acting upon the urinary organs. On the other hand, he rejects in great part those functional disorders of assimilation, which make up nearly the whole sum of Dr Prout's philosophy, and of which, according to him, the alterations in the urine are most frequently only the secondary results. Thus he considers the terms *phosphatic* and *lithic diathesis* as leading to erroneous and unphilosophical views of disease; and represents strongly, by way of

analogy, "the obscurity and confusion which would result from sitting down to prescribe for a rusty sputa diathesis, or a mucous expectoration diathesis," in a case of pulmonary disease.

Although we have already expressed our high admiration of the spirit which has actuated Dr Aldridge in setting pathological anatomy before him as a guide in the investigation of urinary diseases, we must object at once to the above analogy, and to the doctrine which suggested it; to the analogy, because it depends on an imperfect apprehension of the word *diathesis;* to the doctrine, because it cuts the gordian knot which binds together all facts having reference to the functions of assimilation and secretion, which knot it was the endeavour of Dr Prout's book honestly to untie. While we admit that Dr Prout looked too exclusively to the aliment and the blood for a solution of the phenomena of urinary diseases, and paid too little attention to the vitiated condition of the secreting organs themselves, we contend that, on the other hand, an equally exclusive spirit is manifested, and equal or more glaring errors are unavoidable, when it is attempted to confine the sources of urinary derangement within the comparatively narrow range of pathological alterations, to which the urinary organs themselves are subject. That this has been a frequent source of error in Dr Aldridge's work we shall be prepared to maintain, when we examine his views in detail; meanwhile, as it is important that our meaning should be understood *in limine*, let us analyze for a moment the illustration drawn from pulmonary diseases.

For the due performance of the chemical function of respiration, it is sufficient that atmospheric air be brought in contact with the pulmonary mucous membrane. The conditions, therefore, of the efficiency of this function are the integrity—1st, Of the pulmonary circulation; 2d, Of the air passages; 3d, Of the mechanism by which respiration is carried on. These conditions entitle us to look for the source of the greater portion of respiratory derangements in obstructions of the air-passages either by foreign bodies from without, or by exudations from the mucous membrane itself; and of these obstructive exudations "mucous expectoration" is known by experience to indicate disease in one locality, and "rusty sputa" disease in another locality. Hence experience teaches us to consider rusty sputa and other abnormal expectorations not as the evidence of a *diathesis*, but as being the concomitants of grave symptoms of respiratory embarrassment, and, along with these, the proof of lesion of the respiratory organs.

With the urinary function the case is different. The formation of a perfectly normal urine implies a wide series of conditions, among which the most essential are—1st, A right digestion and formation of chyle; 2d, A formation of normal blood; 3d, A right secretion by the skin, liver, and all other secreting organs; 4th, Complete and perfect respiration; 5th, A due degree of waste of all the tissues, and a correct proportion of this to the amount of supply.

Not only, therefore, is it evident that many temporary derangements of the urinary secretion must exist which cannot be considered with propriety as the evidence of disease at all; but also that habitual alterations of that fluid may and must be produced wherever there is habitual mal-assimilation, or indeed continued derangement of any of the important functions above mentioned. Such derangements of the urinary secretion are the *physiological compensation* for errors of diet, or disease of other organs, and cannot, with any propriety, be introduced into the list of urinary diseases.

To take an example. If, from suppressed cutaneous secretion, excessive animal food, rheumatism, or any other cause, an individual becomes affected with habitual secretion by the kidneys of an excess of lithate of ammonia, which thus physiologically compensates for the imperfection of other functions, this derangement, though habitual, is not a urinary disease; it is merely a diathesis, indicative of disease or error in some more or less remote function. This diathesis might indeed become the cause of disease in the urinary organs, should it lead to the formation of a calculus in the kidney, and give rise to irritation there; but in its simple form it is a reparative not a destructive agency, and, in relation to the kidneys, denotes a physiological, not a morbid condition.

We are by no means disposed to regard the alterations of the urinary function as being less dependent on the integrity of the organs allotted to it than those of any other function of the human body. But the renal diseases, properly so called, have yet to be studied in relation to a new pathological anatomy; and the followers of Bright and Rayer have not yet brought their inquiries to any very satisfactory conclusion. In the mean time, the great idea of the mutual dependence of the processes of secretion and assimilation, the origination of which by Prout formed one of the greatest and most practical discoveries of our age, has been worked out in a variety of forms, and to some of these we propose in the present review directing the attention of our readers. We here bring to a close, therefore, these somewhat lengthened preliminary remarks, and approach the subject of

DIABETES MELLITUS.

Semeiology and Diagnosis.—The general symptoms and distinguishing characteristics of diabetes have been very completely described by Prout, Bardsley, and Christison; and comparatively little has been done by more recent writers on this branch of the subject. There is, indeed, little difficulty in distinguishing this disease; the great thirst, the inordinate appetite, the suppressed cutaneous exhalation, the decline of strength and flesh, and the excessive quantity of the urine, scarcely ever failing to excite attention, while its saccharine character, and increased density, may be considered as completing the diagnosis. It is important to know, that according to Dr Golding Bird (*Lond. Med. Gazette*, 1844), the presence of

sugar in the urine can frequently be recognised long before any increase of density has taken place, and before any characteristic symptom has occurred; a circumstance which has also been observed by Dr Prout in dyspeptic individuals.

The very simple tests which have lately been placed in the hands of the practitioner, for the detection of small quantities of sugar in urine, facilitate very much the clinical study of diabetes, especially in its early stages. We believe, that of all the tests which have been proposed for this purpose (an account of the best of which will be found in Dr Bird's work, p. 291), that proposed by Mr Moore is at once the most convenient and accurate. It consists in boiling the suspected urine with half its volume of *aqua potassæ*, when a brown colour, more or less deep, is readily developed in a fluid containing even a very small quantity of grape or diabetic sugar.* This test is better adapted for general use than those of Trommer or Capezzuoli, which depend on the de-oxidation of the hydrated oxide of copper; as these tests, though very minute, and in general accurate, are subject to the objection, that occasionally a somewhat similar reaction is produced by apparently normal urine.—(See *Monthly Journal*, June 1844, p. 564.) The test proposed by M. Biot, depending on the power of saccharine solutions of producing a deviation in the rays of polarized light, is stated by M. Bouchardat to be very accurate, and to be capable, by means of a formula which he has given, of indicating the amount as well as the existence of sugar. We believe this test to be worthy of more attention than it has received in this country. For practical purposes, however, the quantity of sugar may be sufficiently easily estimated by the well-known method of Dr Christison, which consists in confining a portion of urine in a graduated tube, over mercury, and causing it to ferment by the addition of yeast; when every 47 cubic inches of carbonic acid gas discharged will be found to correspond to 45 grs. of sugar.

Pathology.—On the pathology of diabetes we have to record numerous new facts and opinions of the greatest importance. Beginning with those views which appear to have a sufficient demonstrative or experimental basis, we shall find that diabetes has been, by the progress of science, completely removed out of the pale of mere urinary disease; and that the light shed upon the primary assimilative processes of late years by the inquiries of Blondlot, Bouchardat and Sandras, Magendie, Bernard and Barreswil, have, more than any other circumstances, tended to elucidate this obscure affection. We are aware that the idea of diabetes as an assimilative disease is by no means new, and may indeed be said to have guided English pathology since the time of Rollo; but the complete proof and satisfactory

* M. Bouchardat employs milk of lime, which darkens the saccharine urine in the same way as potass, and is sometimes more readily accessible.

application of this idea are of much more recent origin. We shall endeavour to give our readers a clear view of the elements on which the case rests.

The first decided advance in the direction we have indicated was undoubtedly caused by the publication of the papers of Mr Macgregor of Glasgow (*Med. Gazette*, May 1837). By showing that the abnormal saccharine ingredient is not confined to the urine, but exists also in the saliva and fæces of diabetic patients, the supposed connexion of the disease with a special renal disorder was rendered very improbable; and by the confirmation of the experiments of Ambrosiani and Maitland, as to the existence of sugar in considerable quantity in the blood of diabetics, the morbid element was traced still nearer its source. The experiments of Macgregor, however, were especially valuable in relation to the examination of vomited matters in the healthy and diabetic states, and lead directly to the conclusion, not only that the sugar of diabetes is found in great quantity in the stomach, but that even in healthy assimilation a small quantity of the same substance is produced.[1]

In addition to the experiments on sugar, Mr Macgregor adduced evidence to show that the quantity of urea (which had been originally supposed by Prout to be the source of the sugar in diabetic urine), is not in reality diminished, and that the sugar is, therefore, in all probability an *added* constituent, instead of a product of decomposition of any of the elements normally present.

From these researches, therefore, it appears—1st, That the production of sugar in the stomach during digestion to a small amount is a natural process; 2d, That this ingredient is formed by diabetics in the stomach to an abnormal extent; 3d, That the existence of sugar in the blood to more than a very small extent, is peculiar to the diabetic state; 4th, That in diabetics the abnormal sugar appears in the form of an added constituent in the fæces, saliva, and urine, being probably carried out of the system chiefly by the latter secretion. The increased flow of urine would thus seem to be a secondary result, the consequence of the presence of large quantities of an abnormal ingredient in the blood.

In the last two editions of his work Dr Prout has stated very clearly the theoretical views which appear to spring from the researches above mentioned, although he appears to be ignorant of the further experimental confirmations which these have received, and which we shall presently relate. As the quantity of sugar which exists in healthy blood is insignificant, and as sugar is frequently ingested in considerable quantity with the food, it is clear that there must exist in the organism *a faculty of converting the saccharine*

[1] It appears, by the recent researches of Magendie, that in animals fed on feculents a small quantity of diabetic sugar or *glucose* is found normally in the blood, though not enough to affect the secretions.—(See *Comptes Rendus*, 1846.)

principle into some of the elements of the blood. This faculty or function must be of great importance, and its derangement or destruction is the real source of diabetes, which is therefore a derangement of the secondary rather than the primary assimilating processes.

This conclusion is, we think, fundamentally the same as we shall presently show to have resulted from more enlarged views of the assimilative processes, and more precise experiments.

One of the most important results of recent investigations into the phenomena of digestion, is the establishment of the important fact, that the secretions poured into certain parts of the alimentary canal have the power of converting fecula or starch into *glucose*, a form of the saccharine principle identical with diabetic sugar. For the full development of the details on which this fact rests, we are indebted to various inquirers, each of whom individually appears to have taken too exclusive a view. The saliva was indicated several years ago, by Leuchs and Burdach, as the chief agent in the solution of the feculents, which, they maintained, were converted by it into sugar. These views were more recently taken up by M. Mialhe, who, in March 1845, read to the Academy of Sciences in Paris a memoir, in which it was satisfactorily shown that the saliva, as taken from the mouth, contains a principle very similar in its properties to *vegetable diastase*, and possessing in a high degree the power of that agent in converting starch into saccharine matter or glucose. Having ascertained this fact, M. Mialhe sprang with characteristic eagerness to the conclusion, that the saliva is the sole fluid which has the power of digesting the feculents,—a conclusion which was shown to be incorrect by M. Bouchardat, who had previously (in January 1845) read a memoir to the Academy, proving, by experiments on animals, that it is chiefly in the duodenum and small intestine that this change is effected. M. Bouchardat admits the accuracy of M. Mialhe's observations with respect to the saliva; but he has succeeded also in obtaining a *diastase* from the pancreatic fluid, and in proving that the fluid obtained by digesting the fresh pancreas of an animal in water has a strong reaction upon fluid starch. His experiments further show, that in animals which feed upon crude fecula, that substance is little affected in the mouth or even in the stomach, the saliva not having the power of rupturing at once the external membrane of the starch vesicles, which therefore remain intact till after they have remained a considerable time in contact with the fluids of the alimentary canal. M. Bouchardat further established, negatively, that neither the bile, nor the gastric fluid in its normal state of acidity, have the power of acting on the feculents; and he concludes, therefore, that in health the principal digestion of these substances takes place in the duodenum and small intestine, under the influence of the pancreatic fluid; and that it is only where the fecula forming the food of the animal has been previously cooked, that the fluids of the mouth have the power of acting on it.

These conclusions of M. Bouchardat are supported by a series of

experiments which we cannot give at length, but which appear to us to be quite conclusive as to their general accuracy.—(See *Annuaire de Thérapeutique*, Supplément 1846; Mémoires sur la Digestion des Féculents et sur les Fonctions du Pancreas.) Nevertheless the subsequent experiments of MM. Bernard and Barreswil deserve notice. They find that the power above ascribed to the saliva and pancreatic fluid belongs also in a marked degree to the gastric secretion, *provided that the acid naturally existing in that secretion be neutralized by an alkaline carbonate;* and conversely that the saliva and pancreatic fluid, *on being treated with an excess of acid*, acquire the power, formerly supposed peculiar to the gastric secretion, of digesting and dissolving the albuminous or protein compounds. It would thus appear that, with respect to the feculents and the protein compounds, the three fluids above mentioned exert a common digestive power, which is modified in each by the circumstance of their acid or alkaline reaction.[1]

M. Magendie carries this generalization still further. In a memoir read to the Academy of Sciences (*Comptes Rendus*, 1846), he states that most of the composite secretions of the animal economy possess the power of converting starch into glucose. It is still more remarkable that the blood appears to possess this power in a very considerable degree; for, on injecting a solution of starch into the veins of an animal, the blood soon after gave evident traces of glucose.

To sum up the above physiological digression, it appears that the digestion of feculents into glucose takes place normally in the upper parts of the alimentary canal wherever the reaction is alkaline; while the solution of protein compounds occurs in the presence of an acid reaction. It follows that the latter process takes place in the normal state in the stomach; while the former, begun to a very slight extent in the mouth, is mainly carried on in the duodenum, under the influence of the pancreatic secretion. This conclusion corresponds perfectly with the results of Bouchardat's experiments on animals above alluded to.

We have been thus particular in explaining the facts in regard to the digestion of feculents in the normal state, because a clear view of this subject is, we conceive, quite essential to the right understanding of the pathology of diabetes. According to M. Bouchardat, (who has made by far the most extensive and satisfactory series of observations on this disease of late years) diabetes is invariably dependent on an abnormal digestion of the feculents *within the stomach*, instead of lower down in the canal. This fundamental fact he has, we think, proved, not only by numerous observations on the contents of the stomach during digestion in diabetes as compared with

[1] *Comptes Rendus*, vol. xiv. M. Bernard has lately ascribed to the pancreatic fluid the power of dissolving the fatty acids. (See *Retrospect* for July, No. 244); but this has no relation with the present subject.

healthy individuals (which observations agree in the main with those of Macgregor before noticed); but by showing that the fluid from the empty stomach of diabetics has, *even when acid*, a power not possessed by the normal fluid, of converting starch into glucose.—(*Annuaire*, 1848, p. 229). M. Bouchardat considers this peculiarity due to the presence of a diastase, which is absent in the healthy stomach, and which is secreted there in virtue of a vicarious action, the pancreas being, in his opinion, incapable of performing its office in diabetes. This last point, however, is by no means well made out, depending chiefly on a single observation of diseased pancreas in connexion with diabetes.

The effect of the formation of glucose in the stomach is, according to M. Bouchardat, its immediate escape into the blood by the veins in quantities too great for secondary assimilation. Thus arises the necessity for its being carried off by the kidneys. Of this the diuresis is a secondary effect; while the intense thirst is in consequence of the large amount of water required to convert the feculents swallowed into glucose. The habitual low temperature of diabetic patients is explained by the large quantity of cold water drunk, and the loss of caloric which occurs from the conversion of large quantities of starch into sugar.

The most defective part of M. Bouchardat's theory is his explanation of the phenomenon, that sugar when formed in the stomach has more tendency to pass into the blood unchanged than when formed in the duodenum and jejunum. He ascribes this difference to the influence of the portal circulation, which he considers as exerted solely on matters absorbed from the duodenum, forgetful that the venous system of the stomach, like that of the rest of the alimentary canal, is connected with the circulation in general only by means of the vena portæ. This singular oversight makes it necessary to look at this part of the subject with other eyes than those of M. Bouchardat.

The secondary assimilation of saccharine substances, or their conversion into the normal elements of the blood, has undoubtedly much to do, as Prout has supposed, with the pathology of diabetes. But the whole of this subject is still enveloped in considerable obscurity. M. Bouchardat's own experiments show (Sur la Digestion des Sucres, *Supplément de l'Annuaire*, 1846), that when given in very large excess to animals, sugar is in part decomposed in the stomach into lactic acid, but is found in considerable quantities in the blood and bile, and in smaller (sometimes doubtful) in the chyle of the thoracic duct, and in the urine. From its large quantity in the bile, as well as from some other considerations, on which we shall not dwell at present, it is not improbable that, as M. Bouchardat supposes, the liver is endowed with a peculiar power of separating the superfluous or noxious substances which enter the circulating system. That it has the power of converting glucose into the elements of the blood, however, we see no reason to believe, and are

therefore at a loss to know how sugar should be transformed in the portal any more than in the general circulation. We incline to ascribe the conversion or secondary assimilation of sugar to the mesenteric glands, or some part of the lacteal system. It is clear that the lacteals have the power of selecting from the contents of the intestine certain nutritive elements, and, on the other hand, of rejecting substances unfit for assimilation; thus, in M. Bouchardat's experiments (*Annuaire* 1846, *Suppl.* p. 89), saffron and prussiate of potash having been introduced with the food, the chyle was found to contain neither of these substances, while the blood contained both. Experiments are still wanted to determine the composition of the chyle in the lacteals as compared with that in the thoracic duct; but in the mean time it appears to us very probable, that the lacteals, along with oleaginous and albuminous matters, absorb glucose from the intestines; and that this latter substance is decomposed in the mesenteric glands into other elements, thus accounting for its absence, or presence in very small quantity, in the thoracic duct. It is worthy of remark, that alkaline fluids have, even out of the body, a power of acting slowly upon glucose, which gradually disappears when dissolved in water along with alkaline carbonates. —(*Op. cit.* p. 100.)

If this speculation be correct, then the real difference between diabetic and normal assimilation is probably the following: In the former, the glucose produced in the stomach is subject, in the first instance, to venous absorption *only*, and in a situation where we know that venous absorption very readily takes place; and accordingly the blood becomes loaded with unassimilated saccharine matter. In the latter, on the contrary, the glucose produced in the duodenum is at once rapidly absorbed by the lacteals, and none, or at least a very small quantity, passes unchanged into the circulation. This small quantity, if present, is probably either eliminated by the liver in the healthy individual, or undergoes a slow destruction in the blood, which, from the free alkali present, possesses the same power as the alkaline solutions above adverted to.

Such is, we believe, a faithful summary of what is really known, or plausibly supposed, upon the subject of the pathology of diabetes. Did our space permit, we should advert to the ingenious, though we believe erroneous, theories of other authors; as it is, we can only advert in a few words to the views of Mialhe and Dr Aldridge.

M. Mialhe, the opponent on some points of Bouchardat, maintains that in diabetes the natural alkaline reaction of most of the animal fluids is either destroyed or very much impaired. He asserts that he has found the blood of diabetes neutral, or verging on an acid reaction; Bouchardat again denies this, alleging that the blood of diabetics is, like other blood, alkaline when drawn from the vein, but that it has a tendency to become acid when kept, from the presence in it of glucose, which is converted into lactic acid. In other respects the views of M. Mialhe are mostly hypothetical.

Dr Aldridge considers the sugar in diabetes to be formed in the urine, together with ammonia, by the decomposition of the albumen of the blood. "Albumen is formed in vegetables," he says, "by sugar combining with ammonia, and the mixture losing a certain quantity of water and oxygen. If albumen is formed by the union of sugar and ammonia, it is plain that the two latter compounds may be formed by the decomposition of albumen." However ingenious and plausible this chemical theory may be, it is sufficiently answered by the fact, that the sugar of diabetes is *in the blood ready formed;* a fact which Dr Aldridge should have known. This theory of diabetes is eminently subject to the objection which we have made against the general principles of the work which contains it; that of attempting to localize in the kidneys diseases which have their primary seat in other organs.

Treatment.—The treatment of diabetes by specific remedies, and with a view to its entire cure, is a subject which is exceedingly apt to be used by the enemies of medicine as one of its *opprobria.* In this, as in other departments of our art, it is most true, that extraordinary and unfounded theories have always produced a miserable fruit of useless, or worse than useless, remedies; so that, in the words of Dr Prout, "perhaps there is no disease in which so much mischief has been done on false principles, and by random experiment," as in diabetes. Still it is undeniable that there are few diseases in which so much good may be temporarily effected by proper hygienic and dietetic regulations, aided by a skilful and not too zealous employment of medicines. Of the number of diabetics who enter our hospitals, few perhaps leave them without a decided amelioration in the more obvious symptoms. A few, perhaps, are even apparently cured; but their return at no distant period is still counted on by the enlightened physician with the same degree of probability as in a case of phthisis or Bright's disease.

The conclusion, therefore, has been growing in strength, and is now almost inevitable, that while the ultimate cure of diabetes is rarely accomplished, and is not to be compassed *directly* by the resources of art, the circumstances which destroy the comfort, and shorten the life of the unfortunate patient, are capable of being to a great extent modified or controlled. The treatment of diabetes in this sense, we hold to be one of the triumphs of enlightened pathology and scientific medicine; for in this case, as in all others where great results have been attained, science and practice have exercised a mutual influence; and while experience has corrected unsound theory, sound theory has held the torch to experience, and led the way to judicious and successful trials.

It will reasonably be expected from these remarks, that the treatment of diabetes shall have been found to correspond, in some degree, with the improved views of its pathology, which we have endeavoured to enunciate. So much has this been the case, that, even at the risk of being considered too exclusive, we shall set aside in a great

measure the host of empirical or questionable remedies which have been proposed, and proceed at once to the detail of the treatment which we conceive to have the firmest basis both of theory and experience.

The main feature of this treatment has been recognized since the time of Rollo; and is simply the exclusion from the aliment of all substances capable of producing sugar or glucose, which in diabetes is known to be imperfectly assimilated. It is in the application of this method, however, that recent writers, especially Bouchardat and Prout, have chiefly modified the method laid down by Rollo. The exclusively animal diet which, up to a recent period, was generally considered all-important in the treatment of diabetes, was so insupportable to the majority of patients, that it was with extreme difficulty, both in hospital and private practice, that a sufficient strictness of regimen was obtained. Nevertheless, the results obtained by those who subjected this method to the severe trial which was necessary, showed that a power was in this way obtained over the disease which had been till then quite unknown. But independently of the difficulty of carrying it out at all, the animal diet was found to have its disadvantages. It was found that diabetics, instead of having, as was supposed, stomachs all-powerful for the digestion of animal food, were readily injured by indigestible substances; that cheese, butter, fat, therefore, which had been contemplated not only as additions to the diet, but as forming important and even essential parts of it, had often to be discarded, from their not agreeing with the individual idiosyncrasy, which it was discovered required quite as much attention in diabetics as in any other invalids. Hence it was found, that though, when an animal regimen could be maintained with an ordinary degree of care, the morbid symptoms referrible to the urine were readily kept under command, the advantage to the general health was not always commensurate with the apparent success of the treatment; and physicians, as well as patients, were constantly induced to break through the iron rule of Dr Rollo, from the conviction that an exclusively animal diet was apt to be hurtful in its own fashion, and to produce in the patient a disgust for food, which could not but be very injurious.

From the difficulty, and even danger, in some cases, of following out the exclusively animal diet, various modifications of it have been allowed by different practitioners; most of these, however, cannot be considered as any more than a compromise of expediency between the desires or even necessities of the patient, and the rigid system before referred to. It is to M. Bouchardat, so far as we are aware, that the merit belongs of first enunciating and carrying out the principle, *that the dietetic treatment of diabetes depends not on the suppression of vegetables in general, but of saccharine and farinaceous matters in particular.* This principle, which is perfectly in harmony with the theory above detailed, has been submitted by M. Bouchardat to an extensive trial. Forty-five cases of the disease have now

been recorded by him, observed with great accuracy and precision, and several of which have been under his observation for many years. The results are, 1*st*, that where the saccharine and feculent elements can be completely excluded from the food, the abnormal quantity and character of the urine can *always* be completely controlled;[1] 2*d*, that by persistence in this system, and attention to other hygienic regulations which we shall presently mention, the health of the diabetic patient may often be re-established, and a *virtual* cure effected; 3*d*, that in this state the morbid habit is often so far subdued, that a very moderate amount of feculents may be used for a time without any bad consequences, provided a constant attention be given to the state of the urine, and the regimen be resumed upon the least threatening of a relapse. It does not appear certain, from an examination of M. Bouchardat's cases, that a cure, *in the strict sense of the term*, has been obtained in any instance. In some cases, indeed, the tendency to relapse, even under a regimen by no means absolute, appears not to have been observed for a very considerable period; and there are good grounds for hoping, that in such individuals the state of apparent health may be prolonged indefinitely; but strict attention to hygienic regulations, and extreme moderation in the use of feculents, appear to have been necessary, in all instances, to this immunity; and several cases of relapse after considerable intervals of health, prove that the security of such persons is not to be considered as perfect.

Let us not, however, in stating the fundamental principle of M. Bouchardat's treatment, fall into an error, which he himself repeatedly warns us against; that of looking to the restriction of the diet alone as the means of cure of the diabetic condition. Admitting that the urine may be thus rendered normal, it must be recollected that there is a perverted cutaneous function to restore, a bodily and mental debility to remove, a tendency to secondary disease to be assiduously guarded against. To fulfil these indications, it is imperatively necessary, 1*st*, that the patient be clothed in warm flannels, worn next the skin, and over the whole body; 2*d*, that he take exercise in moderation; 3*d*, that he use baths; 4*th*, that his aliment be (within the prescribed limits) generous and varied; 5*th*, that a certain quantity of wine, especially claret (which has been found superior to all the other wines in France), be allowed; 6*th*, that these hygienic circumstances be assisted, if necessary, by tonics, stimulants, and diaphoretics, especially opium.

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[1] Two cases have been recently published by Dr Todd (*Provincial Journal*, June 28, 1848), which, while they show in the most marked manner the favourable influence of a diet restricted with respect to feculents, appear in some respects to be opposed to the results obtained by Bouchardat. We think, however, that a minute analysis of these cases would prove their apparent results not altogether unexceptionable.

The only medicine to which M. Bouchardat appears to ascribe any thing like a specific power, is the carbonate of ammonia, given in doses of from one gramme (15 grains) to five or even ten grammes daily. In this drug he has a considerable degree of confidence as an adjuvant remedy; and he prefers it most decidedly to the bicarbonate of soda, so much vaunted in diabetes by M. Mialhe, whose theory we have already considered. Whether these remedies be justly praised or not we have no means of determining, but we can readily believe, with M. Bouchardat, that the treatment of diabetes by therapeutic means alone involves a most dangerous omission; and we suspect that the theoretical treatment of M. Mialhe, unless supported by a much larger amount of experience than he appears to have had, will not obtain many supporters.

We cannot avoid noticing here, that the large experience of Dr Prout appears to have been conducting him insensibly into the same path with M. Bouchardat. "It will readily be understood," says Dr Prout (p. 39), "that an animal diet ought to form an essential part in the treatment. I do not, however, approve of a diet exclusively animal; but consider a certain proportion of vegetable matters proper. The vegetable matters to which I particularly allude, are the green parts of plants. With regard to farinaceous matters, I have lately prohibited them altogether in the confirmed stages of diabetes." The "farinaceous matters" which *in the fourth edition* were recommended in this place as the most eligible form of vegetable food (see p. 40), are here prohibited; and in their place are substituted green vegetables, which form a most prominent part of the "aliments qui doivent être permis" of M. Bouchardat. Indeed the dietetic treatment of diabetes by Dr Prout, laid down in the present edition of his work as the result of his latest experience, appears to correspond in all essential particulars with the principles sketched above. It may be in part the result of this new system, that whereas in 1843 he had seen "*a few*" only, he has now (1848) seen "*many* cases in which the saccharine quality of the urine has for the time entirely disappeared; and a very great number of cases in which the symptoms have been so far subdued as to give little trouble to the patient" (p. 57).

As regards the details of treatment necessary or advisable, according to M. Bourchardat's method, we must refer to the Supplement of the *Annuaire* for 1846; we shall merely add a few words principally on the dietetic and most important portion. The list of proscribed articles is as follows: "Bread, whether of wheat, rye, or barley, &c. (or oats); pastry; rice, maize, and other grains; radishes; potatoes, and their starch; arrow-root, and other forms of starch; vermicelli, semolina, maccaroni, and all similar farinaceous pastes; the leguminous seeds, haricots, peas, lentils, beans; chestnuts; buckwheat; all confections, and other sugared meats and drinks."

Almost all other articles of food are allowed, with due regard to the digestive powers of the individual. Variety is encouraged, and

every description of cookery allowed; so that even a diabetic epicure need not fare very ill. The longing after feculents, and particularly after bread, however, is generally very strongly felt; and, in order to gratify this taste, M. Bouchardat employs a bread made exclusively of gluten of wheat, from which the starch has been carefully removed by a stream of water. (The method of preparing gluten bread is described in a separate memoir, *Supplément pour* 1846, p. 311.) This bread, which is now regularly manufactured in Paris for the use of diabetics and dyspeptics, is quite digestible, and soon comes to replace the more ordinary article. It is not, as many have supposed, an essential part of the treatment of diabetes in M. Bouchardat's hands; but merely a substitute for ordinary bread, which may be employed without injury, if the desire of the patient for it is strong. Gluten may likewise be made into various dishes.[1]

Fermented liquors are not allowed, on account of the dextrine they contain; but considerable importance is attached to alcoholic stimulants, such as wine, rum, and brandy, taken in moderation. Every excess, however, is to be avoided. Coffee is generally allowed, but must be taken without sugar, or, at least, with very little. Acid liquors, such as lemonade, are prejudicial; but Seltzer water, or such waters as Spa and Vichy, may be taken with advantage. The great thirst, however, complained of by diabetics, is found to be uniformly in proportion to the quantity of feculents consumed; and the regulation of the diet is usually sufficient to control it.

Of all the other precautions to be used in diabetes, the greatest importance is ascribed by M. Bouchardat to the diaphoretic regimen. The use of flannel, indeed, is with him an imperative regulation, subject to no exception. Not only is it useful in restoring the cutaneous transpiration, but in guarding against the supervention of the pulmonary disorder, to which all diabetics are constitutionally liable. Its effects are to be aided by baths, exercise, and the occasional use of opium in the form of Dover's powder, or of the antiquated theriac, the stimulant and diaphoretic properties of which make it a useful preparation, in spite of its strange composition.

We cannot in this review enter into an individual consideration of the numberless remedies which have been employed in diabetes, either purely empirically, or from some species of guiding principle. Astringents, and particularly alum, were among the oldest remedies employed in this disease; and have lately been recommended both by continental and British writers, particularly by Dr Aldridge. M. Bouchardat, while he does not deny their efficacy altogether, appears to have seen many cases in which no good effects followed; and in this respect we think he expresses the common opinion.

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[1] Dr Prout recommends, as a substitute for bread in diabetes, a compound of bran, eggs, and milk, which is made into cakes, and baked in a quick oven. For the process, see p. 44. Fifth Edition.

Notwithstanding the strong recommendations of Watt, blood-letting has never been employed indiscriminately by judicious practitioners in general. By some it has been used to a considerable extent, when the pulse and increased heat indicated a degree of febrile reaction in the early stages of the malady. The temperature of diabetics, however, is more commonly below than above the average; and, considering the tendency which uniformly exists in this disease to death by sudden or gradual sinking, as well as to all the diseases of debility, we cannot but assent to the opinion of M. Bouchardat, that general blood-letting ought to be most sparingly employed, and never as a part of the ordinary treatment. Local blood-letting, in the form of leeches to the anus, he has sometimes employed, when the disease has succeeded to the suppression of a hemorrhoidal discharge; but his protest is most strong against excessive depletion in every form, unless employed with a special object.

We cannot conclude without urging upon practitioners in this country a fair and just trial of M. Bouchardat's method in diabetes. We consider that method to be founded upon just and legitimate inductions; and we believe the cases by which it is supported to be the fruit of sound and matured observation, and very different from the flimsy evidence upon which new therapeutic methods not uncommonly rest. On this point our convictions are strengthened by the consideration, that this method involves no radical or sweeping change upon those whose long-tried efficacy is all but universally admitted; but is more of the nature of a selection from those methods, based upon sound data both of science and experience.

The length of the present review precludes our entering, at present, further into a consideration of the important subjects we had in view in commencing. In a future Number, however, we shall probably recur to some of these. W. T. G.

A Practical Treatise on the Diseases peculiar to Women; Illustrated by Cases derived from Hospital and Private Practice. Third Edition. By SAMUEL ASHWELL, M.D., M.R.C.P.L. Late Obstetric Physician and Lecturer at Guy's Hospital. London: 1848.

IN noticing this third edition of Dr Ashwell's treatise, we cannot but express, in general terms, a very high opinion of its value as a practical work. Coming from a man of reputation, and based upon a vast and varied experience in the diseases treated of, it demands the serious attention and study of all inquirers in the field of the diseases of females. At the same time, we feel bound to state, that the book comes very far short of our expectations, inasmuch as it has no pretensions to keeping pace with the progress of science, or maintaining its position in regard to similar works, as for instance Dr Churchill's excellent treatise. On various subjects—particularly in

diagnosis and treatment—which we could cite, the well-confirmed opinions of the profession are set at nought in an unseemly manner, or no mention of them is made.

The easy and natural arrangement of this class of diseases into the two orders of functional and organic, is that adopted by our author. In discoursing on the functional disorders, he has, however, carried out this separation and distinction to a length that is not justified by a sound pathology. Still, his chapters on chlorosis, amenorrhœa, vicarious menstruation, dysmenorrhœa, menorrhagia, leucorrhœa, hysteria, irritable uterus, &c., are very valuable in a practical point of view.

Of the second part of this work, upon the organic diseases of the genital system, much cannot be said in the way of commendation. His chapters on organic diseases of the os and cervix uteri, and on the diseases of the internal genital organs, will be consulted by all with great advantage. We are sorry our space prevents us entering into details, and noticing some of the errors into which Dr Ashwell has fallen in this part of his work; and we can merely state our conviction, that on further study our author will find many changes demanded in his chapters on tumours of the uterus, on the treatment of pregnancy complicated with organic disease, on displacements of the uterus, &c. &c., changes which we shall look for in his next edition.

On the Cure of Cataract, with a Practical Summary of the best Modes of Operating (Continental and British). By Hugh Neill, Surgeon to the Liverpool Eye and Ear Infirmary. Liverpool: 1848. 8vo. Pp. 224.

The author has undertaken to present the reader with an "analytical review of the leading contributions of eminent continentalists on the cure of Cataract" (Preface, p. 2). Among others, the treatise of Desmarres,[1] whose clinique on diseases of the eye is at present the most instructive in Paris, receives a lengthened analysis. We have been familiar with this work since its appearance in 1847, and fully agree with Mr Neill in the high opinion he entertains of it. We regret to find, however, that Desmarres's opinions are most imperfectly rendered in the present work. Indeed, his meaning is often completely misunderstood by Mr Neill, who has so mixed up his own opinions and experience with those of Desmarres, that it is impossible for the reader to say what belongs to the one, and what to the other, unless, for his guidance, he adopt the plan—which he may with considerable safety—of referring all the sense to Desmarres, and all the nonsense to his reviewer.

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[1] Traité Théorique et Pratique des Maladies des Yeux, par L. A. Desmarres, M.D. Paris: 1847. Pp. 904.

After a very unfair statement of the relative merits of extraction and reclination of the lens in cataract, he expresses a decided preference for the latter. In gaining experience for the operation of extraction, he maintains the absolute necessity of the operator's first "putting out a hatful of eyes," a statement utterly devoid of truth; inasmuch as we have repeatedly seen the operation of extraction performed, by pupils for the first time on the living subject, with complete success. The following extract affords a fair specimen of the author's style, which is always homely, often inaccurate, and generally very confused. He is describing what is termed "the author's (Mr Neill's) process" of reclination :—

The operator, while he fixes — say the patient's *left* eye with his (the operator's) left hand—the process is done thus: and I am supposing that the speculum is not used. The second finger raises the upper eyelid, and fixes it against the eyebrow. The first finger depresses the lower lid. The eye is thus firmly enough fixed: its outer portion is free to be punctured, and there is no impediment to a thorough inspection of the pupil. A *flat* straight needle, with two cutting edges running to a point, is carried, at the eighth of an inch from the cornea, towards the very centre of the eye through the sclerotic. The needle must not be poked into the lens. As soon as it is judged to be behind the *iris*, it should be gently forced to the anterior of the lens, and posteriorly to the iris, *forwards* toward the pupil. Do all gently. Let there be no abrupt or sudden motion. The point of the needle seen in the pupil is now to be depressed, and *pressed down* the face of the lens to its lower edge, and carried back *below the lens* right into the vitreous humour. The needle is then moved horizontally backwards and forwards, so as to cut a free course in the vitreous body for the passage of the lens. All this is a *continuous*, momentary, and essential movement. Attempted otherwise, a laceration of the vitreous capsule is not easily accomplished; and should the membrane be tough, and not lacerated, the lens may be turned topsy-turvy, and tilted into the anterior chamber.

The needle having made its way through the capsule of the vitreous humour, now comes back to the anterior of the lens. Commence pressure with it, to cause displacement, by resting heavily on the anterior portion of the lens. The lens now moves downwards. The needle presses down also, and the lens begins, technically, to *recline:* that is to say, it turns backwards.

The needle ought to get on the (now) top of the lens; and away it goes, launched as it were into the vitreous humour. Its position is supine, offering its whole flat surface opposed to the vitreous humour. It cannot rise up; its flatness is also opposed to the retinal portion of the vitreous humour.

Such is my favourite operation for the cure of Cataract by Reclination—not as hitherto described, confusedly or with needless elaboration in books. In homely language, I will pit hundreds of cases of *Reclination* which I have successfully performed, against the very best cases of *Extraction;* and the eyes of my patients shall show as little injury surgically produced, *aye and less*, than the optics of those who have had the rare good luck to have been "touched off" by the most dexterous "Extractor."—p. 184.

Leaving the reader to discover Mr Neill's peculiar property in this operation, we pass to the concluding chapter of the book, entitled "Compendious Remarks illustrative of the Inhalation and Use of Chloroform and Æther." As the application of these agents in ophthalmic surgery is nowhere alluded to, we do not see what object Mr Neill could have in introducing this chapter, unless it be

to afford him an opportunity of preferring the following *modest* claim :—

As I have put in some claim as an humble pioneer towards its complete discovery—a discovery which unquestionably took place in America—I will state on what grounds I rest my title to be considered as having aided that consummation. Eight years ago, I published a work on the ear ; and in 1841 the same work was published in New York, Philadelphia, and *Boston*. I gave a regular diagram of an apparatus for the administration of Æther by the *eustachian tubes* (air-passages from the throat) for the cure of some forms of deafness, and for the relief of *painful affections* of the inner ear by the induction of *Æther-vapour*. Well, from Boston, in 1846, we have a reflex importation (matured certainly to a climax, entitling the party to the just credit of a complete and triumphant discovery)—to the effect, that Æther-vapour will not only soothe pain, but also produce complete insensibility to pain.

It is strange if a thousand copies of the fourth edition of my little work, printed by Mitchell and Company, of Liverpool—regularly shipped and entered through the Customs, and extensively circulated in America, from the towns specified in its title-page—it is strange that such a pamphlet (telling of the very agency, and declaring the efficacy of Æther-vapour in relieving pain), should have been five years in Boston, and yet that its suggestions apparently should not have been seen—at any rate not acknowledged—by the new discoverer.

But we must now dismiss Mr Neill's book, in regret that the rich field of observation which the author enjoys had not fallen to one capable of turning it to better account. We really hope, for the sake of the Liverpool public, that the motto which he is so anxious to assume, "*Deus nobis hæc otia fecit*" (see Dedication), may be speedily realized in a sense which he appears at present not to contemplate.

Scriptural Authority for the Mitigation of the Pains of Labour by Chloroform, and other Anæsthetic Agents. By PROTHEROE SMITH, M.D., Member of the Royal College of Physicians, &c., London : 1848. 8vo. Pp. 52.

WE regret the cause of the appearance of this pamphlet, while, at the same time, we recommend it to our readers, as being executed in an honest and worthy spirit towards those whose scruples it is intended to remove. That such scruples should have arisen with the public, especially on this side of the Tweed, among minds strongly preoccupied by peculiar theological ideas, and little accustomed to practical or logical considerations, we were prepared to hear; but that these views should be shared extensively by men whose practical energies are daily employed, whether chloroform be used or not, in the "mitigation of the pains of labour," and indeed pain of every sort, does indeed strike us with unaffected surprise. The whole question is, to our view, admirably disposed of, as far at least as the profession is concerned, by the following argument of Professor Simpson, in a letter to the author of this pamphlet, which is by no means its least interesting part :—

The accoucheurs and surgeons among you who object to the use of chloroform, on the ground that it goes, in their opinion, against the object and end of the primæval curse upon woman, strangely forget that the whole science and whole art and practice of midwifery is, in its essence and object, one continuous effort to mitigate and remove the effects of that curse. By warm baths, aperients, regulated diet, &c., they attempt to destroy the intensity of the approaching pains and penalties of childbirth;—during labour, they use counter-pressure on the back, to relieve the intense pains there; they use unguents, perineal support, venesection, &c. &c., to ease the pains and insure the safety of the mother. * * * To relieve our patients, however, by our interference, a little, and a little only, is assuredly, in a moral and religious point of view, just as sinful as if we succeeded in affording them complete relief from suffering. The *principle* of interference is not altered by the *degree* of relief afforded being more or less, greater or smaller.

Littell's Living Age. Reprint of the Report of the Trustees of the Massachusetts General Hospital; with a History of the Ether Discovery, and Dr Morton's Memoir to the French Academy. Edited by R. H. DANA, Jun. March 1848. Boston. 8vo. Pp. 46.

WE notice this pamphlet simply to place on record the conclusion arrived at by the authors of the report, viz. that to Dr Morton belongs the honour of having discovered the power of ether to prevent the pain of surgical operations. Those who are curious to know all the particulars of the painful controversy between Drs Jackson and Morton, will find abundance of detail in the pamphlet before us. The authors have spared no effort to obtain accurate information, and appear to have discharged their duty most faithfully. We may therefore consider this disputed question as definitively set at rest.

Outlines of Lectures on the Nature, Causes, and Treatment of Insanity. By SIR ALEXANDER MORISON, M.D., &c. Edited by his Son THOMAS COUTTS MORISON. Fourth Edition, with Plates, and much Enlarged. 8vo. London: 1848.

THE work of Sir Alexander Morison on insanity is well known, and we can confidently recommend this fourth and improved edition as a work in every way well calculated for those who are commencing the study of the subject. It is written in a peculiarly easy and interesting style, and, without being prolix, contains a full description of the different forms of insanity. Some of the sketches at the end of the volume are very characteristic.

We are of opinion, that the subject of mental diseases is one which deserves the careful consideration of the histologist, and that great fame is to be attained by any one who, with a good previous knowledge of the tissues, will patiently examine into the state of the ultimate structure and composition of the brain in cases of insanity. All our present notions of the morbid anatomy of madness must be

fallacious, for what can be more absurd than speculating about the changes of a tissue, the components of which are so minute as to be invisible to naked sight. Great caution and much labour are required, however, to arrive at trustworthy results, and no one ought to undertake the task who is not at the same time an accomplished pathologist and a skilful microscopic observer.

A Treatise on the Practice of Medicine. By GEORGE B. WOOD, M.D., &c. In two volumes 8vo. Philadelphia: 1847.

THIS work consists of two large volumes, giving an excellent summary of the prevailing opinions and orthodox rules concerning the practice of physic. We do not expect that the doctrines of modern pathology can as yet be brought to influence our treatment of disease, and still less that men of experience should adopt them into their systematic works. For this, as for all good changes, time is required. In the mean time, we can say that Dr Wood's work exhibits great judgment, and may be safely consulted by the student and junior practitioner.

Part Third.

MEDICAL NEWS.

EDINBURGH OBSTETRIC SOCIETY.

MEETING V.—(*Continued from* p. 134.)

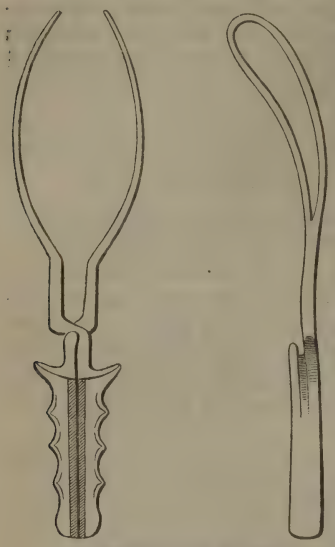
ON THE MODE OF APPLICATION OF THE LONG FORCEPS. BY PROFESSOR SIMPSON.

WHEN the head of the child becomes fixed in the brim of the pelvis, and the powers of the uterus fail in propelling it through that aperture, one of two modes of instrumental delivery is usually resorted to,—namely, either 1. Diminution of the infant's head by perforation and craniotomy; or, 2. Extraction of it by the long forceps.

Craniotomy is the preferable operation where the child is dead, and where the pelvic brim is much contracted, or the child's head very much above the average size. But besides being always necessarily fatal to the child, craniotomy is an operation by no means without danger to the mother;—one in every five mothers (according to Dr Churchill) dying, upon whom it is performed. And in most schools of midwifery, it is now, consequently, much more rarely practised than heretofore. Still many authors and practitioners, as Drs Collins, Johnson, Lee, &c., appear to resort to it in all cases in which the bulk of the head is unable to pass the brim, and reject entirely the other alternative of the long forceps.

The long forceps afford in these cases the best chance of life to the child, and when used with proper views of their mechanism and mode of application, their employment, as is often seen in Edinburgh practice, is by no means so difficult and dangerous as some authorities seem to believe. Misapprehensions on these points are perhaps the cause why they are not more frequently used. They differ from the short forceps in some points of construction, but more particularly in regard to their mode of application and working. They differ for example in their length; in the shanks being parallel for some distance beyond the lock (an indispensable point in order to prevent them injuring the outlet); in their blades being curved; and in the part intended to embrace the

head being sufficiently long and large. The instrument which Dr Simpson has frequently and successfully used (See *Woodcut*) during the last five or six years, has some additional peculiarities. The blades are the same as Dr F. Ramsbotham's, but scarcely so much curved. The lock is Smellie's, but with knees or projections above it of such size as to prevent the blades readily unlocking in the intervals between the pains, thus giving it the fixed character of the locks of Levret and Brunninghausen's instruments, without their complexity. The joints are made so loose as to allow of their lateral motion and overlapping to a very considerable degree, thus facilitating their introduction and application. And, lastly, the handle is that used by Naegele and other German accoucheurs, viz. with transverse knees or rests below the lock for one or two of the first fingers of the right hand to drag by, the long forceps being only properly used as an instrument of traction, not of compression. In addition, the handles are grooved and marked on the anterior side, to distinguish that from the other side when the blades are within the pelvis. The following are some of the dimensions of Dr Simpson's forceps:—Length of the entire instrument, 13¾ inches; length of handle, including the lock, 5¼ inches; length of shank from the lock to the commencement of the curve of the blade, 2¼ inches; length of the blade, 6¼ inches; extreme breadth of blade at 1½ inch distance from the point of the instrument, 1⅜ inches; length of the fenestra, 4½ inches; extreme breadth of the fenestra, 13 lines; breadth of the groove of the lock, ⅜ inch; thickness of the shank to fit in the groove of the lock, ⅜ inch; extreme distance between the blades at 3 inches distance from the point of the forceps when locked, 3 inches; distance between the points of the two blades when locked, 1 inch.



Dr Simpson's Long Forceps.

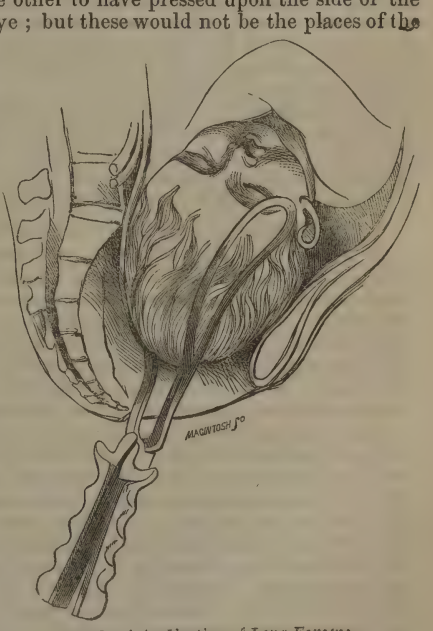
In their modes of application, the long forceps differ from the short. The short forceps are applied always to the lateral surfaces of the child's head, in whatever position the head may be. Generally, the long diameter of the head lies in the right diagonal diameter of the pelvis; and, consequently, the short forceps are placed in the opposite or left diagonal diameter; or, in other words, at right angles to the long diameter of the head. The mode in which the long forceps ought to be applied, and are really applied in practice, has given rise to considerable difference of opinion.

If the long forceps are ever for inertia, hemorrhage, or other such complications, in any case applied while the head is passing through the brim, and the brim and head are natural in size, the instrument may be perhaps applied, like the short forceps, directly to the lateral surfaces of the child's head. But the common reason for the employment of the long forceps, is morbid contraction of the brim of the pelvis in its most general form, and from its most general cause, viz., in the conjugate or antero-posterior diameter, from projection forward of the promontory of the sacrum. How are the long forceps applied when used in this, the case in which they are most generally had recourse to in practice? It is first requisite to state, that under this complication the child's head is found situated in the brim, with its long or fronto-occipital diameter lying in the transverse diameter of the brim, or with the forehead looking to one ilium, and the occiput looking to the other. In other words, the long diameter of the head is not placed, as usual, in the right diagonal diameter of the brim, but

more in its transverse ; for where the promontory of the sacrum forms a morbid projection, the transverse forms the longest diameter of the brim, and, consequently, the one in which the child's head comes to be placed by the uterine efforts. The face or forehead looking to the ilium, and the occiput to the other ilium, the lateral surfaces of the child's head come to be compressed between the protruding sacral promontory and the interior of the symphysis pubis. Now, in seizing the head in this case, some authors aver, that,

1*st, The blades of the long forceps are placed, as in applying the short forceps, on the lateral or aural surfaces of the child's head*, and consequently with one blade in front of the sacral promontory, and the other behind the symphysis pubis. Burns, Dewees, &c., speak thus of applying the long forceps in the conjugate diameter of the brim ; and Dr Churchill has published a woodcut representing this as the actual method of their application in practice. But its application in this position is impossible in the very cases in which the long forceps are generally required, viz., where the conjugate diameter is contracted, for there is not room for the additional thickness of the blades of the instrument ; if applied, they add to the thickness of the head in that one diameter and place in which it is already too thick and large ; their pressure would greatly endanger the urethra and bladder in front, and the soft structures placed over the promontory of the sacrum behind ; and they could not thus be placed in the axis of the brim in consequence of the pressure of the perineum upon the instrument below. Other authors aver, that,

2. *The blades of the long forceps should be placed over the occiput and forehead or face of the child, and consequently in the transverse diameter of the brim.* This is the view of their mode of application taken by Deleurye, Davis, &c. &c., and approaches much nearer the reality than the former opinion ; but that it is not strictly true, is shown by the marking of the place of application of the blades of the instrument after the child is born, and by a more attentive consideration of the mechanism of such labours. One blade is found to have been placed behind one ear, and the point of the other to have pressed upon the side of the forehead, temple, or region of the eye ; but these would not be the places of the markings of the blades if they were applied in the transverse diameter, upon a head placed directly transverse. Dr Ramsbotham has published a beautiful plate of the mode of application of the long forceps, and has given an excellent chapter on the subject in his work on midwifery. He correctly represents in the plate the anterior blade as placed upon the side of the forehead and eye-brow ; but in order to give this view with the forceps placed in the transverse diameter of the brim, he has been obliged to represent the face as turned backwards, whilst in reality, in morbid contractions of the conjugate diameter of the brim, it is actually turned laterally; and he places the long diameter of the blades of the forceps so as to traverse the right oblique instead of the left oblique pelvic diameter.



Mode of Application of Long Forceps.

3. *The blades of the long forceps should, I believe, be placed*

obliquely upon the child's head,—one, the posterior, over the side of the occiput; and the other, or anterior, over the side of the brow or temple, and consequently should be situated in the oblique diameter of the brim. (See *Woodcut*).—The markings on the child's head after birth always show this mode of application of the instrument: when properly applied upon the mother, and when their situation relative to the pelvis is examined, they are found to have assumed this position; and in experiments with the instrument (when the head of a dead child is fixed in a pelvis with a contracted brim), this is the position and relation which the instrument will be seen to assume with relation to the infantile head and maternal pelvis. Besides, in thus placing the instrument, while we incur less danger of injuring the urethra and other important parts, we place the blades of the instrument in exactly those parts of the pelvic circle where there is least pressure, and consequently most room for them. It is apparently in consequence of misconception on this point, that some authors have come to prefer the use of the perforator to that of the long forceps. Dr Collins, for example, argues that when the head is detained in the pelvic brim, the brim "measuring little more than three inches from pubis to sacrum," there cannot possibly be space for the long forceps even were the bones denuded, seeing that the blades of the smallest sized forceps used in Britain, even when completely closed, measure from $3\frac{1}{8}$ inches to $3\frac{1}{2}$." "How (he adds) is it possible with the forceps to drag a child through a pelvis where there is not space, except by force, to introduce, as is commonly said, a straw, or where the smallest flexible catheter cannot be passed in some instances into the bladder?" These and such opinions proceed on the erroneous idea, that the long forceps are to be applied, within the pelvis, at the parts or in the diameter in which the pelvis is *most* contracted, and they suppose that the head when fixed in the pelvic brim fills completely the *whole* circle of the brim. The usual shape of the morbidly contracted pelvic brim is cordate, or rather elliptico-cordate, but the child's head is not of this shape; it is ovoid, and consequently when applied to the cordate brim leaves unoccupied spaces. The most unoccupied spaces before and behind are at the extremities of the oblique diameters of the brim, where sufficient room is left for the passage of the blades of the forceps, and in these points they are passed when properly applied.

[The preceding observations were illustrated by various experiments with the forceps, and by showing its mode of application upon a dead child's head placed in a contracted pelvis. A number of cases illustrative of the use of the long forceps were mentioned by Dr Simpson, Dr Moir, and other members; and the cases in which turning was preferable pointed out.]

CHLOROFORM IN A CASE OF PUERPERAL CONVULSIONS. BY DR CUMMING.

Dr Cumming related a case of puerperal convulsions coming on after delivery. The patient was a young girl, seventeen years of age, who had been seduced; but, during her short residence in the Maternity hospital, had not been observed to be peculiarly depressed in spirits, or otherwise affected in mind. The labour, all circumstances considered, was unusually short and easy; all the three stages having been completed in six hours. She was delivered at four A.M. At nine A.M. the first convulsion occurred. Venesection was attempted by the house-surgeon; but, in consequence of the resistance and convulsive struggles of the patient, unsuccessfully; the other usual remedial measures, however, were put in operation. I saw her about one o'clock, along with Dr Ziegler and others. She was then with considerable difficulty bled at the arm to sixteen ounces, and as much more was taken away from between the shoulders by cupping; the other remedial measures were also persevered in so as to affect the pulse and general hyperemic condition very decidedly, but with no improvement in the convulsive seizures, which recurred at frequent intervals. During the intervals she was occasionally sensible, so far at least as to indicate that she understood what was said to her. Professor Simpson saw her in the evening, and recommended further depletion by cup-

ping, which was done, but without any effect in rendering the convulsions less frequent or less severe. It was then suggested that chloroform should be administered. The effect of its application was speedy and decided. Though she survived six days, every effort on the part of the system to throw itself into convulsion was checked with ease; so that nature was not so soon exhausted as it would otherwise have been, and other remedies could be applied. It should also be mentioned that the interval of most perfect consciousness was after the chloroform had been applied for some time. Appearances were at this time so favourable as to encourage the hope that she would struggle through. Dr Cumming in conclusion remarked, that, though the issue of the case was unfortunate, nothing could be more satisfactorily established than the power of the chloroform to subdue convulsions, and thus, even when it did not (as in other reported cases it appears to have done) exercise a direct beneficial influence over the disease itself, it prevented the excitement and consequent exhaustion of the system, and admitted of the application and administration of such measures as the case otherwise indicated.

Dr Thatcher stated that he had observed that patients who, when not pregnant, were liable to epilepsy, were frequently the subjects of puerperal convulsions when in labour.

Dr Simpson had on a previous occasion attempted to show that one of the most common predisposing causes of puerperal convulsions was albuminuria, or rather some morbid state of the blood dependent upon albuminuria,—a doctrine which he had taught for the last seven or eight years (see *Monthly Journal* for 1843, foot-note p. 1015), and which Dr Lever had studied and successfully illustrated. He believed this diseased condition of the blood to produce a preternatural excess of irritability or polarity of the nervous system, and more especially of the spinal system of nerves; and in this way a morbid predisposition in it to convulsions under forms and degrees, irritation in the uterus, or bladder, or intestinal canal, &c., that would be incapable of producing this convulsive effect were not the polarity of the nervous system for the time being highly exaggerated. Dr Todd of London had shown last year, that, when the polarity of the nervous system was exaggerated in the lower animals by the exhibition of strychnia, the convulsive tendencies resulting from the strychnia could be so far subdued and decreased by making the animals inhale ether. The ether reduced the excessive polarity and consequent convulsions. And the application of chloroform, in the same way in puerperal convulsions, had been tried with success by Dr Wilson of Glasgow, Mr Fearn, Clifton, &c. Dr Simpson had suggested that the organic conditions of the kidneys causing albuminuria, and predisposing to convulsion in puerperal women, was probably often one of *Stearosis*, produced by the previous circulation of the butyraceous or fatty principles of the milk in the mother's blood before delivery. In the present case, one portion of the kidney after death distinctly presented the stearoid or fatty degeneration.

Dr Peddie read a paper "on the mammary secretion; its character, chemical and structural; the value of the microscope in the study of it; its existence an important sign of early pregnancy; and its pathological changes; with hints regarding lactation and the choice of nurses."—[Dr Peddie's paper appeared at length in last No. of the *Monthly Journal*.]

Meeting VI. June 14.—Dr Moir, Vice-President, in the Chair.

CASES OF PROLONGED LACTATION.

Dr Moir mentioned the case of a married lady, who, from certain reasons, was unable to nurse her children, but in whom the secretion of milk was after each confinement unusually protracted, notwithstanding the means used to discuss it. Starvation, purgatives, diuretics, diaphoretics, and alteratives, as internal remedies, and local astringents of various kinds were used, without in the least diminishing the secretion, or having any effect further than that of weakening the patient; so that latterly the only means employed were of a

tonic nature, to sustain her strength. The secretion continued from one confinement till about the third month of the succeeding pregnancy, after which it almost ceased; after her second confinement it continued eighteen months; after her third, twenty-four months; after the fourth, twenty-five months; after the fifth, about twenty-four months, when she had a miscarriage, and since then has had no children. A circumstance that rendered this case peculiarly distressing was, that after her first confinement she suffered from a severe mammary abscess, which, by the practitioner then in attendance, was opened close to the nipple. From this wound the milk continually flowed, and as it never healed up, it was impossible to receive the milk from this breast into any convenient reservoir; so that the lady was kept in a constant state of discomfort, her dress, notwithstanding the use of oiled silk and M'Intosh cloth, being completely saturated with milk. The skin over the abdomen and left side was from the same cause much irritated, and in warm weather partially excoriated.

Dr Peddie knew of a woman having nursed uninterruptedly for three years.

NEW FORM OF PESSARY.

Dr Weir exhibited a pessary for prolapsus of the uterus, which he considered useful from its cheapness, its lightness, and the facility of its introduction and retention. It was composed of a slender stem of light wood, from two to three inches in length, surmounted by a shallow cup (see *Figure*), on which the cervix of the uterus rested. The lower end of the stem was furnished with a small knob or button, which, when the instrument was *in situ*, rested on a simple T bandage. This form of pessary had been used for some years by Dr Weir, and by many others, with the best results.

Dr Weir's Pessary for Prolapsus Uteri.

CASE OF PLACENTA PRÆVIA TERMINATING FAVOURABLY TO THE MOTHER AFTER RUPTURE OF THE MEMBRANES. BY DR E. SKAE.

The subject of the case was a Mrs Orr, the wife of a mechanic, and it took place in her third confinement. In her two previous labours, the presentations were preternatural: in the first the breech presented, and in the second the feet. On both occasions, her delivery was successfully accomplished. At these labours, as well as the one under consideration, she had the assistance of a midwife, who afterwards called me in to complete them. She sent for me on the 26th November 1841, in consequence of a sudden and rather alarming attack of flooding, unattended by pain. She stated to me that she expected confinement in the course of a fortnight; that she had been once or twice threatened with flooding during the two or three weeks preceding. On examining the os uteri I found no indication of impending labour, and the hemorrhage had ceased before my arrival. I enjoined absolute rest, and prescribed repeated doses of opium with acetate of lead. The following day she felt quite well, and on the third day I found her moving about, and stating that she felt perfectly well. On the morning of the 3d of December, five days after, I received an urgent call to visit her without delay. I found, on my arrival, that the midwife had been with her for upwards of two hours. Before mak-

ing any examination, I was informed by this attendant, that, during the time just mentioned, and for some time previously, there had been considerable flooding, that the os uteri was considerably dilated, but that it was occupied by a soft mass, which she had latterly endeavoured to pull away, believing it to be a false conception, but that she had speedily desisted in her efforts to do so, in consequence of increased flooding; that she had kept the passage plugged, and had recently administered the ergot of rye. On withdrawing the stuffing which had been introduced into the vagina, and some coagulated blood, I discovered the upper part of the passage occupied by the placenta, a portion of it projecting considerably into the vagina, and somewhat lacerated, the result, no doubt, of the midwife's interference. On carrying my fingers round this mass, I found the uterine aperture well dilated, but its whole area occupied by the placenta. I could readily perceive, after careful tactile examination, that although the whole circumference of the opening was filled by the placental mass, prior to the accession of labour, its centre must have been implanted not exactly over the os uteri, but rather posteriorly; for by carrying my finger between it and the cervix anteriorly, to the extent of an inch or more, I could reach the membranes, or what I supposed to be them. I saw no alternative but to follow the only steps at that time sanctioned in obstetric practice, viz. to perforate the membranes, if they could be reached, and deliver by turning, should the nature of the presentation require it. After carrying my hand into the vagina, I passed two fingers carefully upwards and forwards, between the placenta and the cervix anteriorly, until I felt satisfied that I was touching the membranes, these I perforated, but had no sooner done so than a gush of liquor amnii was followed by the immediate and forcible descent of the fœtal head into the brim of the pelvis, and, by the continued and energetic contractions of the uterus, it speedily engaged in its cavity, the placenta being thrust backwards against the sacrum. Within ten minutes after the membranes were ruptured, a dead fœtus was expelled, and the placenta almost immediately followed. There was no subsequent hemorrhage, and the recovery was uninterrupted by a single unfavourable symptom.

TRIAL OF WILLIAM HETHERTON, CHARGED WITH THE MURDER OF HIS WIFE BY WOUNDING HER IN THE GENITALS.

William Hetherton was tried at Edinburgh, July 24th, before the Lord-Justice Clerk, Lord Cockburn, and Lord Wood. The Solicitor-General and Mr Crawford conducted the prosecution; Mr Moncreiff and Mr Graham the defence.

Hetherton was a blacksmith, and had been married to the deceased little more than nine months. At the time of her death she was about eight months gone with child. Both were addicted to drinking, the wife in particular. She had pawned his clothes to get liquor, and he had sometimes beaten her in consequence. On the evening before the death of the deceased, between eight and nine o'clock, a noise of quarrelling was heard by the neighbours, and some hours later moans, as of a woman in pain, proceeded from the house. About nine o'clock Hetherton went to his sister's house, about fifteen minutes' walk from his own, where he remained about half an hour, and told her that his wife was in liquor, and that he had trailed her from the kitchen to the inner room, and laid her on the sofa. Between the time when the accused left his sister's house at half-past nine that evening, and four o'clock the next morning, nothing is known of what he did except from his judicial declaration. In it he states, that having gone home from his work that evening between seven and eight o'clock, he found his wife lying on a chest in the kitchen drunk—that he pulled her off the chest, and being unable to carry her on account of her weight, dragged her to the inner room, placed her on the sofa, and then left the house to go to his sister's—that he returned from

his sister's about ten o'clock, and did not then go near his wife, but went straight to bed in the kitchen—that about half-past three in the morning he was awakened by his wife calling out, "Rise, Willie, rise!"—that he arose and found her lying on the floor beside the sofa—that she asked for water but could not take it when he brought it, and did not answer when he asked what was the matter—that he put a pillow under her head and threw a blanket over her, then went to bring his sister-in-law. From his sister-in-law's evidence it appeared, that the accused came to his brother's house about four o'clock in the morning, asking her to come along with him as his wife was taken ill. His sister-in-law wished him to go and get a doctor while she dressed herself—he, however, insisted that she should go and see his wife first, and waited till she was ready. On the way to his house she again told him that nothing could be done without a doctor, while he still urged that she should go and see his wife first. She, however, declared that she would not go without a doctor, and then he consented to her calling for Dr Burn in their way. Hetherton then went on before, and she followed shortly with Dr Burn. They found Hetherton waiting for them near his house, and they all went in together, and found Mrs Hetherton lying on the floor on her left side, with her back to the sofa, and her head a little beyond the end of it, not undressed, and with a blanket over her. Neither Dr Burn nor the sister-in-law appear to have observed any pillow under her head. Dr Burn deposed that he found her quite cold, still breathing, but pulseless and evidently dying—that she was lifted into bed, and in a few minutes expired. Dr Burn had come under the impression that it was a case of premature labour brought on by intemperance, and nothing suggested to him any suspicion of violence. He believed at the time that death had been caused partly by hemorrhage, partly by extreme intoxication. He had heard before that the deceased was much addicted to drinking. He had known the accused for a long time. He observed a bruise on one of her hands, which Hetherton ascribed to a fall. When he first saw her on the floor, there was no appearance of any derangement of her dress. There was some blood on the carpet near her feet, and if that blood had come from the lower part of her person, she must have been moved afterwards towards the inner end of the room; if she had fallen from the sofa, she must have been moved in the same direction after she fell. He did not examine her person or her clothes particularly, having no suspicion of any thing being wrong, but he felt there was blood on her under-clothes. He applied his hand to the abdomen, and examined the vagina, in which he found no coagulated blood. The sister-in-law described the position of Mrs Hetherton when first seen, and also the place of the blood, in exactly the same terms as Dr Burn; and from her statement it appears that the quantity of blood was not inconsiderable. She saw some clotted blood in a chamber-pot beneath the sofa, and in the kitchen a sponge in a soap dish, with what appeared to be fresh blood upon it. The clothes taken from her person were put into a tub of water. Two females who assisted the sister-in-law in dressing the dead body, corroborated her evidence as to the blood on the clothes, and on the carpet, and spoke to bruises on the arm of the deceased. Preparations were made in the course of the same day, with some haste, for the funeral, and in the evening the body was placed in the coffin—but as information had been lodged with the authorities as to the noise of quarrelling heard the previous evening, Dr Weir and Mr Glover were directed to examine the body of the deceased. The essential part of their report is as follows:—

"The body appeared to be that of a person about thirty years of age. *Externally*—With the exception of an ecchymosed spot at the outer angle of the left eye, the head presented no marks of violence. Both arms were covered with numerous marks of recent and old contusions, particularly about the left elbow and forearm; the cuticle was also abraded in one or two places of the left hand. An ecchymosed patch of considerable size was also observed on the left side of the body, a little below the navel. The genital organs were wet

with blood, and stained the shift to a considerable extent. The left labium was greatly enlarged, being three or four times larger than natural; the right was normal both in size and appearance. Both legs, from the hips down to a little below the knees, were covered with marks of contusions, all of which, when cut into, presented an ecchymosed appearance. *Internally—Head.*—On detaching the scalp, we found two ecchymosed patches on its inner surface, one on the fore part of the head, the other on the back part—the first was circular and about the size of a half-crown, the latter more irregular in shape, and much longer. Upon removing the skull cap and dura mater (the external membranous covering of the brain), a serous effusion of a straw colour was observed under the arachnoid (middle membrane). The effusion was most abundant over the posterior hemisphere of the brain. The veins, sinuses, and substance of the brain itself, were almost destitute of blood, giving the organ a most blanched appearance. *Chest*—Lungs healthy in structure, and not collapsed. Heart a little larger than usual, the substance pale and bloodless; all its cavities nearly empty.

"*Abdomen and Pelvis.*—Viscera healthy, but perfectly blanched, particularly the liver. The uterus (womb) contained a fœtus about seven or eight months old, and was healthy in all its structures. On cutting into the enlarged labium already referred to, its substance was found to be infiltrated with serum and coagulated blood. On its inner surface a small portion of the mucous membrane was abraded, similar to that remarked on the left hand. Upon separating the labia, and pushing back the nymphæ, we observed a ragged wound situated immediately to the left side of the orifice of the urethra. The dimensions of this wound were about three quarters of an inch long, half an inch broad, and three quarters of an inch deep. It easily admitted the point of the forefinger, which, when introduced, could be moved about in a cavity of much greater size than the external opening. In the walls of this cavity, and on the parts adjoining, we found a small quantity of coagulated blood. From the above *post mortem* appearances, we are of opinion, that death was occasioned by the profuse hemorrhage from the wound in the genital organs, and the other injuries described."

A number of smith's tools were found in the house, several of which the medical witnesses were of opinion might have sufficed to produce such a wound. The police-officers on searching the house took possession of the carpet and crumb-cloth of the inner room, through both of which the blood had penetrated to the floor; they observed blood upon the tongs and shovel in the same room, on the ends used in the fire; in the kitchen they found the sponge before referred to, with blood upon it, and also a stick with some spots of blood upon it, and under the bed in the kitchen they found a striped shirt, one of the sleeves and one of the sides of which were stained with blood. Hetherton, in his declaration, acknowledged that the shirt was his, but could not account for the blood upon it, unless it might have been put amongst his wife's clothes, or had been stained in assisting to put his wife into bed; but he could not say whether he wore the shirt on the night in question or not. He said, that three or four weeks before, his wife had bled a good deal from the private parts after some days' hard drinking, and he supposed that the loss of blood at the time of her death was from the same cause. As to the bruises on her body, he acknowledged that he had beat her about the upper parts of the body that evening both with his hands and with a stick, not with the purpose of doing her any serious injury, but to make her confess what she had done with a shirt which he missed; and being shown the stick before referred to, he said that that was the stick he had used, and that it was broken over her—he further said, it was after he had dragged her into the inner room and laid her on the sofa, that he beat her, and that she cried "murder" then, and also at the time he was dragging her—further, that he saw no marks of bleeding about his wife when he put her upon the sofa, or before he left the

house about nine o'clock to go to his sister's, but that he saw blood at her feet before he went for his sister-in-law in the night-time—that the marks on the tongs and shovel were not blood, but rust produced by drops of water falling upon them when water was taken from the cistern which stood near—and, being shown the sponge, he declared that he had not used it on the night of his wife's death.

After the declaration, from which extracts have just been given, was read in court, Drs Gairdner and Douglas Maclagan were examined in exculpation. Dr Gairdner deposed, that he had examined the report of Dr Weir and Mr Glover, and was of opinion that the wound near the urethra might have been inflicted by a kick given by the foot from behind, supposing the clothes were removed. It might also have been produced by falling on a hard substance under similar circumstances. Dr Douglas Maclagan gave similar testimony, and added that the laceration might have been produced by any blow impinging on the pubis, though the instrument did not reach the seat of the wound. This, however, was merely a theoretical opinion.

After being charged by the Lord Justice-Clerk, the jury remained in deliberation for an hour and a half, and then by a majority found Hetherton guilty of culpable homicide (manslaughter). He was sentenced to be transported for life.

The verdict shows, that it must have been the opinion of the majority of the jury that Hetherton, by the injuries which he inflicted, had no intention of taking away the life of his wife. In short, they find that he did not, or at least that there is no sufficient evidence that he did, deliberately inflict the wound in the genitals of which she died, with the knowledge that such a wound would probably cause death. As we despair of ever seeing more complete evidence in any instance of this insidious kind of murder, the melancholy conclusion is, that this is a crime which may be committed under cover of very slight precautions against detection, without subjecting the offender to the proper judicial penalty. This woman unquestionably died of hemorrhage—not of that kind of hemorrhage which might naturally arise from mere ill usage in an advanced state of pregnancy, but from hemorrhage the result of a wound that must have taken place but a few hours before. Be it so that such a wound might be the effect of a kick, it could hardly have been other than a kick employed deliberately with the expectation of causing death. If a man uncover his wife's person while she is insensible from intoxication or previous ill-usage, or the joint effect of both, and place her in such a position as shall freely expose the orifices of the internal parts, and then deliberately kick her in those parts, thus producing a rent capable of giving rise to a fatal hemorrhage, he is as much guilty of murder as if he had made a gash there with a razor. In the circumstances of the case before us, it is inconceivable that a mere *random* kick could have caused the wound. The woman was dressed as usual—she had no stays, but she had a printed gown and two petticoats over her chemise. Whether we suppose her standing, or on the sofa, or on the floor where she was found, such an effect in the place described of a *random* kick must be pronounced impossible. If, then, Hetherton was not guilty of murder, the only alternative (since we believe that the wound was deliberately inflicted) is the extremely improbable one, that he did it with the minor intention of injuring her, but not of causing death; unless we resort to the still more violent supposition, that the deceased inflicted the wound herself.

REPORT OF THE COMMITTEE ON POOR-LAW MEDICAL OFFICERS.

WE regret that we have only space for the concluding proposals of this excellent document. They are as follows:—

"1st. That in the opinion of your memorialists, it is essential that the payment of medical officers should be by a fixed salary, exclusive of fees for midwifery and important surgical cases.

"2d. That the amount of salary might justly be fixed, either by an estimate of the average of cases attended during a series of past years, considered in connexion with the area of the medical district, or by the payment of a certain sum per head on the population, corrected by the consideration of the relative density and poverty of the district.

"3d. That making every allowance for the difference which must exist in the remuneration of private and public practice, it would not be just that the salary should be based on a less amount than 6s. 6d. per case, with the charge varying with the area.

"4th. That medical officers of work-houses should be paid a separate salary, based on the average number of inmates, at not less than 7s. to 10s. per head.

"5th. That it is just and proper, and conducive to the interests of the poor, that an extra payment should be made, as at present, for midwifery and for important surgical cases; and that it appears to your memorialists advisable that the fees should be paid for cases occurring in work-houses, as well as in out-door cases.

"6th. That to the cases of surgery requiring payment, enumerated in the General Order of the Poor-Law Commissioners, should be added—

"Fractures of the clavicle;

"Fractures of the skull;

"Retention of urine, when requiring repeated introduction of the catheter;

"Severe burns and scalds;

"And that L.3 be paid for compound fracture of the arm.

"7th. That the payments allowed for midwifery and surgical cases under the General Order of the Poor-Law Commissioners are satisfactory in amount.

"8th. That in order to remove difficulties in the way of a more equitable payment of medical officers, and considering that half the Union medical expenses are already paid from the Consolidated Fund, it is highly desirable that the whole expenses of the medical attendance on the poor should be removed from Unions to the Consolidated Fund.

"9th. That it is expedient that there be a Director-General of the Poor-Law Medical Department appointed by Government, and exercising supervision over poor-law medical practice, in the manner of the other public services.

"10th. That it is expedient that there should be Inspectors-General of Poor-Law medical practice appointed by Government, and acting under the orders of the Director-General.

"11th. That the Inspectors General, acting each in a district assigned to them, should examine the Infirmaries of work-houses, inspect the reports of the medical officers, inquire into cases of alleged negligence, &c. &c.; and that matters of disputed payment should be referred to them.

"12th. That the medical officers of Unions should be appointed by Boards of Guardians as at present, subject to the approval of the Director-General; that their qualification should be as at present ordered, but that all future modifications of the qualifications should be made by the Director-General.

"13th. That the appointments of all medical officers should be permanent—that is, to endure until they die, resign, or are dismissed for some valid cause.

"14th. That the Forms of Books or Reports should be approved by the Director-General, and should be as short and simple as is consistent with the requirements of the public service.

"15th. That the access of the paupers to the medical officers should be made as ready as a due distinction between those who require parochial attendance, and those who do not, will admit. That in doubtful cases where illness exists, and there appears to be temporary destitution only, the Board may grant medical relief by way of loan; that in such cases a fee of be paid by the Board of Guardians to the medical officer. Signed on behalf of the Committee,

"Thomas Hodgkin, M.D., *Chairman*."

SANITARY REFORM—THE HEALTH OF TOWNS BILL.

We congratulate the public of England on the progress of this bill, which is now so far advanced that it may be considered as secure. By an amendment in the Upper House it was proposed, that upon the petition of one-tenth of the rate-payers, in any district where the deaths from epidemic, endemic, or contagious diseases exceed the proportion of 20 per cent. of the whole deaths, the General Board of Health may direct a Superintending Inspector to make inquiry as to the sanitary condition of the district, and report to the Board. The Commons alter this amendment in as far as relates to the above test of the necessity of inquiry. The amendment, in the form in which it has passed the House of Commons, enacts that the Board of Health may institute inquiry, when the deaths in 1000 of the population exceed 23 per annum. A conference with the Lords has been appointed to settle which of these amendments shall become law. Considering that the average mortality in England is about 21 in 1000 of population, the latter would bring many of the more unhealthy districts immediately within the scope of the act.

The limitation of this act to England is its greatest imperfection; and, considering the probable advent of cholera, is a subject of the deepest regret. We are afraid that Scotland and Ireland, especially the latter, will be found no better prepared for this scourge, in the event of its arrival, than at the time of the last epidemic. Indeed, it is to be feared, that if famine be superadded to pestilence in Ireland by a second failure of the potato-crop, the visitation may occur under yet worse circumstances than on the former occasion.

Both Scotland and Ireland were promised separate bills at the beginning of the session; but these with many others have been dropped.

THE ROYAL COLLEGE OF CHEMISTRY.

We regret to see, by the report of the annual meeting of the College of Chemistry, that the financial condition of the institution is not flourishing. It had been found necessary, it is said, to incur a heavy debt for the building of the laboratories, &c., and a fund was opened to defray it, which promised well in the beginning, but eventually left the institution with liabilities to the extent of L.2000. In order, as far as possible, to meet those liabilities, the council proposed a *pro rata* subscription among their body. The result was, that several members of the council, and other noblemen and gentlemen, amounting in all to twenty-three, contributed L.50 each towards the object, and six others promised to come forward with a similar sum. By this means they were enabled to pay off the debt upon the building account, amounting to L.1200, which covered every remaining liability connected with the building, with the exception of L.100 due to the architect. The increase in the number of students and their progress are reported as satisfactory; and it was stated that in the laboratory all classes of society are represented. "Working at the same table, may be found a peer of the realm, a medical student, a member of Parliament, and a druggist's apprentice."

CHOLERA AND MAGNETISM.

The *Manchester Guardian*, quoting a letter from St Petersburg, says,—"A very important discovery has recently been made here, which clearly proves that the malady is in the air, and that, therefore, quarantines are utterly useless. The air here has had a very singular effect on the magnetic power. Whilst the cholera was at its height, the action of the magnet was nearly neutralized; which, now the disease is gradually subsiding, assumes by degrees its former power. A magnetic block which used to carry 80lb. would, during the worst time of the cholera, not carry above 13lb. It strength has now in-

creased again to 60lb. The electro-magnetic telegraph at one time would not work." It was also recently mentioned in the *Lancet*, that, during the prevalence of Asiatic cholera in London, there was a cessation of the disease in St Giles's after a violent thunder storm.

STATE OF THE PARISIAN HOSPITALS AFTER THE INSURRECTION OF JUNE.

What strikes you at first entering the wards of the Hotel Dieu and St Louis is the quietness of every thing; the silence and propriety of the *elèves;* the anxiety of the medical man going round for the welfare of the patients, and the information of the pupils, by whom, I must say, I never witnessed greater kindness or attention shown to any class of patients. At the entrance to each ward the *Personnel* and *Reglemens* are posted up; the former containing the names of every one engaged in the ward, medical men, chaplain, matron, nurses, &c.; the rules also not less conspicuous. The pupils, however, are interdicted from stopping in the ward after the surgeon, from percussing the patients, or in any way interfering with them; matters of rather questionable utility as far as the pupils are concerned.

The hour of visiting the hospitals is eight o'clock A. M. The *blessès de Juin* were lying every where that accommodation could be had. Many of the cases were of the most frightfully bad character; fore-arms torn in pieces by balls; hands shattered, and legs literally smashed by cannon-shot; by far the greatest number, however, were balls through the chest and shoulders, and all parts of the body exposed above the level of the barricade. It was quite wonderful to see what nature was doing for an immense number of these; the *Vis Medicatrix* seemed, indeed, all-potent. It was curious, too, to witness the nice parts the surgeons had to play; a word of comfort to one; a smile with another; a little bit of evasion with a third; many of them having, it is known, had more to do in the work of the barricades than they were willing to confess. There was an appearance of conscious misgiving about many of these poor creatures that one pitied. Several of them were wretched ghastly beings that an *emeute* always throws to the surface; many to whom death would be a respite from their thousand sufferings. There were also the young fellows of the *garde mobile*, especially at the Hotel Dieu; some of them wounded very slightly, others badly. One should see these fierce young fellows under every aspect to understand them; thundering along a *pas de charge,* or rolling in the grass at Versailles, without care or anxiety of any kind, except perhaps for fighting, and without a wish beyond that of standing sentry with a long gun, nearly twice the length of themselves, at the door of the camp. These nearly all recovered. * * * The general appliances of the hospital were excellent. Oiled silk, lint, charpie, sponges, ices, wines, jellies, and soups in abundance. Much more stress is laid on the article of diet than in our own hospitals. Every thing seemed well managed indeed. * * * Several amputations were performed. Other cases, though of a very grave character, were apparently doing very well under the ordinary treatment. In one case the ball went right through the popliteal space without injuring either nerves or vessels; in another, a man shot in the back of the head, the ball went out of the mouth; both were apparently doing well. In many instances the ball had gone through the shoulder-joint, without any very manifest symptoms supervening. In another case a man was shot through the kidneys,—the ball making two openings,—actually broken in two on the bodies of the vertebræ. In another case, doing well, the entire foot was shattered by a cannon-shot. In another, a very severe injury occurred in the tract of the femoral vessels, without, however, any hæmorrhage; the man seemed as if nothing had happened. The injuries of the lower extremities were, indeed, almost beyond belief. In one singular case of injury of the head, the entire of the coverings of the lower jaw had been swept away, leaving the cartilages of the larynx ex-

posed; there seemed no unusual hæmorrhage. In a severe injury of the upper extremity, the ball traversed the upper part of the shoulder, wounding the spinal column, and producing a want of sensibility and motion; the poor man was alive simply because it had not gone one half inch higher, and cut off the origin of the phrenic. * * * The treatment in all the hospitals is, perhaps, a little too general; what is ordered for a stout *mobile* being likewise ordered for a starving insurgent. All those injuries not requiring amputation are treated by cold applications, and venesection if necessary, followed, after reaction, by emollient cataplasms. There is very little cutting or enlarging of wounds, and a great deal is left to Nature. The relative mortality will, perhaps, prove whether this or the opposite system is to be preferred In two of the worst cases of wounds of the chest here, the operation for empyema had to be performed.—*Dublin Quarterly Journal, Letter to the Editor by Charles Kidd, M.D.*

THE PARISIAN PHARMACIENS AND THE INSURGENTS OF JUNE.

WE know the means employed by the insurgents either to procure munitions of war or to render their projectiles more efficient. Several pharmaciens in the Faubourg St Antoine, at La Chapelle, and in the Faubourg Saint Marceau, were ordered to prepare powder and gun cotton. One of these, we are assured, did not require much persuasion; the others only complied with a pistol at their head. Besides this generally efficacious instrument of persuasion, the insurgent gentlemen employed an infinity of politeness, and even of good-humour. A pharmacien alleges that he does not know how to make gunpowder. "Oh, *mon cher!*" replies one of the leaders, "if it were how to invent it, I should not address myself to you; but it is only a question of preparing it, and I shall help you. *Recipe*—Saltpetre 75; charcoal 12·5; sulphur 12·5. Mix in a wooden mortar." "But I have no saltpetre." "Bah! I see on the bottle there NITRAS POTASSÆ. We shall manage with this nitrate of potassa." The unfortunate pharmacien is obliged to execute the order. Another tried in a similar way to elude an injunction to prepare gun-cotton, but forthwith they had placed upon his counter all the necessary ingredients, and set his restive spirit to work. The process succeeded marvellously!—*Feuilleton de la Gazette Médicale*, No. 29.

STATE OF MEDICAL EDUCATION IN TURKEY.

[WE are indebted to a medical gentleman, at present residing in Constantinople, for the following details. They point to the existence of a state of matters, of which very imperfect accounts have as yet reached this country; and such as, undoubtedly, is at once the proof and the earnest of a new era in the history of civilisation having begun in the East.]

"Military hospitals, on a large scale, are either built or a-building in every quarter of the Turkish empire. There are about one thousand European surgeons attached to the different regiments, two hundred of whom are Jews. The chief professor of the Medical College, Dr Spitzer, is a Jew. He is also one of the physicians in ordinary to the Sultan. By him I was introduced to H.E. the Hakim Bashy Ismael Effendi, the chief physician of the Ottoman empire, who kindly permitted me to visit the different lecture-rooms in the Medical College. It is certainly in a very flourishing condition, considering that it has been in existence only eleven years. The pupils are brought, by order of the Sultan, from all departments of the empire, and are lodged, fed, clothed, and educated at the expense of the government. When qualified as physicians and surgeons, they receive appointments in the army and navy, with salaries of L.200 or L.300, and upwards, according to rank, without distinction of Jew or Gentile. Until lately, however, there were no Jews in this college: not that

the government excluded them, on the contrary, they were invited; but that people, who have been scattered amongst all nations, yet amalgamated with none, would not send their sons to this medical establishment, even although the most flattering prospects of education and worldly advancement were held out to them. But the government condescended to smooth all the difficulties which stood in the way of the improvement of this section of its subjects. Through its agents, it held interviews on the question of conscientious scruples with the chief Jewish Rabbis; and the result was, not only the guaranteeing liberty of conscience to the Jews who should enter the Medical School, but the assigning to them a distinct portion of the college, so that they might live separate from the Gentiles, the appointment of a superintendent of their own persuasion to see that their religious duties and services should be strictly observed—also a shockel or butcher of their own; and, in short, every arrangement was made to prevent their being constrained to do any thing contrary to their conscience. In the language of last year's report of the college authorities,—"Toutes les difficultés ont été aplanies, et le Gouvernement n'a reculé aucune sacrifice pour exercer aussi son influence civilisatrice sur cette partie des sujets de l'Empire." The Sultan lately visited the college and presided at the examination of the candidates for the medical degree. When the pupils are first introduced to the college, they are, for the most part, raw, ignorant, and uncivilized. They are at first taught Turkish, afterwards the Arabic and French languages; next geography, history, arithmetic, and other elementary branches of education, including natural history. They have already a very tolerable museum of natural objects, well preserved and well arranged. A small botanic garden is also attached to the college. After undergoing a thorough elementary education, the pupils enter their medical course, comprising lectures on anatomy, physiology, chemistry, materia medica, practice of physic, surgery, and midwifery. The only room I did not see was the dissecting room; it was closed at the time for want of subjects, which it is difficult to procure in a country where so much prejudice against dissection exists, and even against touching a dead body. I was shown into the grand examination room, fitted up with a great throne of state for the Sultan, who presides at the yearly examination of the candidates for the medical degree. There are also a dispensary and hospital attached to the college. The hospital is divided into medical and surgical wards, and a special ward is set apart for diseases of the eye. Dr Spitzer delivers clinical lectures in the hospital, which he kindly invited me to attend.—*Constantinople*, March 1848.

THE PROGRESS OF CHOLERA.

Russia.—The German papers publish letters from St Petersburg of the 3d inst., stating that the cholera has gradually disappeared in that town. The cholera hospitals have been shut up by order of the Emperor.

At the date of the last advices the cholera was fast decreasing, and it was expected that its total disappearance might be soon looked for. The average of cases was reduced to one hundred a day, and several of the hospitals opened exclusively for the treatment of the disease had been already closed.

From a late number of the *Military Medical Gazette* of Russia, it appears that, since the appearance of the epidemic, there were seized at St Petersburg from the 30th of June to the 21st of July, 19,772 persons, of whom 4834 recovered, and 11,068 died. In the whole of Russia, since the first appearance of the cholera, the 28th of October 1846, to the 5th of July 1848, 290,318 persons were seized with the epidemic, and 116,658 died. On the 28th of July there were at St Petersburg 2396 cholera cases; in the course of the day 137 fresh cases occurred; 211 recovered, and 82 died. On the 29th there were 2240 sick, 132 new cases; 188 recovered, and 68 died. On the 30th their remained 2116 cases under treatment. We learn that at Berlin four cases of cholera

have appeared. At Munich the Ministry is taking active preparatory measures in the event of the appearance of the cholera in Bavaria. At Königsberg two cases have occurred; in consequence of which a committee of health was sitting in that city to take measures against the spread of the epidemy.

Posen.—The *Kolner Zeitung* has a letter from Posen of the 5th inst., stating that a case of Asiatic cholera occurred in that city. The patient—a woman—was at once transported to the Cholera hospital, where, it is asserted, she is doing well, and likely to recover. Cholera hospitals have, by order of the Government, been prepared in all Prussian towns and large villages, and every care is taken to lessen the horrors of the approaching pestilence.

Vienna.—The *Gazette d'Augsburg* states that several cases of cholera have been observed in this capital.

CENSORSHIP IN CHINA.

Some passages contained in a medical thesis, supposed to reflect on the sacred person of the Chinese Emperor, were condemned by the committee of censorship, and the author was ordered to receive one hundred lashes. The students immediately assembled to the number of 5000, and, with the assistance of the people, disarmed the military. The Emperor, fearing the result, hastily called his council, and revoked the order.

BOOKS RECEIVED.

1. A Course of Lectures on Dental Physiology. By John Tomes, Surgeon-Dentist, &c. London. 8vo. 1848.
2. Nederlandisch Kruidkundig Archief, Nitgegeven door W. H. de Vriese, F. Dozy, en J. H. Molkenboer. Leyden. 8vo. 1848.
3. Report of the Directors of the Montrose Lunatic Asylum, Infirmary, and Dispensary, (Instituted 1781), for the year ending 1st June 1848. Montrose. 8vo. 1848.
4. Scriptural Authority for the Mitigation of the Pains of Labour by Chloroform, and other Anæsthetic Agents. By Protheroe Smith, M.D., &c. London. 8vo. 1848.
5. Littell's Living Age. With a History of the Ether Discovery. Edited by R. H. Dana, jun. March 1848. Boston. 8vo.
6. Arguments against the Indiscriminate Use of Chloroform in Midwifery. By S. William J. Merriman, M.D., Cantab., &c. London. 8vo. 1848.
7. Oratio Anniversaria Harveiana. 1848. A Francisco Hawkins, M.D., &c. 4to. Londini. 1848.
8. De Colica Scortorum Disquisitio, autore Martino Hassing, Dr. Med. Hauniæ, 1848. 8vo. Pp. 100.
9. University Reform, &c., with a Letter to Professor Pillans. By John S. Blackie, Professor of Humanity in Marischal College, Aberdeen. Edinburgh. 1848. Pp. 67.

ERRATA IN AUGUST NUMBER.

P. 103.—In Dr Gairdner's paper, the woodcut was unfortunately inverted. In the course of the volume the two pages will be reprinted correctly; in the mean time, the reader is requested to bear in mind the inversion, and that the explanation of Fig. 19 refers to Fig. 20, and *vice-versâ*.

P. 132—line 18 from top, *for* "Water ℥ij" *read* "Water ℥ij".

NOTICES TO CORRESPONDENTS.

Dr Waters' communication on Gunshot Wounds has been received. It was too late for insertion in this Number, but will appear in October.

The "*Bibliotek for Laeger*" has been received. Our Numbers shall be sent to Messrs Longman and Co., as directed.

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*Report on the Early History and Progress of Anæsthetic Midwifery.* By J. Y. Simpson, M.D., Professor of Midwifery in the University of Edinburgh.

——— "I do think you might spare her,
And neither heaven nor man grieve at the mercy."
Measure for Measure.

My object in the following Report is, first, to sketch briefly the origin and earlier history of the application of anæsthesia to midwifery; secondly, to adduce some evidence of the present progress and state of the practice, more especially in our own country; and, lastly, to answer some of the supposed difficulties and objections connected with it.

NOTES ON THE FIRST APPLICATIONS OF ARTIFICIAL ANÆSTHESIA IN MIDWIFERY.

In a communication laid before the Edinburgh Medico-Chirurgical Society in November last, I attempted to prove that the idea of cancelling and abrogating the pains inflicted by the knife of the surgeon had not entirely originated in our own times. I showed that Dioscorides, Pliny, Apuleius, Theodoric, Paré, and others,[1] had long ago described, and some of them apparently practised, the induction of anæsthesia previous to operations, both by giving their patients narcotic substances to swallow and narcotic vapours to inhale. While making the researches upon which the communication alluded to was founded, I further attempted to ascertain if any writer had proposed to assuage or annul, by the same or by other means, the pains attendant upon human parturition. I failed, however, in

[1] Monthly Journal of Medical Science, Vol. 1847-48, p. 451.

finding any traces whatever either of any practical attempts to abrogate or modify, by true anæsthetic means, the pains of labour, or of any theoretical suggestions even as to the very possibility of effecting that desirable result. And I believe the history of the induction of anæsthesia in midwifery does not date far back, like the history of anæsthesia in surgery. The first instance in which the practice was adopted, occurred in my own practice in Edinburgh on the 19th January 1847. The case was one of deformed pelvis, in which I had pre-determined to extract the child by turning, and to try the inhalation of ether vapour upon the mother, with a view to facilitate that operation. During a week or two previously, I had anxiously waited for the supervention of labour in this patient; for, by the result, I expected that much would be decided in regard to the effect of ether-inhalation in parturition. Would it merely avert and abrogate the sufferings of the mother, without interfering with the uterine contractions? Or, would it arrest simultaneously both the contractions of the uterus and the sufferings that arise from them? As far as the proposed mode of delivery by turning was concerned, it was a matter of no vital importance whether the etherization stopped the uterine contractions or not. And, on this circumstance, depended the eligibility of the case for a first trial of ether-inhalation. The result was most satisfactory and most important; for it, at once, afforded me evidence of the one great fact upon which the whole practice of anæsthesia in midwifery is founded—it proved, namely, that though the *physical sufferings* of the parturient patient could be annulled by the employment of ether-inhalation, yet the *muscular contractions* of the uterus were not necessarily interfered with; or, in other words, that the labour might go on in its course, although the sensations of pain usually attendant upon it were, for the time being, altogether abrogated.

This case, with its more obvious results and inferences, was communicated to the Obstetric Society at their meeting on the 20th of January.[1] In the course of the subsequent three weeks I had an opportunity of trying ether-inhalation in several cases of natural labour and in one forceps case; and, at the next meeting of the Obstetric Society on the 10th February, I took an opportunity of bringing the subject under the attention of the members at greater length. In the published reports of the society,[2] the various inferences which then appeared to me to be deducible in regard to it are given in the following terms:—

1. That the inhalation of ether procured for the patient a more or less perfect immunity from the conscious pain and suffering attendant upon labour.

2. That it did not, however, diminish the strength or regularity of the contractions of the uterus.

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[1] Monthly Journal, Vol. 1846-47, p. 639. [2] Ibid. p. 795.

3. That, on the other hand, it apparently (more especially when combined with ergot) sometimes increased them in severity and number.

4. That the contraction of the uterus after delivery seemed perfect and healthy when it was administered.

5. That the reflex assistant contractions of the abdominal muscles, &c., were apparently more easily called into action by artificial irritation, and pressure on the vagina, &c., when the patient was in an etherized state.

6. That its employment might not only save the mother from mere pain in the last stage of labour, but might probably save her also, in some degree, from the occurrence and consequences of the nervous shock attendant upon delivery, and thereby reduce the danger and fatality of childbed; and,

7. Its exhibition did not seem to be injurious to the child.

Full details of some of the principal cases upon which these inferences were founded, were, along with other additional instances, subsequently thrown together and published in the form of a communication to the *Monthly Journal of Medical Science*.[1] In that paper I made some observations on the question, Whether it would be proper to employ anæsthesia in natural labour? I adduced various reasons from physiology and pathology for believing, that the parturient action of the uterus would go on healthily and uninterruptedly though the influence of the mind and purely cerebral functions were suspended, and that the dangers of the nervous shock attendant upon labour would be lessened; and I pointed out the necessity of ascertaining, by a cautious series of observations, what counter-indications there might be to the employment of the practice;—whether it were ever apt to give a tendency to hemorrhage or other complications; its influence, if any, upon the child; the length of time its use might be continued in any one case, &c.

At the date at which the paper that I have just referred to was written, viz. the 18th February, the longest time during which I had ventured to keep a parturient patient in the anæsthetic state was about half an hour. And many who believed that this state might be induced without danger for a few minutes, entirely doubted whether it could be sustained for any great length of time without extreme hazard. During the experience, however, of the next two or three weeks, I ascertained the fact, that the anæsthetic action could be safely kept up during labour for one, two, three, or more hours. At one of the subsequent meetings of the Obstetric Society, this result and others were adduced,[2] and the following additional deductions drawn, as stated in the words of the printed proceedings of the society, viz.—that,

1. The state of etherization had little or no influence upon the fœtus, none, at least, of a deleterious kind,—the fœtal heart increasing only a few beats, if at all, when the mother was kept long and fully etherized, either during pregnancy or labour.

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[1] "Notes on the Employment of the Inhalation of Sulphuric Ether in the Practice of Midwifery."—Monthly Journal of Medical Science for March, p. 721.

[2] Monthly Journal of Medical Science, 1847-48, p. 214.

2. The mother, during labour, may be kept etherized, if required, for one, two, three, or more hours. Dr Simpson described two cases, in one of which the mother was about six, and in the other about four hours etherized before the children were born. In both cases the duration of the intervals and of the pains before and during etherization was noted (as in the experiments which Dr Simpson had published on galvanism), and the etherization seemed to have no effect either on their frequency or strength. But,

3. In two or three cases, Dr Simpson had seen a very deep state of etherization modify apparently the full strength of uterine contractions, but they recurred immediately in full force when the patient was allowed to fall back into a state of slighter etherization.

4. Dr Simpson had hitherto seen no traceable injury to either mother or child from its employment, but the reverse.

5. The inhaler he used was either a concave sponge saturated interiorly with ether and held over the face, or a simple portable flask without valves.

The first case of labour in which I employed artificial anæsthesia occurred, as I have already stated, on the 19th January 1847. This case and its results were stated publicly, on the following day, to my Class in the University, and immediately became extensively known to the profession through the medium of the public journals.[1] In the course of a short time the practice of anæsthesia began to be tried in other medical schools. On the 13th February, Dr Murphy of London stated to the Westminster Medical Society that he had employed it in a case of turning.[2] On March 27th a case was published of the use of ether in natural labour,[3] by Mr Lloyd. Dr Protheroe Smith delivered a patient under a state of anæsthesia on the 28th of March; and in the *Lancet* for 1st May he published a paper, "On the Employment of Ether by Inhalation in Obstetric Practice," giving an account of this and two later cases in which he had recourse to anæsthesia during labour. He afterwards sent to the same journal several additional cases and remarks.[4] Mr Lansdowne of the General Hospital, Bristol, subsequently published various cases in which the practice was successfully employed. His first case occurred on the 8th April.[5]

In Ireland, the first case delivered in a state of artificial anæsthesia was on the 28th November 1847. The patient was under the care of Dr Tyler of Dublin. It was an instrumental labour.

In France, the practice was much more early tried. In about a week after the first case occurred in Edinburgh, Fournier Deschamps delivered a patient by the forceps when she was in a state

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[1] See Medical Gazette for 1847, Vol. XXXIX. p. 460. Also Provincial Journal for 1847, p. 84.

[2] Lancet for February 27, 1847.—Before this date, viz. on the 26th of January, a state of anæsthesia was attempted to be induced in a patient upon whom the Cæsarean section was performed by Mr Skey of St Bartholomew's Hospital, London. "But the inhalation of the vapour of ether was unsuccessful, or but very partially successful."—(*Lancet*, Vol. I. 1847, p. 140.)

[3] Medical Times, 1847, p. 96.

[4] Lancet for 1847, Vol. II. p. 121, and p. 305.

[5] Lancet, Vol. I. 1847, p. 446.

of anæsthesia.[1] This was on the 27th of January. On the 8th February, Professor Paul Dubois exhibited ether in a case of forceps operation at the Hôpital de la Maternité of Paris, and up to the 23d February had used it in four other deliveries. He has not himself published, as far as I am aware, any written observations of his own upon the subject; but he early brought the question, in an interesting and able form, before the French Academy of Medicine,[2] and various reports of this important oral communication immediately appeared in different journals.[3]

Professor Dubois' conclusions, as officially reported in the *Bulletin de l'Académie*, were as follows :—[4]

1. The inhalation of ether can annul the pain of obstetrical operations.
2. It can suspend the physiological pains of labour.
3. It destroys neither the uterine contractions, nor the contractions of the abdominal muscles.
4. It diminishes the natural resistance of the perineum.
5. It does not appear to act unfavourably on the health or life of the infant.

Professors Villeneuve[5] of Marseilles, and Stoltz[6] of Strasbourg, subsequently published some additional cases and observations.

In Germany the first case of anæsthetic midwifery, of which I have found any published notice, occurred on the 24th February 1847, under the care of Professor Martin of Jena.[7] The ether was administered during the operation of separating and extracting an adherent placenta. Professor Siebold[8] read a paper on the subject before the Royal Scientific Association of Goettingen on May 8. He had employed ether in several cases of natural and instrumental labour. On the 3d June, Professor Grenser of Leipsic[9] etherized a patient for a forceps operation, and afterwards resorted to it in several other natural and instrumental cases.

In America,—the country to which we are indebted for the first knowledge of the anæsthetic effects of sulphuric ether in surgical operations,—the same agent was not employed in midwifery till the reports of its use in obstetric practice in Europe had recrossed the Atlantic. Dr Channing, professor of midwifery in Harvard University, was, I believe, the first to employ it in labour.[10] He adopted the practice in two forceps cases; the first of which occur-

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[1] Gazette des Hôpitaux, 30 Janvier 1847.

[2] Bulletin de l'Acad. Roy. de Médic. Tom. XII. p. 400.

[3] " Le 23 Février, M. P. Dubois, qui avait eu connaissance des récherches de M. Simpson, communiqua à l'Académie de Médécine le resultat de ses experiences.—(*Chambert. Des Effets des Ethers*, p. 231.)

[4] Bulletin, tom. xii. p. 407.

[5] De l'Ethérization dans les Accouchements. Marseille 1847.

[6] Gazette Médicale de Strasbourg, 27 Mars 1847.

[7] Ueber die Künstliche Anæsthesie bei Geburten. Jena 1848.

[8] Medical Gazette, Vol. XXXIX. 1847, p. 1052.

[9] Ueber Aether-einathmungen während der Geburt. Leipzig 1847.

[10] Two cases of the inhalation of ether in instrumental labour. Boston 1847. —(From the Boston Medical Journal.) Dr Channing, in a postscript to an

red on May 5, the other on May 15, 1847. Drs Clark,[1] Putnam,[2] and other American practitioners, have latterly published the results of their experience in anæsthetic midwifery.

In November 1847, a new impulse was given to the practice of anæsthesia in midwifery, by the introduction of chloroform as a substitute for sulphuric ether. The ether required to be exhibited in large quantities in order to keep up its action; and hence it was objectionable in ordinary obstetric practice from its bulk, and the inconvenience of its carriage. Most medical men believed that an apparatus of more or less considerable size was necessary for its proper and effectual exhibition, thus further encumbering the practitioner. These and other difficulties were found not to appertain to the use of chloroform, and many, in consequence, adopted it in midwifery, who had previously altogether rejected the employment of sulphuric ether. The first case of labour in which I employed chloroform occurred on the 8th of November. On the 1st December, I reported to the Edinburgh Medico-Chirurgical Society a series of cases illustrative of its effects and use in natural and operative labours.[3] In the London weekly journals, since that period, Dr Protheroe Smith, Dr Murphy,[4] Dr Rigby, Mr Lansdowne, Mr Brown, Dr Bennet, Mr Phillips, and others, have published cases and reports upon the subject.

RESULTS OF THE PRACTICE OF ANÆSTHESIA IN MIDWIFERY.

Since January 1847, up to the present time, I have, in my own practice, delivered about 150 patients under a state of anæsthesia. The results to the children and mothers have been as follows:—

Results to the Children.—In the 150 cases, all the children were born alive except one. In this exceptional instance, the infant was expelled in a decomposed and putrid state, between the seventh and eighth month of utero-gestation. It had not been felt to move, nor had I been able to hear the fœtal heart with the stethoscope, for two or three weeks previously. The mother had, before the present pregnancy, borne several premature dead children. Though the infant was small, yet the suffering attending upon its expulsion offered to be excessive, and, to relieve the mother of this unnecessary agony, I placed her under the influence of chloroform.

During the few weeks of my obstetric attendance after delivery

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American reprint of one of my Essays on Chloroform, has announced his intention of soon publishing a volume "On the Employment of Etherization in Childbirth."

[1] Philadelphia Medical Examiner, March 1848.

[2] Boston Medical and Surgical Journal, February 2, 1848.

[3] See Lancet for November 20, 1847, p. 533, and December 11, p. 613; also Medical Gazette for November 26, p. 934.

[4] See also Dr Murphy's able Harveian Oration on the employment of Chloroform.

upon these 150 cases, only one of the children died; namely, a child who sank under the symptoms of cyanosis. Nor am I aware that any of them has suffered, up to this time, under "cerebral effusions," or "convulsions," or "hydrocephalus," or any other of the affections which have been prophesied as certain to befall all such infants as would be borne in labours rendered painless by art. Perhaps it may be proper to add, that none of the children have shown any symptoms of what has been calmly averred, in more than one publication in London, as a possible, or rather probable result of anæsthesia, viz. Idiotcy.[1]—"Dixerit insanum qui te, totidem audiet."

Results to the Mothers.—Among the 150 mothers, the more immediate and direct effect of anæsthesia has been the alleviation or abolition of the physical sufferings attendant upon the latter stages of labour. And, certainly, if the object of the medical practitioner is really twofold, as it has always, till of late, been declared to be, viz. "the alleviation of human suffering and the preservation of human life," then it is our duty as well as our privilege to use all legitimate means to mitigate and remove the physical sufferings of the mother during parturition. The degree of these sufferings is, as a general rule, assuredly such as to call for this aid and mitigation. In proof of their severity, I might cite the unprejudiced testimony of various obstetric authors. At present, I shall content myself with one. Professor Meigs of Philadelphia—a declared opponent of the innovation of artificial anæsthesia in labour—when speaking of the sufferings of the mother in human parturition, fully admits their intensity. "*What* (says he) do you call the pain of parturition? There is no name for it but *Agony;*"[2] and he elsewhere speaks of the pains in the last stage "as absolutely indescribable, and comparable

[1] How can we "know or ascertain the possible consequences of the use of such an agent on the brain of the child? And how can we calculate what may be the ultimate consequences of its action in reference to the development of the mental faculties?"—*Dr Malan, in Lancet for April* 29, 1848.

"It is admitted by all, that the pulsations of the fœtal heart are *greatly increased* during inhalation,—indeed, to such an extent has this been noticed, that in some instances the pulsations could not be counted, so much were they accelerated. Are not effusions to be feared from this? Are not convulsions after birth likely to ensue? And may not that occur which would make the most heartless mother shudder at the bare possibility of herself, by her want of courage, being instrumental in producing? May not *Idiotcy* supervene? Of this we have as yet no experience, nor shall we have, perhaps, for years; but when *one* such case occurs, will there then be found any one who will afterwards be persuaded to submit herself to etherization during pregnancy?"—*Mr Gream, in London Medical Gazette for 7th September* 1848.—It is perhaps superfluous to add, that the premises of the preceding paragraph are as gratuitous as its conclusions; and that the pulsations of the fœtal heart are little, if at all, increased in rapidity when the mother is anæsthetized. "The action of the child's heart (says Professor Siebold) was found to continue quite unaltered, not the slightest change in its frequency and regularity being detected."—*Siebold on the Employment of Ethereal Inhalations in Midwifery, in the Medical Gazette for* 11*th June* 1847.

[2] Females and their Diseases, p. 49. I leave the italics the same as in the original.

to no other pains."[1] Now, surely, if it be the duty of the physician (and who doubts it?) to relieve and remove the pains of colic, of pleurodyne, of headache, neuralgia, rheumatism, &c. &c., it is his duty to relieve pains so severe as to be "absolutely indescribable, and comparable to no other pains." There is not one code of humanity for one class of pains and patients, and a different and opposite code for another class of them.

From November last, when I began to use chloroform in labour, up to the present time, none of the patients, with one exception, at whose delivery I have attended, has been aware of these last "absolutely indescribable" pains; the state of artificial anæsthesia having always been induced for a longer or shorter time before their supervention. And I have kept up this state for a period varying from a few minutes to four, five, six, or more hours before delivery. In the exceptional case referred to, the patient's sufferings were greatly mitigated; but the state of anæsthesia was not, as usual, perfect and complete, the patient having been unexpectedly taken in labour when not in her own house, and the attendant anxiety and confusion of herself and her attendants being such as totally to preclude the requisite degree of quietude. When employing ether, I repeatedly saw cases in which the patients were thus only partially and not completely anæsthetized,—where, in other words, they were not entirely asleep, but were aware of the presence of the uterine contractions, and sometimes experienced from them sensations in some degree painful, but of a very mitigated and blunted character.

Besides thus alleviating and abolishing the sufferings of the mother during labour, the practice of anæsthesia carries along with it other advantages. A number of patients have spontaneously told me, that the prospect of being enabled to pass through the ordeal of parturition with the assistance of anæsthetic agents, and without their usual painful agonies, has destroyed, in a great measure, that state of anxiety and dread of anticipation, which, in former pregnancies, had, for weeks and months previously, silently annoyed and haunted them. If we can thus add to the happiness of our patients, by imparting to them feelings of safety and immunity under one of the severest trials to which nature exposes them, we surely follow out, in its truest sense, that which Dr Meigs correctly describes the office of a physician to be,—namely, "a great mission of benevolence and utility."

But the practice of anæsthesia in midwifery not only saves the mother from the endurance of unnecessary mental anxiety and unnecessary physical agony; it saves her also from some of the dangers attendant upon parturition, by husbanding her strength and warding off the effects of that exhaustion and nervous depression which the pains and shock of delivery tend to produce. In most cases the

[1] Philadelphia Practice of Midwifery, p. 153.

mothers, after delivery, on waking from their anæsthetic sleep, have expressed surprise at their own feelings of strength and perfect well-being; and many, who have borne children previously, have gratefully declared to me the great difference which they have found between their condition after being delivered under anæsthetics, and without pain and suffering, and their state of prostration after former labours, when they were subjected to the endurance of all the usual "pangs and agonies" of parturition. Nor does the benefit end here. By annulling the parturient pains and shock, and their direct and primary depressing effects upon the constitution, we ward off, I believe, to a more or less marked extent, the chances and dangers of those secondary vascular excitements which are always apt to follow indirectly upon them. We increase the chances of a more speedy and a more healthy convalescence. And both patients and practitioners have, as a general rule, had occasion to observe, that the period of convalescence has been evidently curtailed and shortened by the previous adoption of anæsthesia during delivery.

Such certainly has been my own experience. For, since following the practice of anæsthesia, my strong conviction is, that I have seen both more rapid recoveries than formerly, and fewer puerperal complications. One patient, however, had a short attack of peritonitis, requiring leeches, &c. It was her third accouchement and her first living child; and, after her two former deliveries, she had required to be bled, and treated for similar inflammatory attacks. At her first labour she suffered severely from puerperal convulsions. In two others of my patients the convalescence was delayed, in one by an attack of the affection described by Dr Marshall Hall as "intestinal irritation" in puerperal females; and in the other by a fit of jaundice, which supervened two or three weeks subsequently to delivery, and after the patient had been for several days in the drawing-room. In December and January last, an epidemic of puerperal fever swept fatally over Edinburgh and other parts of Scotland. During the period of its prevalence, two of my patients were seized with it and died. But the previous employment of anæsthesia in these cases had nothing to do with this distressing result. Some of my professional brethren here and elsewhere, who were not using ether or chloroform, were much more unfortunate than I was. In a district in the neighbourhood of Edinburgh, one of the medical attendants informed me that, at that time, above twenty mothers were attacked and died, and in none of them whom the disease seized upon, did ether or chloroform happen to be used; while several who demanded chloroform during their labours, all fortunately escaped. The first of the two cases which I met with, was after a second labour. The patient's first labour was extremely tedious and prolonged, and, at last, symptoms supervened which demanded the delivery of the child by the forceps. In her second delivery, the labour was much shorter; the second stage lasted only

for about twenty minutes, and during it she was completely anæsthetized. For fifty hours after delivery she progressed most favourably; and, after seeing her at that time with a pulse at eighty, and otherwise well, I was suddenly summoned, in consequence of extremely severe pain having come on in the uterine region after some muscular exertion. Rigors, rapidity of the pulse, tympanitis, &c., supervened, and she speedily sank with all the usual symptoms of puerperal peritonitis. The second case alluded to was in a primipara. The labour was tedious, the pains severe, and the patient was anæsthetized for four or five hours before delivery. For some days after delivery she went on prosperously, until she became unhappily and greatly excited by discovering intemperate habits on the part of the monthly nurse who was taking charge of her infant and herself. A fit of convulsions (a disease to which, in earlier life, she had been long subject) immediately supervened, and recurred several times. Fatal febrile symptoms then set in, with tympanitis and excessive diarrhœa.

I may add that, in the period during which these 150 cases occurred, I have had under my professional charge 20 or 30 other cases of labour in which anæsthesia was not employed, from the rapidity and facility of the delivery, from the patient being too late to send for assistance, from an aversion on the part of patients to the use of anæsthetics, more especially when ether first began to be used during last year, or from other causes.[1] One of the children in these cases was stillborn, and a second died two or three days after delivery. Two of the mothers suffered from crural phlebitis; a third had a severe attack of puerperal fever, but recovered. Two others died; one of them under an attack of puerperal convulsions and coma, which supervened fourteen days after delivery (see details of it in *Monthly Journal* for 1847, p. 213). In the other fatal case, the patient, who had suffered much in her previous labours, came to the immediate neighbourhood of Edinburgh to be confined, and with the view of using chloroform. But the labour proved unusually rapid, and she was delivered before the call for assistance reached my house. Her recovery went on uninterruptedly for two weeks, when a severe attack of dyspnœa supervened. My friend, Professor Miller, her ordinary medical adviser, saw her in my absence, and suspected some acute affection of the heart. When we visited her together shortly afterwards, the symptoms were then apparently those of acute endocarditis. She was submitted to the usual antiphlogistic treatment, and in four or five days felt again so well as to insist with us upon being allowed to rise, which was forbidden. In the course of a few hours afterwards, another fit of dyspnœa suddenly supervened, and, before Mr Miller reached the patient's house, she was dead. We did not procure an autopsy. If

[1] Since November last I have used chloroform in all the cases of labour, where I have been called in time, except two.

unfortunately she had used chloroform during the labour, as was her intention and wish, many of the objectors to its employment would have, I fear, unhesitatingly attributed the fatal issue in this case to its previous employment.[1]

In addition to the 150 cases of artificial anæsthesia that have occurred in my own midwifery practice, and to which the preceding remarks apply, I have witnessed, during the last eighteen months, a considerable number of instances in which anæsthetic agents were employed in Consultation and Hospital practice; and I have frequently had recourse to their assistance in various obstetric operations that I have been called upon to perform, as in the separation and removal of the placenta, in various cases of turning, in one cra-

[1] In order to show the caution that is necessary in reasoning upon cases of death, apparently from the exhibition of chloroform during surgical operations, I may add that, since November last, scarcely an operation has been performed in Edinburgh without previous anæsthesia, except where the throat or mouth was the seat of incisions, or the operation itself slight and trivial. Amid all the numerous patients thus operated on in public and private practice, when under the use of chloroform, no kind of misadventure or accident has happened; except one case of temporary fainting, a few minutes after recovery from the state of anæsthesia, be regarded as such. On the other hand, among the *few* exceptional cases in which, since November last, patients have been operated on in this city without chloroform, two have died on the table. One of the two was being operated on by Professor Miller for a hernia, which had been strangulated for about fifteen hours; when, after the skin merely was divided, the patient complained of great faintness, vomited, and died with the operation unfinished. This occurred on the 8th of November, two or three days after the anæsthetic effects of chloroform were discovered, and nearly proved the first operation in which it was tried. The second case, a patient of Dr Pattison, had an abscess high up in the neck, requiring a simple puncture for its evacuation. He died without hemorrhage, or admission of air, or other apparent cause, a minute or two after the puncture was made. If chloroform had been used in these cases, would it not by some have been blamed for the result?

Twelve or fourteen months ago, Professor Syme was performing primary amputation of the thigh in the hospital, upon a patient upon whom there was no sign of reaction, and who was not etherized for the operation. "Upon the incisions being made, relaxation of the sphincters took place, the contents of the rectum and bladder were voided, and an effort at vomiting seemed the prelude of immediate dissolution. Before tying (says he) the arteries, I waited to ascertain whether the condition of the patient depended upon syncope or death. My colleague, Dr Duncan, by causing alternate pressure and relaxation of the chest, effected artificial respiration for some time without any sign of returning life; but by and by the actions of the system were gradually restored, and maintained through the use of stimulants."—See *Monthly Journal of Med. Sc.*, Vol. 1847-48, p. 76. Such dangerous symptoms, coming on in an anæsthetized patient, might have been mistaken for the effects of the anæsthesia.

Some time ago, before either ether or chloroform was used in surgery, Dr John Argyll Robertson was called, a few miles out of Edinburgh, to perform the operation for strangulated hernia. After having shaved the groin for this purpose, his patient complained of sickness and faintness, and died before any incisions were made.

Last year, Dr Girdwood of Falkirk came to Edinburgh to see the practice of etherization, in order to be able to apply it in a case of amputation. The day for the operation and etherization was fixed; but, some hours previously, sudden apoplexy came on, and the patient died.

niotomy case, and in several patients who required to be delivered instrumentally by the long or short forceps.[1] In all these varieties of operative practice, the previous superinduction of anæsthesia has appeared to me to be of the greatest and most undoubted benefit. For, besides freeing the mother from the additional corporeal suffering and additional mental anxiety attendant upon operative delivery, the state of anæsthesia enables the practitioner to apply any operative interference that may be necessary with more ease and facility to himself, and consequently also with more safety and success to his patient. When the state of anæsthesia is rendered adequately deep, it renders the patient quiet and unresisting during the required operative procedures; it prevents, on her part, those sudden shrinkings and changes of position which the boldest and firmest woman cannot sometimes abstain from when her mind and body have been worn out, as happens in most operative cases, by a previous long and protracted endurance of exhausting but still ineffectual labour pains;—the introduction of the hand into the maternal passages, or of the hand to guide our instruments, is greatly facilitated both by the passiveness and apathetic state of the mother, and by that relaxation of the passages which deep anæsthesia almost always induces; and, lastly, this state of relaxation and dilatability renders the process of the artificial extraction of the infant through these passages alike more easy for the practitioner, less dangerous for the child, and more safe for the structures of the mother. Besides, in midwifery as in surgery, the utility of anæsthesia before operating, is not, I believe, limited to the mere annulment and abrogation of conscious pain on the part of the patient, and the rendering of the operation itself more easy to the practitioner, but it adds to the safety of our instrumental or artificial interference. For, in modifying and obliterating the condition of conscious pain, the "nervous shock" otherwise liable to be produced by such pain, particularly wherever it is extreme in degree or duration, or intensely waited for and endured, is saved to the already tried and shattered constitution of the mother; and thus, an escape is so far gained from those states of immediate vascular and nervous depression, and of subsequent febrile and inflammatory reactions, that are always apt to follow more directly or indirectly in its train.

MODE OF EXHIBITING CHLOROFORM; DOSE, ETC.

In the course of the preceding observations I have omitted making any remarks on the degree of artificial anæsthesia required in obstetric practice, with the exception of stating that, when instrumental or operative interference is adopted, the anæsthetic state

[1] In one case of placenta prævia to which I was called, the mother had lost much blood, and her lips were pale, and her pulse very weak. On administering chloroform the circulation and pulse rallied—I separated the placenta —no bleeding recurred; and several hours afterwards the child was born. The mother made an excellent recovery.

must be made adequately deep,—so deep, that the patient must be rendered quite passive and apathetic. In fact, when induced for operative purposes in midwifery, the anæsthetic state should be as complete and profound as when it is induced for operative purposes in surgery. But, in common cases of parturition, the anæsthetic agent employed, whether chloroform or ether, does not, in general, require to be given in such large doses as in surgical practice. And in obstetric practice, the rules which I have usually followed in exhibiting the chloroform (the only agent I believe now used in Edinburgh and most other places), are those which I briefly stated when first writing on the subject for this *Journal* in November last. "After the first full dose, a few inhalations, before or with each returning uterine contraction, are generally sufficient. The state of anæsthesia should be made more deep as the head is passing the perineum and vulva."[1] I have elsewhere in the *Journal* stated these rules at somewhat greater length.—(See No. for April, p. 762).

Occasionally I have at first, and especially in the early stages of labour, given the chloroform in small doses only, so as to obtund or obliterate the sensations of pain, without altogether abrogating the state of consciousness. In many patients, this degree of anæsthesia, with the results stated, viz. the loss, in a great measure, of pain without the entire loss of consciousness, can be readily enough induced, and answers excellently well; but, as a general rule, it has appeared to me in some cases objectionable. For not unfrequently, small doses, such as produce this condition, are accompanied with excitement and talking; and sometimes patients have complained to me of this renewal of the chloroform in small doses with each pain, being accompanied each time with a renewal of the ringing in the ears, flashes of light, and other disagreeable sensations accompanying, in some persons, the primary effects of the inhalation. Besides, we are never thus sure that we are really saving the patient to the full extent by the means we are using. If, on the other hand, she happen to be thrown at once into a deeper state of anæsthesia, the chances of such inconveniences and drawbacks are avoided. Often, when the anæsthetic state is thus made deep from the first, the uterine contractions are arrested for a few minutes, but speedily return. In order to effect this, we take care that as soon as the patient is asleep—(and, in natural labour, we seldom or never require to push the inhalation so far as to affect the respiration, and produce noisy inspiration and snoring as in surgery),—the chloroform should be withdrawn, and not reapplied again till the movements of the patient, or the state of the uterus, as felt through the abdominal walls, indicate a returning uterine contraction. A few inhalations given then, and repeated with each returning uterine contraction, keeps the patient in a state of unconsciousness; and this condition may be easily maintained for hours, by administering in this way the chloroform vapour with each

[1] Monthly Journal, vol. for 1847-8, p. 417.

pain, and withdrawing it entirely during each interval. The practice is not to be expected to come upon medical men by intuition; for, like all other practices, some care and experience is necessary in order fully to acquire and apply it. And the *two* main difficulties which every beginner meets with are these; namely, to keep the patient in a state unconscious of pain, and yet not so deeply anæsthetized as to have the uterine action interrupted. For too deep a state of anæsthesia in general interferes with the force and frequency of the uterine contractions; while a lesser degree of the anæsthetic state leaves these contractions unaffected; and a still smaller dose often excites and increases them,—the effects, in this respect, of chloroform upon the uterus, being similar to the effects of opium in different doses. But the influence of the inhaled agent passes off in a few minutes, differing in this respect from the more permanent influence of a drug when swallowed; and if, at any time, the anæsthetic effect is too deep, and the uterine action is in consequence impeded, all that is necessary is to abstain entirely from exhibiting the chloroform for a short time, till the parturient contractions have been allowed to come back to their proper degree of strength and frequency; and then the anæsthetic agency is to be sustained as before, by giving the vapour with every recurring pain, but in smaller doses, or for a shorter time during each pain, than was previously practised.

Anæsthetic vapours, when given in large doses, have less power of reining up the action of the uterus in the last than in the first part of labour. And as the sensations of pain become more agonizing as the head is distending the perineum, and passing through the vulva, the anæsthetic state usually requires to be then rendered more deep and complete than in the earlier stages of the process; and in most patients this may be done without at all impeding the rapidity of the delivery. Indeed, in many women, this latter part of the process of parturition seems to be accelerated by the superinduction of anæsthesia; for the degree of relaxation of the muscular structures of the perineum and vaginal orifice, commonly resulting from it, usually more than compensates for any diminution of uterine action, that may occur. If in any instance it prove otherwise, and the depth of the anæsthetic state interferes too much with the parturient contractions, the simple remedy is that I have already mentioned—a diminution in the state of anæsthesia, so as to allow of a return and increase of the expulsive efforts of the uterus.

The degree and depth of anæsthesia which different patients are capable of bearing without the irritability and contractions of the uterus being impeded, appears to differ greatly in different persons. In some, a very deep state will still leave the uterus almost or altogether unaffected; in others, its action is interrupted by a comparatively slight degree of the anæsthetic state. It is this variability which at first forms the principal difficulty to those commencing the use of chloroform in obstetric practice. But experience and care will soon enable any attentive observer to overcome this apparent

obstacle, and to adapt the dose of the agent to the powers and capabilities of each different patient. I have never yet seen an instance, but I can conceive it possible, that in some rare exceptional cases and idiosyncrasies, the action of the ether or chloroform should, even in such small doses as merely produce unconsciousness to pain, interfere, especially in the first stage of labour, too much with the muscular action of the uterus, and require to be given up, at least till the labour be more advanced. But this would, of course, be no reason for not employing it in those other persons in whom it had not such an influence; any more than because opium occasionally does not act as an hypnotic on particular patients, it should not be given with that indication to any other patients with the view of inducing sleep.

During the anæsthetic sleep which chloroform induces in natural labour, the patient usually lies perfectly quiet and passive in the intervals between the pains, but moves more or less, and sometimes moans, as each uterine contraction begins to return. In the last stage she generally, with every recurring uterine contraction, makes the usual violent bearing-down muscular efforts, and the struggle can often be marked in the expressions of her face. The muscular action of the uterus and assistant muscles goes on, and yet she remains quite unconscious. The strictest quietude should always be observed and enforced around the patient, for noises and speaking, particularly soon after the chloroform is commenced, will sometimes excite and make her talk; and, if this happen, we may require to exhibit to her a deeper dose than would otherwise be at all necesssary. One or two practitioners of midwifery in London have averred and repeated, over and over again, in our medical journals, and in pamphlets intended for non-medical readers, that obstetric patients, under the influence of chloroform, must be liable to talk and act grossly and obscenely. This objection to the practice of anæsthesia in midwifery has been repeated and gloated over by those who have propounded it, in a way forming, apparently unconsciously on their own part, the severest self-inflicted censure upon the sensuality of their own thoughts. An impure mind, more especially in a professional man, may easily fancy and find impurities where none whatever exist; but he is not on that account entitled to imagine, that his own lewd thoughts are typified in the thoughts or actions of his patients. In answer to the supposed objection itself, I have merely to observe, that I never once witnessed any trace of any indecency, either in word or action, in any obstetric patient under the use of chloroform; and the evidence of one and all of my obstetric brethren, at whom I have inquired on the subject here, is to the same effect. In a paper on temporary delirium occurring in the course of labour, Dr Montgomery several years ago described more marked instances of effects of this description, arising merely from "the extreme distress and pain" to which the mother was subjected in the dilatation of the os uteri, &c., during natural parturition, than were ever seen to arise from the influence of means used to abate and abrogate that "extreme distress and pain."

In administering chloroform in obstetric practice, I have always used the handkerchief, as the simplest and best apparatus. Sometimes, when the case is likely to be tedious, I have it folded and sewed into the form of a deep cup or cone. The chloroform is poured into the bottom of the cup, the open end of it held over the nose and mouth of the patient when the action of the vapour is required; and, when its application is suspended, by closing the open end of the cone, the escape and loss of the vapour is prevented during the intervals. Such an arrangement saves the chloroform. But a handkerchief merely folded together and sprinkled with chloroform, answers quite well; and in the intervals it may be compressed together in the hand, so as to prevent the escape of the chloroform. In *first* throwing the patient over into the anæsthetic sleep —(the point which requires the most management),—a handkerchief thus presenting a large surface is often much more serviceable than one folded into a cup shape; for the patient, when first coming under the influence of the chloroform, is apt to move her head from side to side; and, in order to keep up the constant inspiration of the vapour, she can be more easily followed by using a simple handkerchief, than by trying to keep any kind of apparatus applied to her mouth or face.

The quantity of chloroform used varies both according to the duration of the labour, and the susceptibility of the patient. Usually, when the handkerchief is used, about an ounce an hour is necessary, a small quantity being poured upon it from time to time. A less dose will suffice in some, and others require more. In one case lately, where the patient, in a first labour, was anæsthetized for two hours, I expended nearly six ounces, large doses being necessary to keep her in a sufficiently deep state of unconsciousness. The first quantity which I pour on, usually amounts to three or four drachms; but I always judge by the *effects*, not by measuring the dose; and I pour on an additional quantity in a minute or so, if it be required. In holding the handkerchief towards the patient, I take care that plenty of atmospheric air is admitted,—and seldom or never put it in contact with the face. At first, it is better to hold it at a considerable distance, in order to prevent any chance of irritation and coughing; and then gradually approach it. It is always to be remembered, that the vapour of chloroform is nearly four times the specific gravity of atmospheric air; and if the patient is lying on her side, the handkerchief or pillow can be easily arranged so as to keep a larger supply of this heavy vapour opposite the mouth and nostrils. I have always held and managed the handkerchief myself in the first instance, and till the patient was asleep. Afterwards, I have generally trusted it to the husband or nurse, teaching them to apply it near the face when the pains supervened; and folding up the handkerchief in the way mentioned, so as to preserve the chloroform during the intervals.

When exhibiting chloroform in obstetric practice, and in the way I have described, I have been often struck by the circumstance, that

its use is very rarely followed by sickness or vomiting. I do not remember having seen vomiting follow its exhibition in labour in more than four or five cases, and two of these (in the practice of Dr Paterson and Dr Cochrane), were instances in which I was called in to apply the forceps, and where the patients were placed for the operation in a state as deep as that used in surgery. I have repeatedly seen it arrest the sickness and vomiting occasionally accompanying the first stage of labour.

In addition, let me state, that I have usually begun the employment of the chloroform when the os uteri was well dilated, or towards the termination of the first and the commencement of the second stage of the labour. But when the pains were severe I have commenced it earlier, and when the os uteri was still comparatively little dilated. There is, I believe, no limit as to the date of the labour at which we may give it.

REPORT OF THE RESULTS OF ANÆSTHESIA FROM DIFFERENT OBSTETRIC HOSPITALS AND PRACTITIONERS.

The following account of the results of anæsthesia in the practice of the Maternity Hospital, Edinburgh, has been drawn up by Dr Duncan and Mr Norris, two gentlemen who have acted as resident house-surgeons in the institution, and upon whose power and accuracy of observation all who are acquainted with them will place implicit reliance.

Since the use of anæsthesia in labour became general in the Maternity Hospital, shortly after the discovery of chloroform, 95 women in all have been delivered in the house under its influence. Among these, 88 were natural and 7 were morbid labours. In the 88 cases of natural delivery, only one of the mothers died, convulsions coming on five hours after delivery, and proving fatal after a continuance of six days. On a *post mortem* examination, the kidneys were found to have undergone, in some parts, the true stearoid degeneration.—(*See a report of the case, Monthly Journal for September*, p. 196.) Among the same 88 cases of natural labour, there were 5 dead-born children. In 2 of these cases, the birth was premature, being at the sixth month. In the third case, the mother had previously given birth to two dead infants. The fourth dead child had a very large hydrocephalic head. The proportion of still-born children was thus 1 in 17. In the Dublin Hospital the proportion of still-born children, as reported some years ago by Dr Collins, was 1 in 15. The 7 instrumental cases were as follows;—1 application of the short forceps, in an unsuccessful attempt to save the life of the child; the mother recovered well;—2 cases in which the long forceps were applied; one of the women, in whom the head was very long impacted in the pelvic brim, died from sloughing of the maternal passages; the other made a good recovery; both the children were born alive;—4 cases of version; one of the mothers died from rupture of the uterus, the others recovered quickly; 3 of the children were still-born, and in one of these 3 cases the cord was prolapsed.

In addition to these eighty-eight cases of anæsthetic delivery, there have been upwards of fifty women delivered in the house without chloroform. These have been chiefly very rapid labours, where the women have come into the hospital just in time to give birth to their infants, or where the house-surgeon has not been able to see them till very shortly before delivery. From

the expense attending a large consumption of chloroform, it has always been an object to husband it as much as possible;[1] and therefore, in the hospital, it has not been given in cases where the mothers did not very severely complain of their sufferings, nor were harassed with feelings of anxiety and fear.

On the whole, the results of anæsthetic midwifery, as observed by us in the Hospital, have been perfectly satisfactory; and we can confidently state that the recoveries have been altogether more perfect and speedy than before. This has been remarked in so great a proportion of the cases, that there can be no doubt whatever of the truth of the observation. Besides the increased rapidity of recovery, we have noticed the almost entire absence of those uncomfortable feelings of fatigue, languor, and shivering, and of that shattered feeling which so frequently comes upon the mother immediately after an ordinary delivery. Instead of this, we have found the mother almost invariably awake from the anæsthetic sleep comparatively fresh, easy, and cheerful. Not unfrequently the anæsthetic has been found to change, without an intermission, into a natural sleep, which may continue for an hour or two.

Further, there have been, since the introduction of chloroform into the practice of the hospital, far fewer than formerly of those violent attacks of rigors, ephemeral fevers or weeds, and abdominal pains, which are so common in most crowded hospitals, forming a class of cases which used formerly to cause much anxiety, and was a common cause of the mother's being detained in the hospital after the usual fortnight allowed for recovery. In fact, since using chloroform, there have been scarcely any women detained in the house by these causes, and much less Dover's powder, calomel and opium, abdominal fomentations, &c., have been used.

The women have been, invariably, found deeply grateful for the relief to their sufferings afforded by the anæsthetic influence of chloroform.—Yours, &c.

J. M. DUNCAN. H. NORRIS.

At a meeting of the Edinburgh Medico-Chirurgical Society in June last, along with other practitioners, I gave in a report on the employment of chloroform in midwifery. At that time, and since, I have been favoured with written statements of the results by various medical friends in Edinburgh, and in different parts of the country. I shall now give extracts from a variety of the letters which I have received relative to this subject. Many more such communications might easily have been called up and adduced; but I have deemed it useless to multiply unnecessarily this kind of evidence. It will be observed, that, with one exception (see the communication of Mr Lansdowne), the following letters refer—like the preceding statements regarding the Maternity Hospital—to the use of chloroform alone.

The first statement which I give is from my assistant and friend, Dr Keith.

I have employed chloroform in every case of labour under my care, since its introduction, with one exception; and also in almost every case to which I have been called in by other practitioners. In my own cases, amounting to about four-and-twenty, it has been given for a period varying from half an

[1] Perhaps, in a short time, a benevolent government will allow chloroform to be made cheaper, by removing the very high duty on proof spirit when used for medical and chemical purposes (tinctures, &c.) At present that high duty is, in one respect, a direct tax upon the relief of human disease and the mitigation of human pain; and a great obstacle to the progress of British organic chemistry.

hour to eight hours. The quantity of chloroform consumed has been, on an average, about one ounce per hour ; in a few cases double this quantity was found requisite. The anæsthesia has been in almost every case complete ; that is, the patient, on awakening, has declared, that while under the influence of the chloroform, she was utterly unconscious of all pain. In most cases the patient has lain quiet even during the pains, the presence of which is then generally indicated by the breathing becoming more rapid and somewhat laboured. In other cases there is suppressed moaning during the pains, or even, in some, loud manifestations of powerful straining and muscular exertion. I can state most positively that I have seen no serious symptom which could be traced to the chloroform, in any one case, either as affecting the mother or the child. Most of the mothers have made uncommonly good recoveries. Those who have had children previously, have, almost without exception, stated to me, that they felt very decidedly stronger after delivery than on former occasions. In two cases the recovery was rather slow, but this was owing to the patients' having been in a very delicate state during pregnancy,—and, in both instances, I considered the chloroform was of very great service, by saving their strength. All the mothers are now in their usual health.

In no one of the twenty-four cases was the child still-born. In one case labour was brought on at the end of the seventh month, owing to the brim of the pelvis being much contracted. The child was born alive, but died on the second day. All the other children are now alive. They have all been nursed by their own mothers, with one exception.

I have had occasion to use the forceps seven times since the introduction of chloroform, and once to break up the child's head and extract by the crotchet. In all these cases the patient was first put into a deep anæsthetic state, and in most she lay perfectly still and apathetic during the operation. All the mothers have done well, except in the case of craniotomy, where the uterus had ruptured previous to the use of chloroform.—Yours, &c. G. S. Keith.

From Dr Moir, Edinburgh.

Since the beginning of December I have, with a very few exceptions, used chloroform in the course of my midwifery practice, and I have not met with a single case where any unpleasant effects, either to mother or child, can be traced to its use.

As far as my observation has gone, I think it will be found that, in some cases, the chloroform, if freely administered at an early period of the first stage, retards the pains a little, and in others also lessens their power ; and when this does occur, the best remedy is either to intermit its use till the labour is further advanced, or to give it in smaller quantities and at longer intervals, so as not, at that stage, to induce complete unconsciousness. But, whether correct or not in this opinion, I am quite satisfied that the second stage is much accelerated, especially towards its termination, by the chloroform doing away with the resistance offered to the expulsion of the head by the muscles at the outlet of the pelvis,—and this to such an extent, that, in some *first* cases, there is a risk, unless very great care is taken, that the perineum be slightly lacerated, from the head being so rapidly expelled as not to give time to the parts to yield so rapidly as they would otherwise do. But this is comparatively a very rare occurrence, and requires to be mentioned principally with the view of putting young practitioners on their guard against it, and of leading them to use the necessary means to prevent it.

In exhibiting so powerful an agent as chloroform, I think it a point of importance to use as small a quantity as is compatible with the obtaining of its full anæsthetic effects; and, as this seems to depend much upon the rapidity with which it is conveyed into the system, it seems a point worthy of consideration to ascertain the readiest means of so doing. Various instruments have been invented for this purpose, though they have been almost universally superseded by the use of the handkerchief, as recommended by you, and used either in the form

of a hollow cone, applied again and again, after renewing the chloroform, over the nose and mouth of the patient; or simply folded up several times, and frequently having interposed between the folds a piece of wool or flannel, with the view of better retaining the chloroform. In both methods there is a considerable loss of chloroform, much of it being retained in the folds of the handkerchief, and much of it escaping without passing into the lungs, or else passing into them so slowly as not to produce the desired effect. I have, for some time, been in the habit of using a linen or white cotton handkerchief, folded only once, or, if very thin, folded twice; the point requiring attention being, that it should not be so thick as to offer any impediment whatever to free respiration when applied over the mouth and nostrils. Since using the chloroform in this way, I have never failed in rapidly producing the anæsthesia, either in my own practice, or when accompanying some of my patients (who were several months advanced in pregnancy) to their dentists to have one or more teeth extracted. For administering the vapour to patients who are in the erect position, the chloroform should be poured on that part of the handkerchief placed on the palm of the operator; the edge of the little finger should then be applied close to the chin, and the hand gradually raised up towards the mouth, till the sensation of choking which generally accompanies the first inspirations has passed off, after which, the handkerchief should be left on the face and the hand removed; the patient then breathes freely through that part of the handkerchief wetted with the chloroform, and, in general, half a drachm is sufficient to produce anæsthesia.

In obstetric practice, I find it the most convenient plan to place one end of the folded handkerchief under the left cheek of the patient, to pour a little chloroform on it, and then, taking hold of the loose end of the handkerchief, to bring it gradually near the mouth, till it can be left there without inconvenience, the patient breathing freely through it. And it is not necessary again to remove the handkerchief, but simply to pour on it occasionally a very few drops, whenever the patient begins to show symptoms of returning consciousness, or on the accession of a pain. The only precautions necessary are, to raise a small fold of the handkerchief from the skin when the chloroform is to be applied, so as not to blister the skin; and to drop the chloroform, not on that part of the handkerchief immediately over the mouth, but a little above it, so that the vapour, being heavy, may flow down towards the mouth or nostrils, and thus be, during inspiration, more readily received into the lungs. By adopting this plan, I have had the handkerchief applied for nearly two hours without removing it; and the quantity of chloroform I have used in single patients, has been much less than other practitioners have used in cases of the same duration.—Yours, &c.

JOHN MOIR.

From Dr Malcolm, Edinburgh.

Since November last I have employed chloroform in above thirty cases of labour, and with the most satisfactory and delightful results. A majority of these were first labours. I have kept my patients under it for periods varying from half an hour to six hours, and have never found the slightest unpleasant effects result from its use. All the children have been born alive, and are at this moment in perfect health, with the exception of one that died when about a month old, of a sudden and severe attack of dysentery. All the mothers have made recoveries with rapidity and completeness, far above the average which I had previously observed in my practice. This has struck me as the more remarkable, seeing a large proportion of my patients were primiparous; and I can only attribute this result to the entire absence of suffering and shock to the nervous system which is effected by the use of chloroform. Although in a few cases my patients and their friends have at first objected to the use of anæsthesia to abolish pains which they considered "natural," yet every one has afterwards expressed to me sincere gratitude for saving them from their agonies; and I am sure not one who has experienced the beneficial effects of the practice will ever submit to these agonies again, now that they know that they are so totally unnecessary, and can be so easily and safely abolished. I have repeatedly found the mothers

of my patients object to anæsthesia, as if they grudged that their daughters should not experience the same sufferings as themselves,—but I have uniformly found them afterwards as grateful as their daughters for the relief administered.

Generally, I have employed about an ounce of chloroform per hour. I have never seen the uterine contractions arrested by its use, although I have no doubt a large dose would, when necessary, have that effect. I have seen no case of hemorrhage, or convulsions, or any other complication whatever. Let me add, that I cannot conceive on what principle the employment of chloroform in natural labour should be objected to, as long as it is our duty, and assuredly it is our duty, as physicians, to relieve and mitigate human suffering.—Yours, &c.

R. B. Malcolm.

From Dr Thomson, Edinburgh.

After stating the details of ten cases, Dr T. remarks:—

Among the middling classes I have met with more difficulty in using chloroform than I had anticipated, as it has only been when the sufferings of the patient were very severe, or her friends had begun to dread the effects of prolonged continuous suffering on her constitution, that I could get my wishes carried into effect. That fatal Newcastle case, which was trumpeted a good deal in the newspapers, is still haunting their minds, and is very frequently urged by some timid friend when you propose the chloroform to relieve the sufferings of the patient.

My experience of it has been in all obstetric instances analogous to your own; with one exception, I have had no difficulty in getting the patient under its influence: a minute or two was in general sufficient to lay the most restless or ungovernable patient quiet on her pillow.

Not the slightest *post-partum* hemorrhage has taken place in my ten cases, though in the two forceps cases, where it was given deeply, the uterus remained flabby for nearly thirty or forty minutes, and threw off the placenta with difficulty.

Its relaxing effects are, I think, undeniable. In one case, the soft parts had resisted for a considerable time the descent of the head; they yielded very readily within an hour after the chloroform was begun. I have not had another instance of this kind lately; but, were I to meet with one, I feel confident it would yield with much more facility under the chloroform than without it.—Yours, &c.

Alex. Thomson.

From Mr Carmichael, Edinburgh.

I have given the chloroform in twenty-six cases of midwifery, four of which were first labours; the others varied from the second to the eleventh pregnancy. The quantity given varied from two drachms to four oz., and the length of time during which it was exhibited from a few minutes to four hours. The preparation I have used has always been that of Messrs Duncan, Flockhart, and Co., and I have never seen the slightest bad effects from it, either in midwifery or other medical cases, or in any case where I have administered it for amusement, except occasional sickness where it was exhibited shortly after a meal.

I have met with no case of flooding whatever. I have heard it alleged that it drives away the milk; but I have not found it so, as my patients have all been able to nurse, with the exception of one lady, who has not been able to suckle her child for the last three times.

In all of these cases it was administered with the greatest ease and with perfect success, and, in no case, with any bad results.

The recoveries have been certainly more than usually speedy. Indeed the only objection I have met with as to its use, has been on the part of the monthly nurses, who seem afraid that the new practice will curtail their attendance and pay.

All the children were born alive, and are doing well. The only case in which the child proved the least refractory was a footling one.

I have also found it most useful in cases of dysmenorrhœa, in spasmodic colic, and tic douloureux.

I also gave it, in a case which you saw along with me, of most severe neuralgia of the uterus, with the most perfect success.

In no case whatever have I seen any bad results of any kind arise from the use of the chloroform.—Yours, &c. W. S. CARMICHAEL.

From Dr Burn, Edinburgh.

I regret that I cannot give you the number of cases of labour in which I have exhibited the chloroform, but I may state that I have given it repeatedly, and have not seen any bad consequences either to the mother or child result from its use.

All the mothers made rapid recoveries, and the children did not appear to suffer from its use.

I have given the chloroform in three or four cases of adherent placenta where the uterus was firmly contracted, and had far less difficulty in extracting it than I have experienced in similar cases where the chloroform was not exhibited.—Yours, &c., J. BURN.

From Dr Purdie, Edinburgh.

I have now used chloroform in seventeen cases, which I have noted, and in every instance with decided effect, not merely by lessening suffering, but I am perfectly convinced, by the most careful observation, by shortening the duration of labour. The pains have never in my experience been interfered with, except by rendering them quicker, and far more effectual.

There is one of the cases which I would wish to recall to your memory. The patient, thirty-eight years of age, was in her first labour, which commenced early on Wednesday morning, and went on well but slowly till the evening, when its progress ceased, although the pains continued regular and strong. The os uteri was well dilated, but the head made no progress, although there was no very evident cause. About one o'clock on Thursday morning I sent for you to deliver her with forceps. On your arrival, you thought that still there was hope of the labour being terminated naturally. The patient, who was suffering much, was then put under the influence of chloroform and ergot, while you waited patiently for any advancement, for nearly two hours, without effect. You then delivered with the long forceps, which cost you great exertion, from the head being impacted in the brim. The patient's position was changed, the placenta was extracted, she was bound up and laid in a comfortable and easy posture, in which state she continued to sleep soundly until she was awakened after the child was dressed, the crying of which surprised her, as she had not been conscious of what had taken place from the time she got the first dose of the chloroform after your arrival. This patient had an excellent recovery. I never saw a patient suffer less after labour, or recover more rapidly. I may just add, that there are few things vex me more with regard to patients, than to witness the sufferings of a childbed patient, who will not allow, from ignorance or prejudice, the use of chloroform. Happily, however, such cases are very rare among us.—Yours, &c. W. PURDIE.

From Dr Finlay, Newhaven, near Edinburgh.

I have used chloroform in a considerable number of cases of natural labour. It was with much reluctance that I first administered it, and only at the urgent entreaty of a patient who was enduring intense agony before the birth of a first child. It was completely successful. Her screams had been audible across the street. In a few minutes they ceased, and she fell asleep, while the uterus continued to act as powerfully as before. She was not aware that she had got her baby until a quarter of an hour after it was born. In five of the other cases the

influence of the chloroform was as complete. In the other three cases *entire* insensibility was not induced, but the sufferings were greatly mitigated, and the relief was so evident to the patients, that, whenever the labour pain was approaching, they grasped with great eagerness the handkerchief on which the drug was sprinkled. Satisfactory as these cases have been, I have hitherto used the chloroform with considerable hesitation and caution, and only when it was asked for by the patient. But every trial has emboldened me to employ it with greater confidence on future occasions. In each of my cases the placenta was soon and easily detached; in none of them did hemorrhage occur; and they all made excellent recoveries.—Yours, &c. A. Finlay.

From Dr Cumming, Edinburgh.

I have now attended thirty-five cases of labour under chloroform, and it has been used in all with marked advantage. All the patients have made unusually good recoveries; and I have been very much impressed by the fact, which was remarked by the first patient submitted by me to chloroform, and repeated by all, that the convalescence was not accompanied by the crushed and dislocated feeling that they have experienced without it.

I always begin by introducing the chloroform slowly and gradually into the lungs, allowing a large proportion of air to be inhaled along with it. In every instance it was administered, not with reference to the quantity given, but to the effect produced,—this effect being complete unconsciousness during the pain; and thus administered, I have never seen any unpleasant or absurd consequences, nor any thing to excite alarm or even uneasiness.

Two of the cases had had large floodings in a previous labour; with the chloroform there was none. This, of course, I do not impute to the medicine; but it at least tends to prove, that hemorrhage is neither a necessary nor a likely consequence, as many at first were disposed to imagine.

All the children were born alive, and are so still. None of them as yet give the slightest indications of idiotcy, either present or future; nor have I observed in any the temporary stupefaction immediately after birth, ascribed to the presence of chloroform in the apartment, that some have remarked.

I am quite satisfied that, if properly given, it acts as a calmative; and I believe, from what has passed under my observation, that very many of what are called exceptional cases are not so in reality, but appear to be such from error in the mode of administration, and that further experience will amply demonstrate the truth of this.

In short, I am, unfortunately for the appearance of veracity, compelled to say, that all my cases hitherto have been so successful, the recoveries so uniformly good, and the satisfaction on the part of the patient (I may add also my own) so great, that I am rapidly approaching to, if indeed I have not already arrived at, the conviction, that, *if there be any sin connected with chloroform, it is chargeable on those who refuse to administer it.*

I may add, that not one of those patients who have already inhaled it will ever be denied it in any subsequent pregnancy, as they have repeatedly assured me; and certainly I shall not attempt to keep it from them, and that not more for their sake than my own.—Yours, &c. W. Cumming.

If necessary, I might have adduced more evidence in favour of the anæsthetic effects of chloroform in midwifery practice, from Dr Beilby, Dr Ziegler, Dr Weir, Dr Young, Dr Menzies, Dr Gilchrist, Dr Campbell, and other medical practitioners in Edinburgh who have been using it.

In order to vary the kind of evidence, I shall next adduce extracts from various communications which I have received on the

use of chloroform in midwifery, from medical correspondents in different parts of Scotland, England, and Ireland. It is needless, I believe, to attempt to arrange them in any special order; and I shall content myself, therefore, with beginning with the letters of those practitioners who live farthest north, and proceed southward.

The following is an extract from a letter written to me this summer :—

From Dr Grigor of Nairn.

Dr Allan of Forres and myself would as soon think of going to an obstetric case without our chloroform phial, as we would of going to bleed a patient without a lancet. In this quarter, doctors are only called in when things are going wrong, or in extreme cases; so that, since your grand discovery, he and I have only used it in about twenty-four cases, in all which it came up to all you have written about it,—no still-born children—mothers recovering well—fewer after-pains, &c. &c. One of my cases was a first child, the mother nearly forty-eight years of age, weakly in constitution, and of small formation. Had it not been for the chloroform, I do think she would have sunk.—Yours, &c.

J. GRIGOR.

Dr Dyce, lecturer on midwifery in Marischal College, Aberdeen, favoured me some time ago with the following interesting communication, regarding the obstetric employment of chloroform :—

I have reports from my friends Drs Harvey, Pirrie, and Gilchrist, all of whom, I was aware, had been employing it. I may at once state that I consider it a most invaluable agent; that I have every confidence in its safety; that I recommend it almost on every occasion; that no evil consequences have ever attended its use; and that I have found its effects nearly alike in all. I have used it eleven times. The labours have, with one exception, been natural. The exception was a breech case, and a first child. Two of the eleven children were lost; one was putrid, and in the other (the breech case), though the funis did pulsate for some minutes, the child could not be recovered.

Dr Harvey has given me a brief account of four cases delivered under chloroform; all the children were born alive.

Dr Gilchrist at Woodside writes me, "I have observed no evil results to the infants themselves from the use of chloroform." He does not state the number of his cases; but I presume they have been numerous from his remarking, that "latterly I have not used this agent so generally as when it was novel, reserving it now, unless when urgently requested by the patient, for cases unusually painful, whether arising from excessive sensibility of the system, rigidity of the soft structures, or cases requiring manual assistance."

Dr Pirrie has employed chloroform in fourteen cases. Ten of these were natural, two instrumental, and one a case of turning. He says, "as to the children, they have all been born alive, and continue to go on satisfactorily."

I never use it early in labour; generally the second stage has come on, or at least the os uteri is tolerably well dilated. This appears to be the practice of my brethren here. I then keep the patient in a state of insensibility more or less complete during the future progress of the labour. Occasionally I have allowed them to come completely out of the anæsthetic state; but, on the recurrence of the pain, the patients invariably and urgently seek for the handkerchief. When I used chloroform, I imagined that the intenseness of the stupor induced, did lessen the frequency and force of the pains, and even put a stop to the labour altogether; and I am still of opinion, that if the full effect is produced and kept up for any time, the pains will cease; but if a more moderate

effect is produced, so that consciousness to a certain extent remains, my conviction is, that instead of checking uterine action, its use enables the organ to act with more freedom; the mental influences are quickened; and, more than this, it has a decided power in relaxing the soft parts, and thus removing a very frequent cause of protracted labour. I had a very interesting case illustrative of these remarks in January last, in a lady in her fifth pregnancy, of a very irritable, anxious, and highly nervous temperament, who had previously to labour determined to use this wonderful agent. Her former labours had always been tedious, and very painful throughout every stage. This commenced and had continued for a couple of hours before I saw her. She was complaining much of the severity of the pains, especially of her inability to move from one posture. Her anxiety and agitation were very considerable; her pulse was above 100, and her body was already wet with perspiration. On examination I found the parts rigid, and the os not larger than a shilling, while the rectum actually encroached upon the vagina from its loaded state. I determined, therefore, on emptying the gut by an enema before employing the chloroform, and mentioned this to my patient, and gave the nurse the necessary instructions. I very soon found that I had added not a little to her already excited state, as she was not prepared for delay, and would only, after some persuasion, consent to its exhibition, and only then on condition that immediately after she was to be allowed the "stuff." Her request was complied with, and I confess to you its effect on her general state from the moment she was under its influence, not only gratified myself, but pleased her friends, who complained much of her impatience and imagined evils. A few inhalations sufficed to induce insensibility, before which she gave one or two hysterical laughs, and made some incoherent remark. She then became still and quiet, until a pain returned, —the only indication of which was a gentle writhing of the body. Her pulse very soon fell to 80, at which it remained; her skin became cool; the parts rapidly relaxed; she moved, and allowed herself to be moved in any direction readily: the pains were certainly more regular and efficient than in any of her former labours; and in two hours and a half from the first inhalation, the child was born. Once only during this period, and that was after one hour had elapsed, was she perfectly sensible. This was permitted at the solicitation of the nurse and friends, who had not seen chloroform used before, and could not be convinced that so sudden a change could exist and the labour progress perfectly. They soon had evidence of this, by the patient screaming out in her former impatient tone, "Pain, pain! where is the handkerchief?" From this time until delivery she was perfectly unconscious. The placenta came away naturally in ten minutes. The roller was put on, some of the soiled things were changed, and it was not for several minutes ere she was convinced that her trials were over. I need hardly say that she was gratified in the highest degree to find that she had escaped so much of her former suffering, and expressed her deep thankfulness for so valuable a boon. The child was putrid. Her recovery was perfect, and more rapid than on any former occasion.

In my practice I have met with no instance of chloroform failing in producing its usual effects in a very few minutes, where it was willingly and readily inhaled; in some nervous patients, a few seconds are sufficient for this purpose. In the case I have just given, I was surprised at the few inhalations she took; and Dr Pirrie mentions that one of his patients was so highly susceptible of its effects, that, on making three or four inhalations, the state of anæsthesia became so complete, that the labour was entirely suspended; he therefore, in this case, discontinued its use. On some occasions I had difficulty in persuading the patients to use it; hence a longer time elapsed. In one case when persuasion failed, I watched an interval of drowsiness, and in spite of herself brought the woman fairly under its influence, in which state she remained for twenty minutes, when the child was born unconsciously. Its cry

aroused her; she turned herself hastily around with an enquiring look, and after a momentary gaze said, "What is that? you have been giving me the stuff."

The quantity of liquid used has varied in my hands from ʒss to ʒiiss. This last quantity was expended in the case of a young woman, of eighteen years of age, pregnant of her first child, and who was more or less under the influence of chloroform for five hours. As this was amongst my first cases, if not the very first, and the longest period in which I have employed this agent, perhaps you will excuse my giving it somewhat in detail, more especially as ergot was given along with it, which I am not aware that I had seen any where recommended. Jane ——, æt. eighteen, unmarried; first child, December 18, 1847. When seen at five P.M., the membranes were reported to have ruptured twelve or thirteen hours before; on examination, the os uteri was found dilated to the size of half-a-crown, and the breech was discovered as the presenting part; the pains were frequent, irregular, at times only effectual, but they had been increasingly severe during the after part of the day, and now were strong.

About seven P.M., after some persuasion, she first inhaled the chloroform. She soon became insensible; it was then only presented to her at intervals as a pain recurred, a single inhalation being generally sufficient. She laughed and sang for a few minutes, but at length seemed perfectly unconscious, and we were only sensible that she had pain by her now and then rolling about, as is the case when labour-pains continue under convulsions. For an hour she was kept in this state; but thinking the pains were becoming more distant, and the progress of the labour slower, she was allowed to come out of this torpid state. Soon after ʒss. of powdered ergot was given, and repeated four times at intervals of a quarter of an hour. The pains now became stronger and more frequent, and my patient, who was conscious of the powerful agency of the chloroform, and the advantages she had derived, insisted on its repetition. She had, in fact, secreted the handkerchief, and was now keeping it at her mouth. But, alas! its talismanic powers had vanished; she threw it rather angrily at the student, who, at that moment, sat by her bed-side, desiring him to give her the bottle in its stead; she assured us that she had felt no pain until now, that she had been dreaming of her illness, which she believed was finished. From this time (nine o'clock) until her delivery, which took place between one and two o'clock the following morning, she was kept in a perfectly unconscious state, at times more so than at others, according to the severity of the pains. Towards the end of the labour the throes were very severe, but during none of them did the patient complain of the slightest pain. The only indication, as I have already said, of apparent suffering, was a movement of the body, and occasionally a low moan. The child in this case did not breathe or cry; it was not therefore legally in life, though the funis pulsated for some minutes.

I can scarcely think that the death ought to be attributed to the chloroform, or even to the ergot. The length of the labour, the nature of the presentation, and especially its being a first child, were almost against its being born alive. So that, with this exception, no injury has befallen the infant in the practice of any of us here who have employed chloroform.

I have only further to say, that the recoveries in every case have been most satisfactory. They appeared even more rapid than under ordinary circumstances.—Yours, &c. R. DYCE.

From Mr Lawrence, Montrose.

I am sorry I cannot furnish much information as to the employment of chloroform in midwifery in this quarter. It has been very little used by myself or brethren, owing chiefly to a very general prejudice on the part of our parturient patients against it. The very last case I attended, the patient resolutely refused it, although suffering very severe pain! However, in one of those in which I administered it, and to which I had gone unprovided with the chloroform, my patient compelled me to send for it! In one case in which

a patient of mine had convulsions in her first labour, I exhibited chloroform at her second confinement with the usual success.

I have seen no injurious consequences of any kind.—Yours, &c.

SAMUEL LAWRENCE.

From Dr Steele, Montrose.

I have used chloroform in midwifery practice on six separate occasions. In two of the cases, it was given to the extent of producing only a partial immunity from suffering; but with the effect of converting, especially in one to whom it was administered for about four hours, what would have been a very severe, into a very easy labour. In the other four the effect was highly satisfactory; two of the children being born without the consciousness on the part of the mothers, for some time after, of that event having taken place. One lady remarked, that she thought "the march of intellect had never taken a happier direction than when it led to the discovery of chloroform."

All the children did well. One of them, however, was asphyxiated for nearly a quarter of an hour after its birth.

From the little experience I have had of chloroform, any opinion of mine as to its merits is not entitled to much weight. It seemed certainly in several of my cases to prolong the interval between the pains, and thereby retard the termination of the labour; and I thought also that the uterine action was sometimes less effective when the patient was under the power of the medicine. —Yours, &c.

GEORGE STEELE.

From Dr Paton, Dundee.

I have ascertained that chloroform has been employed in upwards of fifty cases up to this date (5th June 1848), several of them instrumental, and with the best effect; and no bad consequence has attended it either to the mothers or children. All the children have been born alive. In the cases in my own practice in which I have used it, it speedily induced the anæsthetic state, and appeared to accelerate the uterine action in some. The recoveries of the mothers were more favourable than in former instances where no such agent was employed. In all, the children were born alive, except one, where the child was *acephalous*.

To explain the small number of cases in which it has been used here, I ought to mention that, in consequence of the deaths of two ladies in a respectable rank of life, from puerperal fever, when it was epidemic a few months ago—in both of which cases chloroform was used—the public, of course, attributed the unfortunate result to the new agent; and since then it has been difficult, and in many instances impossible, to overcome the prejudice against it. Of late, however, from the favourable opinion entertained of it by those who have had it administered, it is not so frequently objected to.—Yours, &c.

GEO. PATON.

Dr Anderson, president of the Medico-Chirurgical Society of Glasgow, and one of the most experienced and esteemed physicians in that city, some time ago wrote me a letter, from which I make the following extract:—

Not being connected with any obstetric institution, and having now little practice of this kind amongst the poor, I am sorry I cannot offer you any sufficient statistics on the use of chloroform; but I have had so much reason to be satisfied with its effect that I almost always use it, and most of my patients have expressed themselves as strongly in its favour as you have done. I recollect only two exceptions, where, although the usual effects were produced, both patients complained that, previous to the full anæsthetic effects, their feelings, instead of being agreeable, were very much the reverse. One

lady said, that she felt it like approaching insanity; and that she would rather endure the labour pains than be subjected to this again. In no case have I had reason to suppose that mother or child suffered injury. All the mothers have done well, and several of them have spontaneously remarked, that they made better and quicker recoveries than after former accouchements. In this I coincide. Two children were still-born. One, in the sixth month, died during parturition, after long-continued discharge of the liquor amnii for many weeks before the induction of labour; the other, in the eighth month, after hemorrhage from fatigue, followed by protracted labour: in this case a placental clot was found after delivery, so that the child's death was sufficiently accounted for.

On the whole, my experience of chloroform in parturition is more favourable than in other cases, where I have several times seen it produce alarming spasmodic and other nervous symptoms, followed by sickness. These effects, I think, are most apt to occur in subjects who have had spinal irritation, or an excitable state of the nervous system, or who use the chloroform when exhausted, or after abstinence from nourishing food.—Yours, &c.

A. D. ANDERSON.

The following communication was sent me in June last—

By Mr Spencer, Douglas, Isle of Man.

I forget how often I had used the chloroform when I last wrote you; but, since then, I have used it frequently, with invariable success so far as it is concerned. There have been no still-born children from its use, and all the children have continued to thrive well; none as yet have had a fit from teething—this I consider a great matter. We want some powerful proof here to bring it into general use. I cannot use it in half my cases, as I would not do it without leave from the patient. I had a case of puerperal mania the other day, and did *not* use it in it. If I had used it, I should have been blamed. This is so small a place that every thing one does is at once known. I have often operated under its use, with no ill effect.—Yours, &c.

J. H. F. SPENCER.

From Mr Ceely, Aylesbury.

I have much pleasure in communicating to you the general results of my very limited use of anæsthetic agents in midwifery. I have used both ether and chloroform in numerous operations, small and great, in public and private practice, have been several times the subject of their influence, and have repeatedly employed them in hysteria, epilepsy, and other nervous and spasmodic affections; but my employment of them in midwifery has been more limited, and at present restricted chiefly to primiparous and difficult or tedious cases. I have used them in only six cases as yet.

It has happened that most of my late obstetric cases have been so easy and so rapid as to supersede the use of chloroform. My own observations of its effects in midwifery, would induce me to *urge* its use in all *severe*, *protracted*, or *operative* midwifery; and in any case of natural labour, with only the ordinary suffering, I would not hesitate to employ it, with the concurrence of the patient or friends, if I saw no contra-indication; because I believe that, in proper doses, with suitable management, it is harmless and may be beneficial; but if the patient, under these circumstances, were averse, I should not, of course, press it. Neither do I think I should venture to employ it in natural and easy labour, where I suspected or knew that subsequent hemorrhage would arise, from flabbiness of fibre and flaccidity of the organ; because I think the *secale cornutum*, during the last hour or two of such labours, has in my hands been so beneficial, and seems more appropriate.

In all the cases in which I have employed ether or chloroform, the patients and their friends have been perfectly satisfied and gratified with the results, and abundantly thankful for the boon. In only one case has there been any

sickness or syncope. In a town about twelve miles hence, it has also been successfully used by Mr Knight of Brill, in several cases. In Bicester, I have heard of its use, but chiefly by younger medical men.

I will only add that, in using the chloroform, I have, in some of the cases, now and then omitted to repeat the dose, to show the friends and attendants how differently the patient was situated; but the latter has soon removed any hesitation of the friends by calling out hastily for that "nice chloroform" again.—Yours, &c. ROBERT CEELY.

In a printed pamphlet[1] obligingly furnished me some time ago by Mr Stallard, surgeon to the Leicester General Dispensary, that gentleman observes:—

I have exhibited the chloroform in upwards of thirty cases of midwifery during the present year, and they have included a greater number of severe cases than the general experience would support; this is accounted for by the fact that two of the worst were pauper patients, to whom the surgeon's attendance is required only in cases of unusual severity. One other formidable case also occurred to my father, and in it I was requested by him to use the chloroform. In this experience I have never once observed the least retardation of the parturient paroxysms, *so long as absolute insensibility was not induced;* and in several cases the pains were palpably increased. When a patient is suffering acute pain, she does all in her power to suppress it, and the act of doing so is well known to retard the process of parturition; when, therefore, the pain is no longer felt, the effort to suppress it no longer ensues, and the accouchement is accelerated in a remarkable degree. But again, it has been argued that the pain of labour is desirable; and that it is wrong to interfere with this natural indication of what is going on. Now, with this assertion I am totally at variance; there is nothing so depressing to the powers of life; nothing so soon exhausts the already feeble body; nothing so effectually prevents the sufferer from aiding nature by her volitional effort, as pain; and consequently nothing so powerfully maintains the strength, or invigorates the system, as its annihilation. . . . . In the thirty cases I have attended I have not had a single case of flooding, and two individuals had never been free from it on former occasions. This is, in my opinion, an additional recommendation for its exhibition; and in all those excitable persons in whom flooding is most likely to occur, the diminution of fear and excitement caused by its exhibition, has a direct tendency to prevent the flooding. Nothing but a very extended experience can justify any conclusion upon this head; as far as my own experience goes, I am decidedly of opinion that chloroform exerts no perceptible influence upon the child.

From Dr Protheroe Smith, London.

I have records in my own practice and that of my friends of upwards of 125 cases of anæsthetic labour; and, with one exception, all have done well. In several thus treated, no hemorrhage has ensued, though in previous labours there was flooding. In nearly all, the getting up has been more speedy, requiring no aid of opiates and purgatives; and it is my sincere conviction that chloroform lessens the chance of puerperal inflammation and fever. I usually employ my inhaler to administer it, and with it I fancy I can administer it with greater success and precision. I have kept patients under its influence from half an hour to twenty-eight and a half hours. I have used it in cases in which bronchitis was present, and one lately, in which, at the time, there was complete aphonia. She has never had so good a time—in four or five previous births she had protracted recoveries, floodings, peritoneal attacks, &c.; these she has wholly escaped. I have used it in turning and forceps cases, &c., and in

[1] Practical Observations on the Administration and Effects of Chloroform in Natural Labour, p. 16, &c.

all I have been most successful. In administering chloroform I adopt your plan of inducing rapidly complete sleep, and afterwards keep up the effect by repeating the inhalations at each recurrence of uterine effort. The other day I had three cases, the one after the other, which strikingly illustrated the various effects of the chloroform on different subjects,—1st, a primipara, æt. twenty (administered in the second stage), kept under its influence four and a half hours, slept sweetly without any movement of the body, save during the last expulsive efforts, when the usual abdominal muscles were called into action. The "pains" under the chloroform became more frequent and powerful, and a fine male child was born. After the expulsion of the child, the patient awoke refreshed as from a sweet sleep, without any pain or evidence (save the advent of her first-born) of having been confined, as far as her own feelings were concerned. The uterus contracted firmly, and I instantly left her for the second—a lady of title. She required to inhale for a minute and a half (in the third stage), before the induction of anæsthesia was complete, when she became very talkative, addressing her maid in French, speaking of matters wholly foreign to her condition, and of an amusing character, as though she was perfectly at ease and had nothing to do with the labour, which advanced rapidly, and terminated in an hour from the first exhibition of chloroform. On recovery, in about three minutes after, she was introduced to a fine boy, and remarked she had been sound asleep. The third case had the chloroform in the *second* stage; she slept soundly in half a minute, and then became conscious of what was going on—described to the nurse who stood by, the progress of the labour, and observed that she was greatly delighted that she had her senses, and yet was wholly unconscious of any pain or distress of any kind. She remarked that with the entire freedom from suffering she was sensible of the progress of the labour, as though it was effected by an agency with which she was altogether unconnected. During the last throe I made her sleep; so she was ignorant of the birth of a fine boy: in this and the second case the placenta immediately followed; the abdomen and uterus contracted firmly. In all three cases no after treatment was required, and they made rapid recoveries.—Yours, &c. PROTHEROE SMITH.

From Dr Rigby, London.

Of course I use the chloroform in midwifery, but I cannot think that a large dose at first, is the best way. In almost all private patients, you can draw the line between loss of pain and loss of consciousness. I give but a small quantity at a time; the patient takes it herself (on a strip of sponge), and when it is exhausted she asks for more.—Yours, &c. E. RIGBY.

From Mr Lansdowne, Bristol.

I have now used ether or chloroform in seventy-one midwifery cases; I have two modes of administering it, the one with a bladder, in which is placed a brass pipe with a stopcock, and into this is screwed, after I have poured the chloroform into the bladder, a piece of elastic tubing with a mouth-piece, the whole being pierced with a bore ⅛ of an inch, through which the vapour can be readily inhaled. If I find I am likely to be giving the chloroform for a long time, I use this apparatus, both for the sake of convenience, and also of economy, as ℥j will last me nearly or quite an hour with this; and, should I use it many hours, it not only effects a great saving of material, but does not so frequently require replenishing, and is always ready at the approach of each separate uterine action; and it may (as has been the case with me) be used by any friends, or by the nurse, should the practitioner require to be absent for a short time. The other apparatus is an inhaler, such as is commonly sold; it is made of a thin and pliable lead, adapted over the nose and mouth, having a piece of perforated zinc in its front, and containing a piece of sponge, over which the chloroform is thrown; the depth of this inhaler is such as to prevent

the nose being touched by the chloroform. It is home manufactured, not expensive, and very easy of construction. This latter I make use of if I am likely to be wanting it for a short time only; it requires to be supplied afresh every five or ten minutes, and, accordingly, I use ʒj or ʒss, which latter is my quantity when about to extract a tooth. If the action of the uterus causes great pain, as is frequently the case in an early period of the labour with the first child, I commence giving it as soon as the os uteri is sufficiently dilated for the head to pass; I have given it when the opening has not exceeded the size of half-a-crown. I believe it may be given with impunity as early in the labour as we please, and the only obstacles to its being so used that I can see, are the inconvenience to the medical attendant in being thus occupied with one patient for such a length of time, and also the very great expense which such a lengthened use of it must entail. On the patient's account, I can see no possible reason why it may not be used for a whole day, or even more; indeed, I cannot see why a limit should be set to the length of time in which it may be used. I have no doubt but that it will soon be the anodyne generally used at the latter stages of painful cancerous diseases. The greatest length of time in which I have used it has been 16½ hours, a fresh inhalation being made at every renewal of the action of the uterus; in other cases, I have given it 11½ and 12 hours, and the only reason of the inhalation being limited to this time, has been the cessation of the necessity for its use, namely, that the child has then been born, otherwise it would have been continued until such event had taken place.

I have found that nearly all my patients have recovered very rapidly; most of those who have had children previously, have been astonished at the unusual rapidity of their recovery.

I find no difference as to the expulsion of the placenta and the subsequent discharge, when administering chloroform, to what takes place in the usual natural labour. I have, upon two occasions, used it for very severe after pains, pains so severe that their cries could be heard at a considerable distance; indeed they appeared worse than the pains of actual labour; in both cases the pain was completely subdued by its use. Both these persons had determined not to avail themselves of the benefit of the chloroform during labour, neither did they, but they were delighted afterwards with its soothing effects. The former of these I had long resolved to give it to for this express purpose, it was her thirteenth child; her labours have always been very rapid, scarcely any pain accompanying them; but no sooner has the child been born than her agony has been almost past bearing, the pain recurring at intervals for a fortnight. Upon this occasion I gave it to her three times within the first ten hours, and she had nothing to complain of afterwards.

As regards sickness, I have not found that symptom, except where fluid has been previously taken; on the contrary, if the patient has been sick the chloroform has almost invariably checked it. The cramp I have not heard them suffer from whilst under its influence. I have never yet met with any thing which has caused me to regret having used it.—Yours, &c.

J. G. Lansdowne.

During the course of last winter and spring the practice of anæsthesia in midwifery was tried in London by a very intelligent young Russian physician, Dr Haartman of Hessingfors, while, for the sake of information, he was living as house-surgeon in the great Lying-in Hospital at Westminster. Dr Haartman subsequently visited Edinburgh, and kindly drew up for me, before leaving England, the following account of the results of his practice. I have much pleasure in publishing his letter, both on account of its own intrinsic value, and because the results of the practice of anæsthesia in this Hospital have been in no small degree misrepresented, both publicly and privately.

Results of Anæsthesia in the Westminster Lying-in Hospital.

I fulfil with the greatest pleasure your wish, in writing down my observations on chloroform as used in midwifery. I do it certainly not without much embarrassment, the greater the more I think of my ignorance of the English idiom; but, encouraged by your indulgence, I do not hesitate to begin the task, more especially as I know that some erroneous ideas of the use of the chloroform in the General Lying-in Hospital in London have been spread about.

During the three months I was house surgeon in that hospital, I had the opportunity of observing 105 women in labour, of whom about twenty-five were attended by my friend Dr Delafield of New York, the other eighty by myself. But having been most of the time the only house surgeon, and otherwise much engaged, I was not able to exhibit the chloroform oftener than in twenty-five cases, which, I need scarcely say, were in no way selected. The most of these patients were, however, *primiparæ*. In the administration of the chloroform I followed your method, using simply a towel, upon the interior of which the liquid was diffused; but I held it as a rule to exhibit it in the beginning of each case rather in small doses, and with caution, watching, in the mean time, the pulse as the surest indicator of any danger. But when the patient got accustomed to it I gave it freely, not removing the towel till the patient was in deep sleep. In this state, the respiration was in general sonorous, sometimes stertorous; the pupils were usually somewhat contracted, sometimes, however, dilated, sometimes not at all affected. The pulse was, in the beginning, either a little accelerated, or quite natural, I think, when the patient was not frightened. Yet, I have seen the pulse without any bad consequence go down to 40, when the chloroform was used for a long time or in large doses. Its strength was very seldom altered, being then weaker. The patient usually lay quiet, all the muscles being relaxed till the uterine contractions came on, during which they all, with few exceptions, pressed down as in common labour. Some of them complained, although they afterwards said, "they felt no pain whatever." Few talked or laughed, except the Irishwomen, whom I found rather refractory to the anæsthetic influence, probably because they are, in general, too much accustomed to drinking spirits. All the cases were, with few exceptions, observed with the greatest care, and both the duration, the frequency, and the nature of each uterine contraction, both before and after the chloroform was exhibited, were marked down. Thus, I find, by comparing all my annotations, arranged in tables, that, during the full effect of the chloroform, the uterine contractions became less frequent and, I should say, less powerful, but, when the effect had passed off a little, they then became more frequent and shortened in duration; for example, if the interval between two uterine contractions before the use of the chloroform was 6 minutes, and the duration of each $1\frac{1}{2}$ minute, both these periods were reduced after its exhibition. The interval, from 6 to 5, 4, or 2 minutes, and the duration of each pain from $1\frac{1}{2}$ to 1 or $\frac{1}{2}$ minute. Besides, I think, that in this state the uterine contractions in general came on more suddenly than in common labour, a circumstance which, however, may depend on an illusion, by the absence of pain existing and announcing the action of the uterus in the usual labour. I have never observed the uterine contractions quite arrested, yet often somewhat delayed. The supposed relaxation of the soft parts seems to me at least uncertain and accidental. In two cases in which I thought the chloroform had very much relaxed the rather rigid parts, I had, notwithstanding, rupture of the perineum. Among the twenty-five cases of chloroform, I had only in one a slight hemorrhage, a fact the more remarkable, as at the epoch at which I used chloroform most frequently, I can say I had hemorrhage, and that often very severe, in almost all the other cases in which chloroform was *not* used.

After the delivery, I found the patient in general quite unconscious of any

occurrence during the anæsthetic state, some of them saying, nevertheless, that they had felt something going on, although they did not know what. They seldom complained of headache, and it usually disappeared during the following day. The mothers recovered all speedily and perfectly, with one exception worthy your attention, although I am fully convinced that the accident by no means was the result of the chloroform, of which only two drachms were used—my provision at that time not being larger. During the use of the chloroform this patient was, as usual, insensible and quiet; but, for want of chloroform, the effect could not be kept up for a long time, and she was delivered with the usual pains. She was the following day perfectly well, and continued so till the 8th day after the delivery—she had then complained to the other patients of headache. I was, however, not called for before she, in the afternoon, had had a fit of what is commonly called apoplexia nervosa; after which she got paralytic on the right side. I need not to give you a tedious description of the proceeding, and the treatment of the disease; allow me only to state that the patient began, by the use of strengthening medicine, to walk about in a month.[1]

Of all the 105 children, six were either still-born, or died sooner or later after the birth—two only of them belonging to the twenty-five cases of chloroform. But, before I relate these cases, I beg permission to point out some general remarks. In the majority of the cases I could not discover any change whatever upon the child, some of the children being rather bloodfull, others anemic—states depending, I think, as usual, on the duration and the nature of the labour. In one case, however, in which Dr Ferguson was present, the newly-born infant was found rather strange, or, to use the eminent doctor's expression, "tipsy." It breathed less frequently and more abruptly than usual, the sound of its scream being rather singular. It recovered, nevertheless, perfectly. I must here observe that the mother (Irish), who, after the confinement, had brought up a large quantity of gin and broom, confessed having taken spirits before admittance into the hospital. Of the two children who died, the first was rather weak and thin, but continued pretty well till the seventh day, when it got peritonitis, and died on the tenth day after birth. The second, a large and fat child, died four hours after birth: in this case, when the head was born, I observed around the neck small blue spots, which I found to be blood extravasated under the skin. After birth the same kind of spots began to appear over nearly the whole of the body. The child breathed well in the beginning, but would not suck. It died without convulsions. By the *post mortem* examination, the spots were found to be blood, extravasated from the capillary vessels of the skin; a large quantity of half-coagulated blood was found in the cavity of the peritoneum, and small blood coagula in both lateral ventricles of the brain. The lungs and the brain were slightly congested. The blood seemed to be quite natural, and had no anormal smell.[2]

[1] The "puerperal paralysis" was in this, as it is in most other cases, probably connected with albuminuria. One of my patients, who was confined for the second time, four months since, was attacked two years ago with hemiplegia immediately after the birth of her first child. I have a patient at present under my care, from Forres, who several years ago was attacked with hemiplegia at the time of delivery.

[2] This special case of purpura has been often mentioned. Every one acquainted with infantile pathology knows that purpura is not a rare occurrence at birth, as the cases and observations of Andral, Billard, Otto, Lobstein, and Graetzer, &c., amply prove. A patient who came some time ago from London to be confined in Edinburgh under my care, was gravely assured by a distinguished London physician, that if chloroformed, her child would be sure to be, as they all were in the Westminster Hospital, "either dead born, or with their blood in a putrid and dissolved state!" The want of the head in the anencephalic child born at Dundee (See Dr Paton's letter, p. 235), might have been as logically ascribed to the mother's use of chloroform, as the purpura in the above instance.

Before I finish, I beg to add that I have used the chloroform in a case of turning with the greatest advantage. I was called out from the hospital to a poor woman in labour, and found a presentation of the cord and the uterus so contracted that I could nowhere introduce my hand, although I made repeated attempts without success. I then gave a large quantity of chloroform, and I was quite astonished at the great change which took place, it being now uncommonly easy to perform the turning.—Yours, &c.

CHARLES DE HAARTMAN.

Of the use of chloroform in midwifery in Ireland, I have few details. From Dublin, my friend Dr Tyler wrote me in June last, that the practice has not yet been much tried. He observes—

As to the progress of chloroform in obstetric practice here, I regret to state that its virtues have not yet been fairly put to the test by any of our Dublin accoucheurs, owing to a dread of bad consequences resulting, although I am unaware of any case followed by such being brought home to it, except a rumoured one of *threatened* convulsions and spasms, where ——— was administering it.

As to the surgeons, I hear them all speak in the highest terms of it. The resident surgeon of Steven's Hospital, Mr Wilmot, told me yesterday that he administers it previously to every operation there, now in some hundred cases, without meeting with any untoward result.—Yours, &c. A. TYLER.

Subsequently Dr Tyler informed me that Mr Shekleton, master of the great Dublin Lying-in Hospital, was trying the effects of chloroform. While this sheet was passing through the press, Dr Denham, assistant physician in that hospital, visited Edinburgh, and informed me that Mr Shekleton has now exhibited chloroform in upwards of forty cases of labour, most of them of an operative or instrumental kind, and that the effects have been such as to induce him to go on with the trial of it. No unfavourable results, either as regards the mothers or children, have been observed to follow the use of the chloroform. Some of the principal private practitioners in Dublin, are also, Dr Denham informs me, beginning to employ the practice.

Regarding the use of chloroform in midwifery on the Continent of Europe, I possess little or no information. Dr Krieger of Berlin, tells me that in that city most medical men are opposed to the practice of anæsthetic midwifery, but still, five or six accoucheurs constantly make use of it. Writing to me in July last, Dr Krieger says—

About five or six accoucheurs of this place, I don't think there will be more of them, use chloroform in almost every case they attend; many more only in cases of morbid labour; the majority not at all. The cause of this curious occurrence—*curious*, because in *every* surgical operation chloroform is made use of—may be found, not only in the disinclination of the public at large to such extraordinary means in quite a natural process (as they take labour for), but perhaps also in the expense, chloroform being still a costly article. I don't know whether the preparation we get here is less strong than it ought to be, or not, but I seldom require less than six drachms or an ounce, sometimes more, for one delivery, and the price is as much as half-a-crown per

ounce at apothecaries' shops. Professor Martin of Jena has used ether in seven, chloroform in ten cases of morbid labour, and cannot strongly enough recommend the latter anæsthetic agent, stating, that he never witnessed but favourable consequences. I am sorry I cannot give you any more details about the progress chloroform has made in Germany—but the perplexing political affairs have so very bad an influence on scientific publications, that we get but very little medical news from any where on the Continent, and those only such as were long prepared before we fell victims to revolutions.—Yours, &c. E. Krieger.

In a letter which I have lately received from Vienna, the writer, Dr Arneth, informs me that the use of anæsthetic agents in midwifery is, as a general rule, discountenanced in that city by the two leading professors of midwifery in the University, but that, in several late cases, they have used it in operative delivery. He further states, that in Würzburg the use of chloroform in obstetric practice is more common. He states—

While I am writing this letter, Professor Kiwisch of Wurzburg tells me, that in that place no woman is confined, neither in private nor in hospital practice, without having been chloroformed. In two cases of eclampsy he saw decided effect of these vapours, viz., the fits subsided.—Yours, &c.
F. H. Arneth.

The preceding kind of evidence, in relation to the practice of anæsthesia in midwifery in this and other countries, might, as I have already stated, be very easily increased and multiplied by a little more extended inquiry, and by an appeal to the experience of the numerous accoucheurs here and elsewhere that have employed it. But the evidence as it stands—and spontaneous as it is in most cases—is amply sufficient to show both the great extent to which the new practice has already been adopted, and the great success that has attended upon it. Every innovation in medicine which implies, like the present, a violent and extensive change in existing doctrines and old established practices, has always been, for a length of time after its introduction, stoutly decried and resisted. The history of the first introduction and subsequent progress of the three greatest modern improvements in practical surgery, midwifery, and medicine—viz. the ligature of arteries, the induction of premature labour, and the discovery of vaccination—afford sad but strong historical proof of this observation; and we have many minor instances of the same constant enmity to change, in the bitter opposition to which the first employment of antimony, ipecachuana, cinchona bark,[1] and other

[1] The London physicians have, on several occasions, specially distinguished themselves by their determined and prejudiced opposition to all innovations in practice not originating among themselves. In the whole Pharmacopœia, there is perhaps no one remedy which, at the present day, is acknowledged to be of greater value, or to have saved more human lives, than cinchona and its preparations. In the seventeenth century, the proper time and manner of using the cinchona bark, for the cure of the then prevalent intermittent fevers of England, was made out by Robert Talbor, a medical practitioner in Essex. When Talbor subsequently removed to London, and began to use with success the new remedy

medicines encountered. And I believe that I am correct in stating, that probably no innovation, embodying so very direct and decided a deviation from all the former routine and rules of practice, as the employment of anæsthesia in midwifery implies, ever, in the same short period, made such extensive way and progress as it has done among the profession. As a matter of course, however, it has called forth also abundance of published and unpublished opposition and objection. No small share of the resistance against it has taken the form of personal or professional abuse of me as the introducer of the practice. All that I most willingly pass over and excuse, as, judging from all past experience in medicine, it was nothing more nor less than I was entitled to expect under the circumstances. But some difficulties and objections of a more palpable nature have been urged against the practice; and, in conclusion, I will very briefly allude to, and attempt to answer, the more prominent among these that happen to have been brought forward.

ALLEGED DIFFICULTIES IN THE SUPERINDUCTION OF ANÆSTHESIA.

In a previous page, I have stated the principal circumstances which require to be attended to in the exhibition of chloroform in labour. I have been occasionally told of cases, in which it was supposed that it was impossible to produce the anæsthetic effect of this agent. In my own practice (and I have now used chloroform in many hundred persons), I never yet met with a single instance in which a person was proof against its full influence.

It has been sometimes averred in the English journals, that on attempting to use chloroform, jactitation, incoherent talking and delirium, spasms, &c. &c., have supervened *instead* of a state of quiet anæsthesia. These are symptoms which do occasionally come on in the *first* or exciting stage of its action, more especially if strict

in the cure of the common agues of the metropolis, he found that, as he gained the favour of the world, he lost that of the physicians of London; and apparently their persecution of him became such that the King at last interfered, and in the year 1678, King Charles II. sent a royal mandate to the College of Physicians, commanding them not to give Talbor "molestation or disturbance in his practice." Among the list of London physicians averse to the new practice of curing ague by cinchona bark, De Bergen mentions the illustrious names of Sydenham, Harvey, &c. In 1698, a Dutch physician, Dr Groenvelt, published a work entitled, "De tuto cantharidis in medicina usu interno." A few years previously, viz. in 1693, when Groenvelt practised in London, the President of the College of Physicians imprisoned him in Newgate for daring to recommend and use the new remedy whose virtues he had discovered. Six or seven years after vaccination began to be generally used throughout England, Dr Moseley, a member of the London College of Physicians, suggested to his College the propriety of putting down "the beastly new disease," as it was termed, of cow-pox; and in 1805, he boasted that the middle and inferior classes of London had then "renounced the delusion." In the last Number of a respectable London medical journal, a London medical practitioner questions whether the practice of relieving women, by anæsthetics, from the pains and agonies of parturition, should not "be considered criminal according to law!" See *London Medical Gazette* for September 8, p. 424.

quietude is not enjoined; and, though they are apt to terrify the beginner, they are in reality no more serious in their effects and character, than some of the equally frightful symptoms sometimes seen in hysteria. They are an evidence, however, of one of two things; either that the vapour is being given too slowly, or that it is given in too small quantity,—in an exciting, in fact, instead of a soporific dose; and the simple remedy, as every one properly experienced in its action knows, is at once to increase the dose in order to pass the patient as speedily as possible into the *second* or full narcotic stage.

Chloroform, it is alleged, sometimes gives rise to much coughing, and pulmonary irritation. Certainly not so, if the chloroform is of good quality, and its vapour is not at first approached in too strong and concentrated a form to the face of the patient. After some experience, it will be found that it can be given so as seldom or never to induce even coughing. Some time ago, a well known physician, in a large city of the south of England, wrote me, that he and his townsmen had found it too dangerously irritant a substance to breathe, and that he had seen it produce cough, bronchitis, phthisis, &c. The answer was simple; it never produced any such effects in Edinburgh practice. And I believe that the explanation was equally simple; he and his townsmen had experimented with an imperfect and impure article. A few days ago, one of the principal druggists in Edinburgh showed Dr Christison, Dr Douglas Maclagan, and myself, a bottle of chloroform of high specific gravity, viz. 1490, which he had just received from a very large manufacturing chemical house in London. It was impossible to breathe it without feeling great irritation in the throat and chest. It emitted fumes that at once reddened litmus paper; and which, on examination, proved to be muriatic acid. Is it wonderful that bronchitis, coughs, and more serious disasters, should have followed the inhalation of such an improper and dangerous article?

Dr Letheby has shown, that some kinds of chloroform in the market, besides containing muriatic acid, are also mixed with aldehyde, hydrochloric ether, hypochlorous acid, &c.[1]

[1] See *Medical Gazette* for June 16, 1848, p. 1038. The presence of some of these deleterious agents has been supposed to be an inevitable and speedy effect of the spontaneous decomposition of very pure chloroform. But I find that some of the article, manufactured several months ago in its purest form by Duncan, Flockhart, and Co., of this city, has undergone no kind of change, even though long exposed to the sun. Messrs Smith have also shown me the same, in regard to their chloroform. I have reason to know, that the dangerous article alluded to in the text as containing a quantity of muriatic acid, has been extensively offered to the profession, at a price two or three shillings per pound cheaper than is charged for the pure chloroform manufactured by other houses; and, probably, its very cheapness has led to its extensive use. The following is the formula by which chloroform is prepared by Messrs Duncan, Flockhart, and Co. of Edinburgh, whose article I have always found of the most superior quality:—4 pounds of chloride of lime, and 12 pounds of water, are first well mixed together, and then 12 ounces of spirit added. Heat is then applied to the still (which ought not to be more

OBJECTIONS TO ANÆSTHESIA IN MIDWIFERY.

Objections of various kinds, religious, moral, and medical, have been zealously brought against the practice of anæsthesia in midwifery.

Elsewhere[1] I have attempted to answer the supposed religious objections that were at first so very strongly urged in various quarters against the practice, on the supposed ground of the permanence of the primeval curse; and I have shown that the disputed word "sorrow," *'etzeb* ("in sorrow thou shalt bring forth children"), does not in the original Hebrew really signify the sensations of pain, but the muscular efforts and contractions connected with childbirth. Besides, if this were not the fact, and that it was the duty of man to give effect to the curse, instead of struggling to ameliorate and resist its penalties and influences, then the whole art of physic should require to be abandoned entirely, for, in the primeval curse, man was doomed to die; and yet is not the great leading aim and object of the physician a continuous attempt to preserve him in life? All forms of obstetric assistance would require also to be rejected, for the whole art and science of midwifery is one undivided effort to abate and ameliorate the effects of the curse; and to attain that object imperfectly, as heretofore, by venesection, baths, by counter pressure to the back, and other minor practices, is as sinful as to attain it more perfectly now by anæsthetics, inasmuch as the principle of interference is not altered by the degree of relief given; "for whosoever shall keep the whole law, and yet offend in one point, he is guilty of all." In short, if there is any evidence of feelings of impiety and irreligion in the whole question, it is surely on the side of those persons who suppose that pain is permanently ordained in the primal curse as an accompaniment of human parturition; and yet that by anæsthetics, man, the creature, has discovered a power by which he can alter and subvert an immutable decree of God, the Creator.

The principal moral "objection," as it has been termed, against the employment of anæsthesia in midwifery, amounts to the often repeated allegation, that it is "unnatural." "Parturition," it is avowed,

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than a third full), but as soon as the upper part of the still becomes warm, the heat is withdrawn, and the action allowed to go on of itself. In a short time the distillation commences, and whenever it begins to go on slowly the heat is again applied. The fluid which passes over separates into two layers, the lower of which is chloroform. This, after having been separated from the weak spirit forming the upper layer, is mixed with half its measure of strong sulphuric acid, added gradually. The mixture, when cool, is poured into a leaden retort, and distilled from as much carbonate of baryta by weight, as there is of sulphuric acid by measure. The product should be allowed to stand over quicklime for a day or two, and repeatedly shaken, and then redistilled from the lime. The specific gravity of the resulting chloroform is generally 14.96 or 14.97.

[1] See also Dr Protheroe Smith's late pamphlet entitled, "Scriptural Authority for the Mitigation of the Pains of Labour."

is a "natural function," the pain attendant upon it is a "physiological pain" (Dr Meigs),[1] and it is argued that it is impossible "to intermeddle with a natural function;" and to use anæsthetics is a piece of "unnecessary interference with the providentially arranged process of healthy labour" (Dr Ashwell).[2] The above is, perhaps, the most general and approved of all the objections entertained and urged at this moment against the practice of anæsthesia in midwifery. But it certainly is a very untenable objection; for, if it were urged against any of our similar interferences with the other physiological functions of the body (every one of which is as "providentially arranged" as the function of parturition), then the present state of society would require to be altogether changed and revolutionized. For the fact is, that almost all the habits and practices of civilized life are as "unnatural," and as direct interferences with our various "providentially arranged" functions, as the exhibition of anæsthestics during labour. Progression upon our own two lower extremities is a "providentially arranged" function, a "natural process;" and yet we "unnaturally" supplement and assist it by constantly riding on horseback and in carriages, &c. The "physiological process" of walking is apt to produce pain and injury of the uncovered foot of man, and we "unnaturally" use boots and shoes to bind the foot, and add to the protecting power of the cutaneous and other structures of the sole. Mastication and digestion are "natural processes;" but we daily intermeddle with and attempt to aid them by the arts of cookery and dietetics; and so on with regard to other functions.

To annul the pain of labour by anæsthetics is, argues Dr Meigs, "a questionable attempt to abrogate one of the general conditions of man." Riding and railway travelling abrogate one of the general conditions of man (progression), and are constantly leading to accidents and deaths. Should we never travel therefore except on foot? Disease and death itself form one of the most "general conditions of man,"—and medicine is a "questionable attempt to abrogate them." Should medicine therefore be at once abandoned?

In a note now lying before me, an eminent London divine urges the following objections against anæsthesia either in midwifery or surgery; and I notice it here, because it is an objection which I have often heard repeated. He writes:—"The question with me is not the alleviation of pain, but the destruction of consciousness. I should hesitate greatly to take a step which destroys consciousness." Now, certainly, our consciousness is destroyed in natural sleep as much as in the anæsthetic sleep. I have little doubt that the distinguished writer whom I have quoted, has many a day, perhaps during almost every day for a long lifetime, voluntarily given up and destroyed his own consciousness in sleep, for an hour or

[1] Philadelphia Medical Examiner, March 1848, p. 152.
[2] Lancet for March 11, 1848, p. 291.

two longer each morning than the necessities of his system required. Putting these many hours together, he has perhaps now, from first to last, unnecessarily but voluntarily, surrendered up his mental consciousness for periods that, if added together, would count up weeks, and months, and perhaps years. He has done so too, merely for the reprehensible indulgence of indolence; and yet he insists upon his fellow creatures not surrendering up *their* consciousness for a short time on rare and extraordinary occasions, when the object is the far more legitimate one of the avoidance of unnecessary physical pains, and the securing life and health by saving the system from the endurance of these pains. If we may sleep, and thus indulge in the destruction of consciousness to avoid and cure corporeal fatigue, surely we may do the same to avoid and cure corporeal agony.

Dr Merriman[1] opposes the employment of anæsthesia in natural labour, on the ground of "the great superiority of allowing nature to conduct the whole process of the birth." But the practice of anæsthesia does, in reality, allow "nature to conduct the *whole* process of the birth:" it merely abstracts that intensity of pain and suffering which accompanies the act of labour in the civilized woman, —a "disadvantage inseparable" from civilisation, to employ Dr Merriman's own expression, and which is not an essential part of the process of parturition, according to his own doctrine; for, as he himself states, "in the earliest ages of the world, and in savage nations at present, childbirth appears to have been, in almost every instance, *easily* accomplished; the mother suffers *little*." And in this state of natural anæsthesia the convalescence is consequently unusually rapid; for again, to quote Dr Merriman's words, she almost "at once resumes her ordinary occupations." Dr Merriman afterwards, in speaking of the use of chloroform, decries its propriety in any except "instrumental or very tedious labours,"—arguing that we should not interfere unless where the labour is morbid, for (to use his own words), "the duty of the physician is to *imitate* nature as far as possible, and watch her methods of acting." But surely the physician strictly imitates nature in her most natural state, according to Dr Merriman's own premises, when, during labour, he induces by art that state of anæsthesia, which, in Dr Merriman's opinion, originally pertains to parturition in the human mother. For the female in an uncivilized state more truly shows us the true method and types of nature, than the female in a civilized state. Besides, are we not called upon to relieve the woman, when we can, of her sufferings, as an act of common professional duty and common professional humanity? In law and in morals, we judge greatly of actions by their intent. No accoucheur would intentionally inflict upon a patient the agonies of labour by a deliberate act of *commission* on his

[1] Arguments against the Indiscriminate Employment of Anæsthetic Agents in Midwifery. London, 1848.

part. Is an accoucheur properly justified in intentionally refusing to save a patient from the agonies of labour by a deliberate act of *omission* on his part? When a child, at birth, is intentionally destroyed by the loss of blood, it does not matter, in the eye of the law, whether the death has been produced by voluntarily omitting to tie the umbilical vessels, or voluntarily opening other vessels.

Up to within the last few months, and till the power of annulling pain by the inhalation of ether was discovered, severe bodily pain, such as we witness in surgery and midwifery, was universally regarded by the profession as possessing an evil and morbid effect. Some of the opponents of anæsthesia have taken up a novel and different view; and, as a medical argument against the practice of anæsthesia in midwifery, it has been particularly averred that a labour pain is "a desirable, salutary, and conservative manifestation of life force" (Dr Meigs). Parturient "pain is (says Dr Copland)[1] often salutary as respects its effects; neither its violence nor its continuance is productive of injury to the constitution," &c. No opinion, I believe, could be more erroneous. I have already shown in this Journal, from the evidence of extensive statistical returns, that some of the graver operations of surgery are now much less fatal in their results when patients are operated on under the condition of anæsthesia, and consequently without any attendant pain, than the same operations were formerly, when patients were submitted to all the agonies of the surgeon's knife in their usual waking and sensitive state. The prevention of the pain in surgical operations is, in other words, one means of preventing danger and death to those operated on; the saving of human suffering implies the saving of human life. And what holds good in relation to pain in surgery, holds good in relation to midwifery. Pain, whenever it is great in degree or great in duration, is in itself deleterious; and by shielding our patients, by anæsthetic measures, against the more severe portion of the pains of parturition, we not only preserve them from the agony of their more immediate sufferings, but we preserve their constitutions also from the effects and consequences of these sufferings. And the evidence which I have adduced in the preceding pages, that, when thus freed from the endurance of pain by artificial anæsthesia, they assuredly, as a general rule, make both more rapid and more perfect recoveries than when such means are not used; just as woman in a savage state, and where she enjoys a kind of natural anæsthesia during labour, recovers more easily and rapidly from the shock of labour than the civilized female. In short, in cancelling the pains of parturition by anæsthesia, we also, I believe, to a great extent, cancel the perils of the process; for the mortality accompanying labour is regulated principally by the previous length and degree of the patient's sufferings and struggles. In the Dublin Lying-in Hospital, when under Dr

[1] Dictionary of Practical Medicine, Vol. III. p. 434.

Collins' able care, out of all the women, 7050 in number, who were delivered within a period of two hours from the commencement of labour, 22 died; *or* 1 *in every* 320. In 452 of his cases, the labour was prolonged above 20 hours; and of these 452, 42 died; *or* 1 *in every* 11,—a difference enormous in its amount, and one surely calculated to force us all to think seriously and dispassionately of the effects of severe suffering upon the maternal constitution.

The last and certainly the principal objection against anæsthesia in midwifery, is the supposed danger accompanying the exhibition of anæsthetic agents. In the earliest paper which I published on the subject of chloroform in this Journal, I pointed out this circumstance in mentioning various cautions in the use of it. When we consider the immense extent to which it has already been employed in all quarters of the world, in medicine, surgery, and midwifery,—the little care sometimes observed in its use,—and the deleterious and dangerous articles with which, as we have seen (p. 245), it is sometimes mixed, the wonder is that so few alleged accidents have happened from its employment. By saving a vast amount of human suffering, it has already, I believe, been the means of saving no small amount of human life; and it is assuredly improper to argue, as some have done, that the mere chance of its disagreeing with some rare and special constitution, now and then, is any valid reason for refusing its use for the abatement and abrogation of human suffering. If there were any soundness in the reasoning, a thousand things beside would require to be abandoned. Railways, steamboats, stage-coaches, &c., when used as substitutes for the natural and physiological function of human progression, are ever and anon attended with accidents to limb and life. But surely no one would, from this, maintain that these means of conveyance should, in consequence be abandoned. Many persons are annually drowned in bathing.—Should bathing, therefore, be prohibited, and this powerful means of maintaining and restoring health be entirely forsaken? According to the Registrar's official returns, a great number of lives[1] are lost yearly in England by the improper medical use of opium.—Should the use of opium, therefore, be given up? Patients sometimes sink under the depressing action of antimony, calomel, &c.—Should these valuable drugs, therefore, be banished from the Pharmacopœia? Many a patient has perished in consequence of venesection.—Should this operation be expunged from the art of surgery? From mistakes and errors, &c., in diagnosis and practice, medicine and surgery are sometimes the unhappy means of destroying instead of saving life.—Should these arts, consequently, be interdicted? Published works on medical subjects have sometimes led both pa-

[1] In 1840, out of every 1,000,000 living in England and Wales, 24 were poisoned by opium, and 22 by other medicines improperly given to children below the age of five years alone.—(See *Seventh Annual Report*, p. 82.) See Taylor on Poisons, p. 187, &c., for the great numbers destroyed in England by opium, &c., improperly given.

tients and practitioners into serious and fatal errors.—Should no medical works, therefore, be allowed to be published? Long ago Raynalde, in sending forth the first work on midwifery ever published in the English language, seems to have foreseen that, against the utility of publishing books on midwifery, the same argument would be used as we have found in our own day used against the application of anæsthesia to midwifery; and he has answered the argument in a style so earnest and apposite, that I shall quote this reply, by anticipation as it were, in his own words, in an abridged form :—

"Lo!—such is the light judgment of them, the which in every thing, whereof may ensue both good and evill, have alwaies their eyes waking and firmly affixed and directed upon the evill, picking and choosing out the worst of every matter, omitting and leaving to speake of the best, as the thing which were nothing to the purpose. If every thing in this world should be weighed and passed upon after this sort, then should we be faine to condemne and banishe those things farre from us, which are, at this time, accompted and taken for the most necessary, worthie, and of greatest price or estimation. To be short, there is nothing under heaven so good but that it may be perverted and turned to an evill use, by them that be evill and naught themselves, and so abuse it: ne is there anything so absolute and perfect, but by the occasion of the abuse thereof, at one time or other, may and doth ensue great danger and damage to mankinde? Fire and water be two right necessary elements to the use of man, without the whiche he could not live: yet by the meanes of them many a miserable deed hath been done and perpetrated. By fire hath bin consumed and devoured whole cities and countries; by water, swallowed and drowned infinite men, shippes, yea, and whole regions. Againe (he continues), meate and drinke, to the moderate users thereof doth minister and maintain life; and contrary, to the unmeasurable and unsaciate gourmands and gluttons it hath full many times brought surfeet, sicknesse, and at the last, death. * * * * But (he argues) should men, for the avoyding of the aforesaid inconveniences, and for the reasons above said, condemne and banish fire and water, or forsake their meate and drinke? No, it were but madnesse once to think it. Therefore I say, the judgment of that eye can never be equall and indifferent, which hath more respect and regard alwaies to the displeasures and hurts possible to happen (only through the misuse of a thing) then to the emoluments and profites daily and commonly like to ensue to the wel users of the same: and that of it selfe is good, is never to be disallowed for the sake of them that do abuse it. For to them that love good themselves, whatever it be, it is to them a sufficient matter and occasion therein to seeke the glory of God, and the only profit of their fellow Christian: And contrary, such as be of ill disposition, will in every thing, be it never so good and salutary, pick out matter of maintenance to their lewdnesse, turning matters of discretion to foolish and peevish prating contention."—(*Prologue to Raynalde's Birthe of Mankinde*, Edition of 1626, p. 9.)

ARTICLE II.—*Observations on Gunshot Wounds, made in Paris during the Summer of* 1848. By EDWARD WATERS, M.D., F.R.C.P., Edinburgh, formerly Senior President of the Royal Medical Society of Edinburgh.

(*Read before the Parisian Medical Society, August* 9, 1848.)

A MORE extensive field for the observation of gunshot wounds than that presented by the Parisian hospitals, after the fatal struggles of

February, and more particularly of June last, has rarely offered itself to the profession, under equally favourable circumstances for study; and it is to be hoped that the opportunity thus afforded will be made available, in reducing to some general rules of action the discordant opinions which have hitherto prevailed in this department of surgery.

Different and even opposite methods of treatment have been pursued by surgeons of high reputation, and a discussion has been opened at the Academy of Medicine to enable them to develope their views, and record the results of their experience; a solution, however, of the numerous questions involved in the subject is not to be looked for.

The following communication is the result of observations collected in various services. In it an attempt will be made to estimate the value of certain modes of practice, and to throw light on some points, in connexion with gunshot wounds, respecting which diversity of opinion still exists.

CHARACTER OF THE ORIFICES OF GUNSHOT WOUNDS.

The profession generally, guided chiefly by the opinions of military surgeons, till within a recent period adopted the belief, that the orifice of entry of a musket or pistol ball was, in all but exceptional cases, distinguishable from that of exit; that the former presented a clean equal border, somewhat depressed, and more or less contused; the latter a more or less irregular one, projecting outwards, and evincing less contusion; further, the exit of the ball was supposed to be marked by a larger orifice than its entrance.

M. Nelaton agrees in opinion with the military surgeons; but insists on the different appearances which the orifices present, according to the period at which they are examined.

When the wound is recent the orifices offer notable differences; that of entry is depressed, ecchymosed, contused; that of exit is somewhat prominent and lacerated;—the former presents a loss of substance; the second a simple solution of continuity, the flaps of which, if brought together, would almost completely close the wound; the irregularity of the flaps and borders renders the latter orifice the larger, notwithstanding the absence of loss of substance.

After the lapse of some days the case is different; the eschar becomes detached from the orifice of entry, which is thus proportionately enlarged; on the contrary, the flaps, bordering that of exit, become partially united, and this orifice is diminished accordingly.

In a medico-legal light these distinctions possessed great weight; recent observations materially diminish their value. There can be no doubt that, where all the distinctive signs enumerated exist, their testimony is conclusive; but unfortunately it is found that, in a very considerable proportion of cases, some of the signs are absent, and under such circumstances there is abundant evidence to prove, that frequently no positive deduction can be drawn from those present.

Different from what occurs in general engagements, the combatants in the streets of Paris often saw the hand that struck them, and were thus enabled to state positively the point of entrance of the ball. On these data, tested by all the circumstances throwing light on the inquiry, such as a knowledge of the position of the patient at the time of being wounded, the direction of the wound, &c., it is clearly established that, in numerous instances, the orifice of entry exceeded in size that of exit, and that its form was more irregular. M. Roux stated before the Academy, that, in his cases, he observed about an equal number of variations on the one side as the other; an observation which may be extended to the practice of other surgeons. In a case of gunshot wound of the thigh with only one orifice, in the service of M. Boyer, the ball had traversed the muscular parts transversely at the upper third, and remained lodged beneath the skin on the side opposed to that where it penetrated. The ball, extracted by incision, was perfectly round, notwithstanding which, the orifice, that necessarily of entry, presented an irregular border. In other instances, in the same service, where the limb was completely traversed, not the slightest difference in character between the two openings could be detected on the most careful examination directed specially to that end.

M. Blandin appears to have been the first to question the correctness of the rule thus impugned; his experience in gunshot wounds has long convinced him that the aperture of entrance is, in all but exceptional cases, the larger, and experiments performed by him are said to have proved the accuracy of his observation. According to him, the ball where it strikes the skin being supported by parts more or less resistant, acts as a punch, and produces an aperture nearly equal to its diameter; at its point of exit, on the contrary, the skin gives way before it, stretches in virtue of its elasticity, and is only traversed when it has attained its utmost degree of extensibility; the loss of substance is thus rendered very trifling, and the aperture consequently smaller than that of entry. M. Gerdy maintains the same opinion; and in this case also it is founded not merely on the examination of the wounded, but on numerous experiments performed on the dead subject and on living animals; in some of these the two orifices were precisely similar. It may be objected that results obtained on the dead body and inferior animals cannot be considered an exact criterion of what occurs in man during life; it is difficult, however, to conceive experiments better devised to solve the question, and, when conjoined with the facts observed in the Parisian hospitals, they constitute a body of evidence, which seems to place it beyond doubt, that in a great number of instances the orifices do not afford conclusive characters by which that of entrance may be distinguished from that of exit.

No satisfactory explanation of these conflicting facts has been advanced; but it is not improbable that the distance at which the wound is inflicted may affect its appearance. Thus when the shot

is fired at a short distance, the wadding, being carried along with the ball, may render the orifice of entry larger and more irregular than that of exit, in consequence of the wadding having been deposited in its passage through the soft parts; when the shot is fired at a greater distance, the absence of the wadding may render the two orifices nearly or exactly alike; if the ball in its passage come in contact with a solid substance, such as bone, its form may be altered, and its exit give rise to a jagged opening with projecting borders.

When a ball is nearly spent, as shown by the distance to which it penetrates, where there is only one opening, this not unfrequently resembles a simple incision, a circumstance observed in some cases. That the point of exit should occasionally present a similar character is therefore easily explained.

ANALOGY BETWEEN GUNSHOT WOUNDS, BURNS, AND POISONED WOUNDS.

The older surgeons believed, that in gunshot wounds the balls, being heated by the deflagration of the powder, cauterized the tissues they traversed. The groundlessness of this belief was demonstrated by A. Paré in a very simple manner; he showed that a ball might be fired through a heap of powder without igniting it.

Rejecting the theory of combustion, M. Velpeau insists that burns and gunshot wounds occasion analogous lesions, inasmuch as the contusions resulting from the latter may be followed by all the effects of the former, from simple erythema up to death of all the structures implicated. One of his patients was struck by a musket-ball on the chest; the projectile caused no wound, but simply a contusion, terminating in the formation of an eschar: without information it would have been impossible to pronounce what had occasioned it. In a similar manner the comparison might be extended to the effects of cold, which is equally capable of causing identical phenomena; redness, vesication, ulceration, eschar, and death; the only difference being, that in this instance the effects are secondary, dependent on reaction, and not primary.

With respect to the opinion originated by Vigo, that these wounds are poisonous, M. Velpeau considers that, though as regards the theory it will not bear the slightest examination, still in practice it should not be altogether thrown aside, as after some days these wounds contain a sort of poison, in the putrilage resulting from the elimination of the eschar, and which is liable to be absorbed.

DILATATION OF GUNSHOT WOUNDS, UNCONNECTED WITH FRACTURE.

The practice of dilating the orifices and canals of all gunshot wounds once prevailed. John Hunter was among the first to oppose this useless and injurious proceeding, and the force of his judgment and reasoning speedily showed itself in the general adoption of

his views by British surgeons. Foreigners of eminence, amongst whom Larrey *père*, and Sanson, may be mentioned, followed in the same track; still, long after the abandonment of the practice amongst us, it continued to be inculcated on the continent, where the influential example of Dupuytren contributed powerfully to postpone the period of its rejection.

The confidence of the profession in its utility is now so completely overthrown, that any recapitulation of the objections to the proceeding is unnecessary. M. Baudens, the celebrated surgeon-in-chief of the military hospital of Val-de-Grace, declares the threatened tension, to obviate which the scarification was practised, to be exceedingly rare where the ball has traversed soft parts without producing fracture, and stigmatizes it as a scarecrow; his testimony is the more valuable as he once entertained a contrary opinion. Imbued with the doctrines imbibed during his studies in Paris, and acting under the dictates of what he considered to be an imperative duty, he, on his first arrival in Africa, dilated all the wounds, though not without feeling acutely for the sufferings he inflicted. While acting thus, a *voltigeur*, with the two thighs traversed by a ball at their superior third, without fracture, was brought to him. To effect dilatation large and deep incisions of the four wounds were requisite. His courage failed him; cold water dressings were applied; and, while dreading the consequences of his conduct, the cure progressed steadily and rapidly.[1] From that moment he abandoned the practice, and the propriety of his having done so will not at the present day be disputed. Several cases are recorded where flesh wounds treated on this principle have been observed to heal by the first intention; when this happens, it seems certain that the contusion, ordinarily produced by the passage of the projectile, cannot exist. It has been attempted to explain the occurrence by supposing, that in these examples the projectile had acquired an angular form, and consequently acted as a cutting, and not as a contusing body. A more simple explanation is based on the fact already recorded, that, when the force of the ball is nearly exhausted, its contusing power is lost, and it produces, notwithstanding its spherical form, a simple incision. This view derives corroboration from the circumstance, that the orifice of exit and the portion of the wound near it have been seen to heal by the first intention, while the remaining portion passed through the slower process of separation of a slough, suppuration, and granulation.

M. Velpeau denies that gunshot wounds can heal without the separation of an eschar. He asserts that all the tissues touched by the ball are disorganized, and must be thrown off, and that the premature closure of the external orifices, by imprisoning the putrid

[1] The conversion of M. Baudens, which shows to how late a period this practice was advocated in the surgical school of Paris, is recorded by Sir George Ballingall in his "Outlines of Military Surgery," 2d ed. p. 216.

detritus, becomes a source of great danger. Of the applicability of this reasoning to the immense majority of cases, there can be no doubt; but the value of those of an opposite bearing is not on that account in any degree affected, and indisputable facts are mentioned by Larrey, Sir George Ballingall, Sanson, and others, where the cure was completed without any separation of eschar.

The utility of the rejection of scarification is not limited to the conditions already detailed. It has been extended with the best results to cases where important articulations have been traversed. The following occurred in the service of M. Jobert at St Louis :—

Salle St Augustin, No. 68.—Delevie, soldier, aged twenty-five, of good constitution and sober habits, wounded June 23, 1848. He was struck by a ball in the popliteal space between the heads of the gastrocnemius muscle; it traversed the articulation of the knee-joint and emerged in front, having passed through the patella at its inferior portion without fracturing it. The wound did not occasion any marked general disturbance; no examination by probe or otherwise was made in the tract of the ball; the knee was kept perfectly at rest, and dressed with cold linseed meal poultices, frequently changed during the day and night; horizontal position on a pillow; no splint. Slight effusion took place in the articulation; but the inflammatory symptoms were exceedingly moderate, and speedily got under. The patient suffered no pain; the orifices of the wound gave no evidence of suppuration, and on the 25th of July cicatrization was complete; there was no swelling of the joint, and he was allowed to move about with the assistance of crutches. The movements of the limb were preserved; extension of the leg was performed without difficulty, but flexion occasioned some uneasiness.

M. Jobert is of opinion that some slight adhesions exist within the joint, occasioned by the development of false membranes. In the wards of the same surgeon, two other cases exist in which the articulation of the knee-joint was traversed, where the same treatment has been equally successful. An analogous one is said to be seen in the service of M. Gosselin. A similar case happened in the practice of M. Vidal de Cassis, and is related in his work on "External Pathology."

A soldier, wounded in July 1830, had the articulation of the knee traversed by a ball, which entered in front above the patella and emerged posteriorly, immediately below the popliteal space. In consultation with M. Guersant *fils*, he decided on not dilating the wound, and the patient was perfectly cured. There was neither local nor general disturbance; the patient affirmed that he suffered nothing from the wound, the progress to cure of which was remarkably simple. The suppuration was so trifling, that without the attention bestowed on the wound, it might have been considered to have healed by the first intention.

It seldom happens that balls traverse articulations without serious injury to the articular surfaces; and examples of such success are, therefore, necessarily rare. M. Vidal de Cassis was not acquainted with one at the time at which he wrote. In two instances of wound of the same joint in the service of M. Jobert, the result was not so fortunate; and in the wards of M. Velpeau there are also two examples where the suppuration is very abundant. The fortunate ter-

mination in the cases that have been mentioned is not the less to be attributed to the treatment; and the good effects of non-interference with cutting or other instruments, are thus strikingly exemplified, and afford every encouragement for the adoption of the same course under the like circumstances. Dilatation of a wound of a synovial membrane is at least as formidable as amputation.

Where, therefore, a ball has traversed an articulation without material injury to the bones,—injury of which would be betrayed not only by local, but also by sympathetic constitutional disturbance,—no exploratory attempts should be made, but the strictest repose enjoined, and appropriate treatment adopted, in order to ward off or diminish inflammation.

BALLS LODGED IN THE BONES.

Hôpital St Louis, service of M. Jobert. Salle St Augustin, No. 7.—Boy, aged sixteen, wounded June 23, was struck by a musket-ball near the inferior third of the right leg anteriorly; the ball buried itself in the body of the tibia, without otherwise fracturing the bone; no attempt at extraction was made; the ball remained in the bone; the external wound marched steadily towards cure; on the 25th of July cicatrization was complete, and a day or two subsequently the patient left the hospital. The integuments covering the tibia anteriorly in its superior half presented extensive cicatrices, the result of necrosis of a considerable portion of the bone, caused by the fall of a piece of iron on his leg. This was followed by a long suppuration, and the extraction of considerable fragments. The patient had recovered from the effects of this accident previous to the infliction of the present wound, which occupied a portion of bone exempt from and inferior to that previously implicated.

Some rare cases have been observed where balls have remained lodged in bones without causing any disturbance; a preparation exists in the Musée Dupuytren, in which a musket-ball is encysted in the substance of the tibia. The subject from whom it was taken was a French soldier wounded in 1814. Percy mentions the case of an old carabineer who carried a ball in the tibia up to the time of his death, a period of twenty-five years; a few others are on record, but their small number constitutes a *prima facie* argument in favour of the conclusion, that balls cannot occupy such a position without causing symptoms which demand their removal; numerous cases are related where, after a shorter or longer period, their extraction was thus necessitated.

M. Jobert maintains a contrary opinion, influenced by the observations of balls lodged with impunity in the bones of animals, the hippopotamus, elephant, stag, &c.; he maintains that the same may equally take place in the human frame, and consequently condemns their extraction, as causing unnecessary torment and retarding the cure, unless when they are superficially situated, and can be removed without difficulty. In this he is opposed to nearly all surgeons. M. Baudens unites with others in insisting on their extraction, and, where the forceps is unavailing, prescribes the application of the trephine. Dupuytren directs their extraction when practic-

able, as it is only very rarely that they do not occasion grave accidents; caries and necrosis almost always follow. Guthrie establishes the same principle; according to him, their presence in the extremities of bones gives rise to caries—in the body of long bones and in flat bones, to necrosis.

The practice of M. Jobert in these circumstances, is based on so small a number of facts that it stands little chance of being adopted. Arguments, founded on what is observed in animals, are admitted to have but a very limited application to man; for, were it otherwise, ligature of the aorta would be proved to be a practicable operation. Blandin once failed in killing a sheep, though he opened the arterial trunks of all the extremities and the neck.—

TREATMENT OF FRACTURES OF THE EXTREMITIES IN CONNEXION WITH GUNSHOT WOUNDS.

Two distinctly opposite methods are pursued in the treatment of fractures caused by gunshot wounds, and these extremes are connected by the practice of those who follow a middle course. An examination of the results and merits of these different methods will now be entered on.

In order to facilitate the consideration of the subject, these fractures will be divided into, *first—simple*, the term being used not in the ordinary surgical sense, but as indicating that the bone has been fractured without the detachment of fragments; and, *secondly—comminuted*, where this complication exists.

The *second class* is that in which the difference of treatment is most striking, and will be first treated. It offers several points for examination. These are:—*a.*—Dilatation and extraction of splinters. *b.*—Dressing and apparatus. *c.*—Cases demanding amputation. *d.*—Cases demanding resection. These questions will be discussed separately.

a.—Dilatation and Extraction of Splinters.

The various circumstances under which the splinters of bone characterising the second class of cases present themselves, modify the treatment, and a division of them which indicates these circumstances is therefore indispensable; that of Dupuytren answers every purpose, and will be here followed. According to him, the fragments are of three kinds, namely,—primary, secondary, and tertiary.

Primary fragments are those which are entirely separated from the bone and soft parts during the infliction of the wound.

Secondary fragments are those which are not completely detached from the bones and soft parts, being still connected to the latter by portions of tendon, muscle, ligament, &c.

Tertiary fragments, those, namely, which result from the contusion of the bones by the projectiles in the portions which border the point of fracture, and which nature produces in virtue of a particular process, ordinarily exceedingly long in completion, and lasting some-

times ten, fifteen, or twenty years. Dupuytren relates the case of a soldier wounded in 1813, and admitted into the Hôtel Dieu in 1830, in consequence of tertiary fragments following a gunshot wound received at that period.

Among the surgeons of Paris, MM. Jobert and Baudens respectively occupy the extremes of the scale referred to as marking the treatment of these injuries, and are distinguished by the zeal with which they support their opinions.

In all gunshot wounds M. Jobert adopts a uniform and exclusive mode of practice; he deprecates all interference with the wound in every instance, even in cases of comminuted fracture. He not only condemns the extraction of splinters of whatever kind, and leaves their discharge entirely to nature, but interdicts formally all examination of the wound. He combats local inflammatory symptoms by antiphlogistic measures, and, when the tension becomes excessive, relieves it by two longitudinal incisions, from three to five inches long, and penetrating through the skin and cellular tissue, practised on each side of the orifices at a distance of about two inches;[1] he invariably avoids dilatation of the wound itself. He justifies this treatment by the advantage obtained in preventing the access of air to the fracture, with which that obtained from the extraction of the fragments cannot, he contends, be compared. He further maintains, that direct benefit is obtained from the presence of the fragments—from those even that are completely detached—in that they contribute to the consolidation of the fracture, and prevent the formation of false joints; and he believes that in many instances useful limbs may thus be saved which would otherwise be sacrificed. The patient, it is admitted, may occasionally suffer for a time from the presence of the necrosed portions; but these are gradually eliminated, and the limb ultimately regains its original power. In other instances, in accordance with his views in respect to the lodgement of balls in bones, he maintains that the necrosed portions may occupy the callus, and remain for an indefinite period without giving rise to any annoyance. To support the correctness of these views he adduces the following cases:—in one of these the patient was wounded in February. The humerus was fractured comminutively near its head; the ball passed out; no dilatation nor extraction of fragments was practised, but some portions of bone were removed during the suppuration. An enormous mass of callus is developed around the seat of fracture, and consolidation has taken place. The callus incloses portions of dead bone, and a fistulous opening on the outer surface of the arm communicates with the seat of fracture. The patient retains the use of his hand, but is unable to raise or support the

[1] When a wound heals with difficulty, in consequence of the ablation of a considerable portion of the skin, M. Jobert adopts the same practice. In a case recently in his wards, where the breast had been removed on account of a tumour, these incisions were practised with excellent effect.

weight of his arm, which he carries in a sling; he suffers but little, and his health is good. The remaining patient had the humerus fractured by a musket-ball at the upper third, during the days of June; no examination of the wound nor extraction of fragments was practised. Consolidation of the fracture is now complete, and the wound perfectly healed. The only deformity of the arm is occasioned by the exuberance of the provisional callus, which is believed to contain necrosed portions of bone. The patient continues to support his arm in a sling.

M. Jobert attaches great importance to these cases, regards the results as among the finest ever obtained in surgery, and as fully justifying him in not extracting fragments of any description.

M. Baudens pursues an entirely opposite treatment. In all cases of fracture caused by gunshot wounds, his first care is to ascertain the nature of the parts by direct examination; and if the existence of splinters be established, and there be any hope of saving a useful limb, he immediately places the patient under the influence of chloroform, practises the necessary dilatation, and extracts all the fragments of bone, whether primary or secondary; his object in this being to convert, as nearly as may be, these formidable wounds into simple ones.

The first authorities, among whom Dupuytren may be cited, have always ascribed the local accidents attendant on these fractures to the presence of the splinters, which, acting as foreign bodies, light up and aggravate inflammation. In several cases M. Baudens, guided by the character of the local symptoms, the tension and discoloration of the skin, inflammation in short putting on a phlegmonous appearance, assumed the presence of splinters, and confirmed his diagnosis by forthwith administering chloroform to the patient and extracting them; in such cases he then, with a razor, practised long superficial incisions, not embracing the whole thickness of the skin, in order to relieve the engorgement, and applied ice to diminish the action.

It is thus shown that M. Baudens extracts all foreign substances, and that he not only removes the primitive or detached fragments, but also the secondary, or those still connected with the soft parts. Till lately no difference of opinion existed as to the propriety of this practice in the first class; but with regard to the second the case was different. Dupuytren, who advocated the immediate removal of the first, inculcated in his lectures, that in the greater number of cases the second were not to be extracted, unless when the operation could be performed without danger of hemorrhage or severe pain, and then only when the fragments had been more or less detached by suppuration; that is to say, at the termination of a variable period, eight, ten, fifteen, twenty days, a month, or even later. M. Baudens removes them at once, and his long experience in Africa gives great weight to his opinions.

Two objections only are urged by Dupuytren against the latter mode of proceeding; one is the severity of the pain attendant on the

operation, and he has recorded a case where he was forced to desist from researches which he had commenced, owing to the sufferings of the patient. The anæsthetic properties of chloroform completely overrule this objection; it is only necessary to witness its employment in these cases, to acknowledge unhesitatingly its beneficial effects.

The remaining objection, that, namely, of the danger of hemorrhage, alone therefore demands consideration. This danger, as resulting from the extraction of the splinters, seems to be more apparent than real. In reference exclusively to fractures of the extremities, it is difficult to conceive any grounds for dread in this respect, where amputation would not be directly indicated; were it required, however, to proceed to their extraction in comminuted fractures of the thigh, the depth of the parts might seriously complicate and impede the operation. On the other hand, cases exist where the presence of the fragments seems to have been the cause of hemorrhage.

Case fifty-three, in the work of Dupuytren, supports this conclusion; it occurred in July 1830. A ball fractured the two bones of the leg at the superior third; an attempt to preserve the limb was determined on. The orifices of exit and entry were dilated, the limb fixed in a fracture apparatus, with the looped bandage, and laid on pillows. Every thing went on well; the fever was moderate; the patient suffered little, and a healthy suppuration established itself, which daily became more abundant. The general condition of the patient continued pretty good, no rigors, no diarrhœa, no colliquative sweating, no pain in any region of the body. Such was the condition the 16th day after the accident, when, after a dressing performed with the greatest gentleness and precaution, a violent hemorrhage of a bright red colour manifested itself, and amputation of the thigh was performed.

Examination of the amputated leg.—The tibia and fibula were fractured comminutively at their superior fourth; fifteen or twenty fragments belonging to one and the other of these bones, and almost all detached, occupied the seat of fracture; the soft parts were torn, destroyed, and reduced to a pulp; purulent collections existed between the different muscular layers; the anterior tibial artery was altered by putrefaction, and presented an opening at its passage through the interosseous ligament.

This is a most instructive case; the condition of the parts at the time of amputation, showed how delusive were the hopes of success which had been entertained during the treatment. The artery was either opened directly by the action of a splinter on its altered coats, or by ulceration resulting from the inflammation entertained by the fragments of bone.

Ligature of the femoral was agitated and rejected. The advocates of M. Jobert's treatment, may attribute the unfavourable result to the dilatation of the orifices, permitting the admission of air to the seat of fracture; in the following case this practice was not adopted, and yet the result was equally unfortunate.

One of the wounded of June was admitted into the hospital of St Louis, under the care of M. Malgaigne, with comminuted fracture of the humerus at its upper fourth, from a musket-ball. No dilatation nor extraction of splinters was attempted; there were two orifices. July 10th, considerable hemorrhage manifested itself, which was arrested temporarily by compression; it recurred again

and again, and the following morning disarticulation at the shoulder joint was performed. The patient was in a weak blanched state, the operation was executed while in bed, chloroform was administered; a very trifling amount of blood was lost, but death supervened during the operation; the union of the flaps was in consequence not effected.

Three other cases of secondary hemorrhage have occurred in the practice of this surgeon, where no examination of the wound had been performed, and all terminated fatally.

The necessity of examining the wound in these fractures was forcibly exemplified by its condition in the present instance; numerous detached fragments occupied the seat of fracture, the surrounding soft parts presented a disorganized, dark, pulpy appearance. A portion of the ball, much disfigured, was extracted from under the pectoralis major muscle.

M. Pelletan has recorded a case of hemorrhage on the 70th day, occasioned by the presence of a splinter.

These cases might easily be multiplied, but a sufficient number has been advanced to show that the occurrence of hemorrhage may with equal reason be urged against allowing the fragments to remain, as against proceeding to their extraction. Any dread from this cause, cannot be considered sufficient to deter the educated surgeon of the present day from a beneficial operation.

It may be argued that the secondary splinters, retaining connexion with the soft parts, and consequently not completely cut off from the circulation, are not incapable of preserving their vitality, and on that account should not be removed. Experience does not confirm such a conclusion; the inflammation which they excite speedily isolates them; the pus resulting from a chronic abscess may long rest with impunity in contact with the periosteum, but that accompanying active inflammation rapidly destroys it, the nutrition of the fragment ceases, and it becomes converted into a true foreign body. When comminuted fracture of the bone has occurred without breach of the tegumentary surface, where a cutting instrument has effected a separation of a portion of bone without destroying its connexion with the soft parts, where a compound fracture has resulted from an accident which has exerted no contusing power directly on the bone, we know that reunion of the fragments may take place. In comminuted fractures from gunshot wounds, the condition of the parts is in no way the same; the projectile exerts on the bones an action analogous to that observed on the soft parts; the contusion caused by the projectile adds to the disturbance occasioned by the fragments, and a high degree of inflammation is inevitably excited, which rapidly destroys their connexion with the living organism; the state of the parts in these fractures differs, therefore, from that existing in those produced by other means.

Experience fully convinced Dupuytren of the futility of expecting that the secondary fragments would retain their vitality; he asserts that their detachment is operated spontaneously, through suppuration

at a shorter or longer period, varying from eight days to even months. During this period they keep up a constant irritation, and aggravate the local symptoms; the general disturbance is known to be always in proportion to that of the local inflammatory accidents; moderate when these are calmed by judicious measures, but, on the contrary, of a formidable character when these are neglected, or badly treated, or treated ineffectually.

It is to obviate these inconveniences that Baudens and Nelaton advise the removal of these fragments, as well as the primary ones, in order to place the patient at once in a condition, which is otherwise only attained at the expiration of an indefinite period,—the period necessary for their spontaneous removal. The patient is thus in the best possible position for ultimate cure; the chief cause of local inflammation is removed; the general fever diminishes proportionally; the strength is not worn down by prolonged suffering and suppuration, nor the nervous irritability of the patient morbidly increased. Twenty-five cases in the wards of M. Baudens, in which he followed these rules, and which are all terminating favourably, attest the efficacy of the practice.

Another argument in favour of this practice may be drawn from the occurrence of tertiary fragments, as whatever tends to heighten the local inflammation favours their formation. M. Baudens proceeds to the extraction of these as soon as their existence is ascertained, and the operation practicable.

The question now presents itself, whether the success of M. Jobert, and those who side with him, is such as to overrule the treatment here approved. Statistics are not yet attainable, and it is probably by these that the question will ultimately be judged. The practice of M. Jobert already offers two successful cases, so called; in these, consolidation of the fracture has certainly taken place, but an enormous mass of provisional callus is formed, inclosing several necrosed portions of bone; these represent true sequestra, will necessarily play the part of foreign bodies, and probably, at a future period, become the cause of great suffering. In the mean time, the progress of the healing process within the provisional callus is unknown, and a considerable time must elapse before it can be ascertained. In the man wounded in February a fistulous opening exists, as already stated, in communication with the seat of fracture. M. Jobert believes that portions of necrosed bone, may, like balls, remain in the bones without injurious consequences; according to Dupuytren their sojourn in the midst of callus is one of the most common causes of deformity, and of interminable fistula. In certain cases no provisional callus forms, and it is only after the separation of the necrosed portions that consolidation takes place.

M. Malgaigne adopts the expectant mode of treatment at first; but when suppuration is established, makes frequent use of the probe and forceps, extracting splinters six and seven weeks after the infliction of the wound. In a case of comminuted fracture of the fore-

arm, no examination of the fracture was made on admission; fifteen days afterwards, splinters began to come away by the suppurative process; as late as eight weeks after the infliction of the wound, he dilated one of the orifices to facilitate the extraction of a splinter.

Other surgeons extracted immediately the most easily reached primary fragments, and renewed the examination of the wound, day after day, to extract those remaining, and the secondary as they became loosened; at each examination causing much suffering to the patient.

The foregoing facts and reflections are held to indicate, that in every case of comminuted fracture of the extremities, where an attempt is made to save the limb, extraction of all the splinters of bone is imperatively demanded.—*(To be continued.)*

ARTICLE III.—*On a Peculiarity of the Humerus and Humeral Artery.* By JOHN STRUTHERS, Esq., F.R.C.S., Lecturer on Anatomy, Edinburgh.

A FEW instances have been recorded in which the humeral artery and median nerve continued downwards in a direction towards the internal condyle, above which, in order to gain their usual position in front of the elbow, they passed around a peculiar process, there developed from the humerus, as if for the purpose of affording them protection.

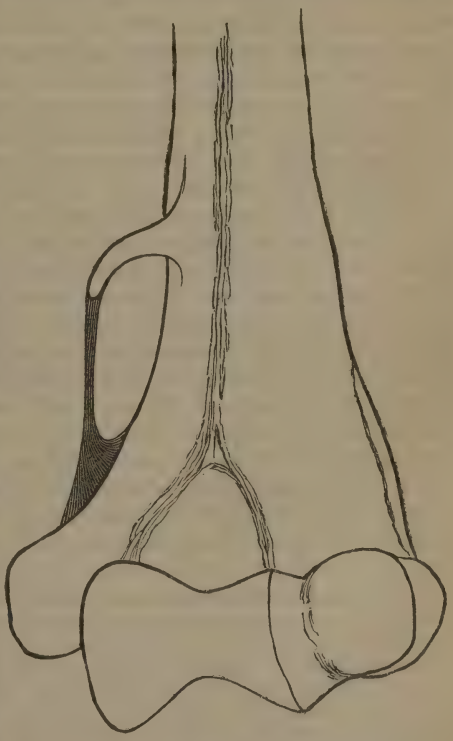
This singular arrangement has occurred comparatively so often of late in the subjects dissected in my rooms, as to induce me to call more particular attention to it, since it is one of some importance to the surgeon, and is interesting also from the analogy it presents to the usual arrangement of these parts in certain animals.[1]

The process, and the ligament by which the protective arch is completed, are represented by Tiedemann ("Tabulæ Arteriarum," Pl. xv. fig. 3. 1822); but, in this instance, the artery which passed through was not the main trunk, but a large interosseous, which had arisen high in the arm. The next notice which I find of this peculiarity is by Dr Knox (Ed. Med. and Surg. Journal, 1841, p. 125), who—after reasonably doubting the correctness of a statement, which appears to have been made by Magendie, that there existed a "supra-condyloid" process and foramen in the humerus of the yellow races of Africa—states that approaches to the same structure might be found in "many" European arms; and records an instance in which the arrangement had just occurred to him in "the left arm of

[1] In many animals—amongst the Quadrumana, Rodentia, Edentata, Marsupialia, and more especially the Carnivora—the median nerve and humeral artery pass through an opening in the humerus, which is formed by an arched process crossing obliquely the inner surface of the bone, close above the internal condyle.

a stout muscular middle-aged man," with the deviation of the artery and nerve in order that they might pass around the process. Another instance of this arrangement is recorded by Mr Quain in his recent work on the arteries, and is represented in Plate xxxvi. fig. 3, of his splendid illustrations of the usual and unusual distributions of these vessels.

I first saw this variety in the instance already quoted from Dr Knox, the preparation—showing the artery and nerve passing underneath the osseo-ligamentous arch—being now in my possession; and I have since met with it on seven occasions, in subjects dissected by me, or by the pupils under my superintendence. The first of these I met with four years ago, whilst engaged in practising the operations on the left arm of a female subject. The second and third occurred the following year in both arms of another female subject; and the remaining four during last winter session, being in both arms of two male subjects.



In all of them the protective arch was well formed, and transmitted the median nerve and undivided humeral artery, except on the right side of the last subject, on which the artery held its usual course, the nerve passing through the arch, along with a small muscular artery, which, in size and origin, resembled the inferior profunda. The osseous portion of the arch arose at a distance from the internal condyle, varying from an inch and a quarter to two inches and a half, and nearer to the anterior than the internal border of the humerus. It then curved downwards and inwards, and—in none of the above instances having exceeded three-fourths of an inch in length—gave attachment to the ligament, which, stretching in the same direction, joined the line close above the condyle, and thus completed the arch. In four of the above instances, the process, as now seen in the macerated bones, is from half to three-fourths of an inch in length. In the other three it is

shorter; more especially in that one in which the arch transmitted the nerve but not the artery, whilst on the other humerus, from the same subject, the process is the longest. The latter is represented in the accompanying outline of the lower third of the bone. The process and ligament are seen on their external and inferior aspect, so as to show the size of the opening as it now appears in the preparation.

I believe that this peculiarity of the humerus, when present on one side, will, from its nature, generally exist also on the other, unlike most varieties affecting at least the arteries; although in one of the subjects the difference in length between the processes was remarkable, as also was the difference in the position of the arteries. I may add, that I have on several occasions met with a similar want of correspondence in dissecting these parts in some of the animals in which the arrangement obtains; the bony arch being partly deficient on one side, whilst it was fully formed on the other. One of these was in a full-grown cat; in which animal also I have seen the opening on one side transmit—as in one of the instances above related of its occurrence in man—the nerve only; from which the natural inference may be legitimately drawn. I have also observed, in dissecting the young of these animals, that, at different stages of its development, the process resembles very much the conditions which it has presented in man. It is developed as part of the shaft of the bone, the ossification proceeding from above downwards; and very soon after birth it becomes united below to the lower end of the shaft, not to the epiphysis, of the bone.[1]

The above instances of the occurrence of this singular variety in man, having led me to think that it might have been present more frequently than had been noticed or recorded by anatomists, I lately examined the preparations of these parts in the museum of the Royal College of Surgeons of this city; and amongst those which had belonged to Dr Barclay, I found four instances of it, although he has not alluded to the variety in his work on the arteries. Two of these occur in separate arms, both of the left side. One that of a very young child, where the process is long; the other that of an adult, where the ligament also is preserved. The other two are seen in

[1] This account of the formation of the opening in animals, is opposed to the view of those comparative anatomists who believe "that the passage of the artery through a foramen of the humerus, results simply from the extensive development of the inferior extremity of that bone," by which the vessel becomes at length encircled, in order, merely, that it may not be thrown out of its original and direct course; and tends to confirm the more common view, that the arrangement is one specially for the purpose of affording protection to the parts which the opening transmits; although, on the other hand, a consideration of the habits of the animals in which the arrangement occurs, and the dissection of the parts, do not render it evident that they, more than some others, require such a special means of protection for the nerve and artery. See "A General Introduction to the Natural History of Mammiferous Animals," &c. P. 89. By W. C. Linnæus Martin.

the preparation of a child. The process is equally developed on both sides, but the ligament has been removed. On the right side, the undivided artery deviates and turns round the process. On the left, as in the other two instances, the radial artery arises above the middle of the arm, and continues downwards along the inner margin of the biceps, and the larger trunk (ulnar-interosseous) is removed to the inner side, and passes around the process.

To the fourteen instances above referred to or related, may be added, practically considered, four recorded by Mr Quain, in which the artery deviated in the same manner from its usual position. In two of these (op. cit. p. 259) the main trunk, in the others (as represented in Pl. xxxvi. fig. 4, and Pl. xxxvii. fig. 1) one of two trunks into which the humeral had divided, passed downwards along the inter-muscular septum, which they perforated or crossed at a distance of from two to three inches above the internal condyle.

The number of the above instances is sufficiently great to render the variety one of interest to the surgeon. It will be observed that in one-third of them there was a high division of the humeral artery, and that, whilst one of the trunks occupied the usual position of the vessel, the other was removed considerably to its inner side; and in the remaining two-thirds, that the undivided trunk was so situated, that it could scarcely have been reached by an incision made in the usual course of the artery. Further, at the bend of the elbow—as the pronator teres (or an additional muscular slip, which below joins the pronator) arises, in such a case, as high as from the ligamentous part of the arch—the artery is so overlapped or covered, that, before it could be exposed here, some muscular fibres would require to be displaced or divided; although, it may be remarked, the circumstances usually requiring an operation upon this part of the vessel could scarcely occur, owing to its deep situation.

It may be added, that the bony prominence was very readily felt before the dissection of the parts was begun, in the subjects in which it occurred in my rooms, even in the instance in which it was shortest; and the detection of this—in the preliminary examination, in a case where the operation was about to be performed—would, together with the situation of the pulsation of, and the effect of pressure on, the artery or arteries, at once indicate the existence of the variety in question to one whose attention had been called to its occasional occurrence.

Part Second.

REVIEWS.

Report of the Council of the National Institute of General Practitioners in Medicine, Surgery, and Midwifery, on the present state of the Medical Reform Question. 8vo. pp. 84. London, August 9, 1848.

We are contented, for the present, to be chroniclers rather than critics, of the movements of medical reformers. This does not arise from indifference to the good work, but from apprehension lest in venturing on an opinion at the present time on points yet debated, we might possibly do injustice to some of the disputants. The facts on which our judgment must be based are not yet fully before the public; and we shall recur to the examination of them at a future time with the advantage of knowing the whole case. To the cause of medical reform we have ever been most ardently devoted; and we think that those who are lukewarm, and whose indifference assumes the form of supineness, when all the great interests connected with our profession are manifesting a just sense of the importance of reform, will deserve little sympathy if they should suffer for their neglect.

The cause itself is of great and undeniable importance. As respects professional men, it is the question, whether we are, in a medical sense, to continue to be three countries, or to be, in fact as well as in name, one united kingdom of Great Britain and Ireland; and whether or not we are to be subjected to vexatious iterations of the process of examination, serving no useful purpose, merely because we choose to shift our quarters from one part of the Queen's dominions to another. As respects medical incorporations, it is the question whether they will best consult their interests and their respectability by continuing to have a little preserve for each of them, and by prosecuting as poachers those who intrude upon it, or by exercising in common certain great functions for the benefit of the entire community of Great Britain and Ireland. And as respects the public, it is the question whether or not the present motley system tends to the good of her Majesty's lieges, and whether or not it be of importance to the people to have at all times access to the best and ablest advice, unembarrassed by restrictions of the kind we have alluded to. This is the question. It is, as we have said, a most important question. It is a question, the parties to which, in its threefold aspect, must have a common interest, if they are capable of seeing it; and it is not a little gratifying to observe the many indications now arising that they do see it, and are gradually preparing, in good faith, for a concurrent solution of its many difficulties.

An investigation of the medical institutions of the country before a committee of the House of Commons, commenced in the last Parliament, was resumed in the present. The evidence is now completed; a great deal of it has been made public by successive parliamentary reports; and the whole of it will soon be in our hands. During its progress professional men were made fully aware that they were expected by members of the legislature to facilitate their work, by endeavouring to co-operate on those great and broad principles of public utility which have so often received the sanction of large sections of the profession in every part of the country, and more especially by suggesting a plan for the legislative enforcement of a high and uniform standard of medical education and examination, as the basis of the desired equality of privilege throughout the whole extent of the British dominions.

In July 1847 a parliamentary volume came out, embracing the evidence taken, in the late Parliament, before the Select Committee of the House of Commons, "on medical registration and medical law amendment." The evidence proceeded entirely from witnesses on the part of the Royal College of Physicians of London, and of the Royal College of Surgeons of England, as there was no time for more before the dissolution of Parliament. On the assembling of the new Parliament in February 1848, a similar committee was almost immediately nominated, consisting, for the most part, of the same individuals, and having for its chairman the Lord Advocate of Scotland, instead of Mr Macaulay, who had filled that important office in the committee of the late Parliament. The new committee immediately proceeded to take evidence, which was ordered to be published on the 28th and 31st March 1848. It consisted of additional examinations of Fellows of the Royal College of Surgeons of England, members of the Council of that College; and of witnesses from the Apothecaries' Company of London, and from the National Institute of General Practitioners in Medicine, Surgery, and Midwifery.

While the evidence was thus proceeding in the Parliamentary committee, a most important movement took place in another quarter at the suggestion of the government. A conference was held in February, at the Hall of the Royal College of Physicians of London, between representatives of the two English Colleges, of the Society of Apothecaries, and of the general practitioners belonging to the National Institute which we have already mentioned, and whose very able and lucid "Report" is now before us. The result of the conference was the publication, in February last, of certain "Principles on which a Bill for regulating the Practice of Physic and Surgery should be founded," recommended by the joint authority of the parties we have enumerated. Of this document we gave some account in our April number (p. 765). It may be well to inform our readers that the National Institute professes to represent a very large body of the general practitioners of England, who

practise neither Physic, nor Surgery, nor Midwifery *exclusively*, but are engaged in mixed practice, consisting of the two former, or of all the three, and who are consequently inadmissible to the *fellowship* of the Royal Colleges, though many of them are *members* of the Royal College of Surgeons of England. The strictness with which the Colleges adhere to the principle of *pure* Physic and *pure* Surgery on the part of their Fellows, led those men to seek a separate incorporation as a Royal College of General Practitioners, which they had nearly attained in 1845, when Sir James Graham was at the Home Office. The failure of Sir James' proposed bill disappointed their expectations for a time. Some attempts have since been made to procure the incorporation of the general practitioners with the College of Surgeons of England; but they have been uniformly resisted by that college, and the conference is decidedly opposed to such attempts, its "principles" being based on the presumed necessity for the separate incorporation of the general practitioners as a third Royal College, and on the cessation of the functions of the Apothecaries' Company as a separate licensing body, in which, on that condition, the latter is willing to acquiesce.

The "principles" of the conference had thus received a large amount of support from the most influential bodies in the South, but had hitherto not been submitted to the medical men and medical institutions of Scotland and Ireland. As the session, however, advanced, the progress of the investigations conducted by the select committee of the House of Commons, made it necessary that representatives of the various medical interests of these divisions of the kingdom should be summoned to London to give evidence; and these gentlemen, as they came to town, were successively added to the conference at the Hall of the College of Physicians. Representatives were also admitted from the Universities of Oxford, Cambridge, and London. All this was done at the suggestion of the special committee of the House of Commons, to facilitate their labours, by suggesting such modifications of the "principles" of the conference as might be necessary, in order to accommodate these to the framework of their several institutions, and to the great and essential object of uniformity in education, examination, and privilege. These conferences appear to have accomplished in an important degree the object for which they were instituted. The representatives of large bodies of medical men in various parts of the country, pursuing the same general ends by somewhat different machinery, have been enabled by discussion to interchange important explanations, and to introduce such modifications of the "principles" previously agreed upon, and such additions to them, as were rendered necessary by the peculiarities of the institutions which they severally represented.

At the conclusion of the sittings of the conference (which lasted during the greater part of the parliamentary session), they were requested by the select committee of the House of Commons to present to them the result of their labours in the shape of the "outline

of a bill." This accordingly was done before the meetings of the conference were suspended; and the document contains, in a different form, nearly the same matter which we published in April.

The last portion of the evidence taken by the select committee was only very recently ordered to be printed. It will contain the examinations of the representatives of the medical institutions of Scotland and Ireland, and of the Universities of Oxford, Cambridge, and London. It will also contain the examinations of graduates of the London University, of *members* of the Royal College of Surgeons of England, and of extra-urban licentiates of the Royal College of Physicians of London.

In conclusion, we hope we may be permitted to congratulate our brethren of the medical profession on an increasing appearance of earnestness and good faith on the part of medical reformers, and on a more universal recognition by them of those great general principles of equality and equity upon which a reform of medical legislation must necessarily be founded, if it is to deserve that respectable title. We trust that in such circumstances the profession will be true to itself, and will be prepared to relinquish all narrow aims and exclusive interests which are opposed to the great general object. We borrow from the preface to the Report of the Council of the National Institute, the following excellent remarks on the temper and spirit in which the reform question ought to be approached by the members of the medical profession:—

"If the profession deem it just to the public, and expedient to itself, to listen to advice the direct tendency of which is to continue professional anarchy; if it deem it prudent to make demands which under present circumstances cannot be conscientiously conceded, either by the existing institutions, or by the Government itself; if it believe its influence sufficient to induce the Legislature to carry measures in opposition to the convictions of the parties now in authority; if it be conscientiously assured that an opposition to all reform is justifiable, because each section into which it is split, or each individual, cannot obtain the full complement which his own prejudices or his own aspirations have led him to desire; then let the present proposals be rejected. If, on the other hand, the profession can take a sober view of its own case, and, as a body of practical men, is willing to accept a real practical positive even though partial good, the advantage of which will be immediate, rather than aim at a shadow, and content itself with an Utopia which assuredly will never be realized,—then, in the opinion of the Council, it will cordially support the principles which have been assented to—ever remembering—that to secure the success of those principles, even under the present favourable circumstances, *unanimity* is most essential; that some of the more powerful parties to the arrangement may shrink from those active operations which are still necessary to carry it through;—ever remembering—that if, unhappily, the honest convictions of the active promoters of this measure are too lightly prized, and their efforts, founded upon those convictions, neutralized by any vexatious and irrational opposition, upon the heads of the opposing parties will rest the responsibility; and that, whatever evils themselves may have to bear with, from the indefinite postponement of all reform, or the enactment, amidst the chaos of opinion, of a measure inimical to their own interests or wishes, to none other than themselves can they impute the blame."

We trust, too, that the legislature will perform *its* duty with

firmness, and will feel it to be its high function to overrule those unreasonable objections and propositions which might otherwise again defeat the great object. Our legislators cannot now shelter themselves under the plea, that the subject has been insufficiently discussed, or that there does not exist a sufficient amount of concurrent opinion in the profession itself to enable them to do their part with effect; and they surely cannot think it wonderful that in a body of men so numerous, so separated from each other in place, and subjected to such various kinds of influences, there should still exist some differences of opinion on subordinate matters. Legislating for the medical profession would resemble no other sort of legislation if our legislators had merely to perform the easy task of putting the stamp of authority on that which has been prepared for them by others. They have now fully investigated the case; they cannot deny that the grievances are serious and their removal important; and we have a right to expect that they will perform their part as umpires with carefulness, firmness, and impartiality.—J. G.

Oratio ex Harveii Instituto in Ædibus Collegii Regalis Medicorum habita die Junii 24, 1848. A Francisco Hawkins, M.D., Coll. Reg. Med. Lond. Socio et Regestario. Olim Coll. Di. Jo. Bapt. Oxon. Socio. 4to. p. 27. Londini: 1848.

Our readers are probably aware of the nature and occasion of the annual Harveian Oration in the Royal College of Physicians of London. The great discoverer of the circulation left to the College his patrimonial farm of about fifty acres, on condition of an annual social meeting being held, and a Latin oration delivered, exhorting the members "to study and search out the secrets of nature by way of experiment, and, for the honour of the profession, to continue mutually in love." We have often allowed the profane doubt to enter into our minds, as to whether the establishment of this oration has, on the whole, produced results commensurate with the benevolent aims of its distinguished founder. We are very sure, that could his modest and benign spirit have anticipated the small common-places of eulogium which have been lavished upon him from time to time; still more, could he have been aware of the manner in which it has of late been the practice to invoke his honoured memory, and the facts of his life, in favour of every crude theory which meets with a reception too cool for the glowing brain of its originator,—he would have been disposed to renounce posthumous fame for ever, seeing to what base uses it may be applied.

We prefix these remarks in order to have the pleasure of making an exception in favour of the oration before us. We have seldom read a character of Harvey at once so true, so pleasing, and so elegantly expressed, as that by Dr Hawkins in the following paragraph. It forms in itself a most admirable answer to those who, in the pre-

sent day, profane the memory of the great physiologist by making his name the signal for strife, and who attempt to bear down the scepticism of the world by fierce invective or querulous satire, instead of by the legitimate method of rational argument and experiment.

"Attendite, quæso, et cum animis vestris reputate, quisnam et qualis fuerit, qui nos *ad amorem mutuum* cohortandos curaverit? Is erat, qui, cum summum beneficium in omnes contulisset, tamen, a nonnullis, non sine contumelia atque convitio, oppugnaretur. Is, qui sanguinis motum circuitumque primus invenerat, ipse *Circulator* audiit. Et eum, nimirum, quem primo derisui habuerant, postea, insimulaverunt furti. At ille, qui 'nullius lacessivit famam,' ne suam quidam acriter defendit. Obtrectatorum partim risit et neglexit, partim redarguit, at quam benigne! neque istos refellendi causa, sed quo pulcherrima sua reperta enucleatius explicaret. 'Convitia vero aut præterivit, aut laudibus rependit.' Testor Riolanum, 'hominem,' ut ait Laurentius, 'in disputando arrogantem et pertinacem,' quem, cum, 'erroris atque inscitiæ convicisset, quasi unguentis delibutum atque floribus redimitum dimisit.'"

Dr Hawkins' address opens, as in duty bound, with a character of Harvey. He afterwards passes to an enumeration of the more distinguished members of the College of Physicians, to each of whom his meed of praise is awarded. The consideration of so many men distinguished alike in the practice of the healing art, and in general scientific and literary studies, suggests to him the errors of those who assert the uselessness of these studies to the medical man;—"qui *litterarum* et *scientiæ* studia medicis indici, saltem ex auctoritate, noluit; qui studia hæc medicis obfutura, atque ad medendum impedimento fore contendant." He thinks that scrupulous and strict examinations and enforced attention to these subjects are necessary to the medical man, to preserve him from the rocks and shoals of popular applause and blame; "ne famam quærat et opes, non artis prudentia, sed verbosis strophis, atque stupore vulgi." He quotes from Lord Bacon to show, that medical capacity cannot be judged of by the public, like that of the advocate or the pilot of a ship, who are judged, he says, not by the issue of their acts, but by their inherent merit (?) Nay, in medical matters, the public are the more likely to be deceived, "quia magna pars hominum amat mirifica, et vult decipi: neque facile sibi errorem eum, quo delectatur, extorqueri patitur." Hence the numerous tribes of quacks, some of whom are very roughly, and we think ungenerously, handled by Dr Hawkins. It was scarcely worth while, and was stooping at very poor game, for a Fellow of the Royal College of Physicians to go out of his way to startle the ghost of St John Long, or to prefer such grave and revolting charges against the Mesmerists and Homœpathists as we find here. We need not say that we have no extraordinary sympathy with these gentlemen; but we think they might be passed over, or reprobated in a more dignified and charitable style than that of the orator, without either the truth of history or the honour of the profession being less effectually vindicated thereby.

With respect to the remarks of Dr Hawkins on medical education and qualifications, we are far from participating the opinions of those against whom his arguments are directed; but neither can we agree with him in deprecating the influence of public opinion on the character of medical men. No doubt public, and still more individual, opinion may often err in estimating medical qualifications, still we think that there is generally a large share of justice in its awards; and as public opinion is, and always must be, the ultimate court of appeal by which professional reputations are judged, we have no doubt it will become daily more efficient, and will in the end be the only efficient bar to the practice of that charlatanism which Dr Hawkins so heartily abhors, and which, let us hint to him, may be found to almost as great an extent within the legitimate profession as out of it. Examination and enforced education are undoubtedly necessary for the security and honour *of the profession*, to prevent the admission of ignorant and incompetent persons into its ranks; but great care must be taken that artificial and arbitrary tests be not substituted in place of those natural and practical ones which the profession will readily acknowledge as just and proper, otherwise an oppressive pedantry, combined with real ignorance, will infallibly become the characteristic of the body of medical men, and will be met by contempt and disaffection on the part of the public. For the security *of the public*, the public itself must provide; and it is only when this feeling, hitherto so sedulously obstructed by medical corporations, becomes general, that men will proceed to choose their medical attendant with the same care, and deliberation, and reliance upon informed persons, as they employ in the case of their lawyer or banker. Then and then only, we believe, will quackery in its present aggravated forms cease to exist in this country.

After the extracts we have given, we need hardly say that this Harveian oration is excellent in its kind; it is the production of an active and refined mind, and is well adapted for its purpose.

The Latinity of this oration is unexceptionable. It is classical; it is smooth and flowing; it is free from pedantry. In freedom the style improves as the discourse proceeds. The great fault in the modern Latinity of men who are scholars, especially in set discourses like that before us, is a perpetual slavish reference to expressions in particular passages of classical authors. Dr Hawkins does not err deeply in this respect. But we are sure his style would not have suffered had he practised this habit even less. One of the great excellencies of the Latin language is the forcible simplicity of which it is susceptible. This beauty is seldom to be traced in modern Latinity. And the cause plainly is, that those who write Latin formally are commonly more desirous to show their familiarity with the writers in Latin than with the language. What a patchwork would an English discourse seem were it made up of expressions borrowed, in every second or third sentence, from the classics in the language? And yet this is the common plan on which much of the

modern Latin of scholars is written. Dr Hawkins has shown himself alive to the prevalence of this fault, though he has not been able wholly to avoid it. The practice of keeping up an annual Latin oration in the London College of Physicians, is undoubtedly a good one; and as it is fixed by Harvey's will, we are glad it is not in danger of being soon abolished by the undiscriminating fervour of modern innovation. We trust it may long continue to keep alive among the fellows of the College the laudable ambition of writing a pure Latinity like that of Dr Hawkins.

Part Third.

MEDICAL NEWS.

DEATH OF BERZELIUS.

The public prints have already made our readers acquainted with the death of Berzelius, whose name has been for forty years a household word in the chemical laboratory, and is interwoven with the history of chemistry by a series of investigations probably unequalled, and certainly unsurpassed, either in extent or importance, by those of any of the great chemists of the present century. Prolific as that period has been in distinguished chemists, Berzelius has, perhaps, more distinctly than any other left the impress of his ideas upon the science; and, though the greatest step which chemistry has made belongs to Dalton, it is not too much to attribute to the Swedish philosopher a large share in the work of developing and establishing the atomic theory. For the few following details of his life we have been principally indebted to an article in the *Athenæum*, and partly to some other sources of information, which have enabled us to fill up some points slightly touched on in that article.

Jöns Jacob Berzelius was born in the village of Väfversunda, in the canonry of Linkogring, in Ostgothland, on the 29th August 1779. His father was the village schoolmaster, and under his instruction he appears to have acquired his early education. At the age of seventeen he entered upon his medical studies at the University of Upsala, and here he also commenced his chemical studies. Afzelius was professor of chemistry at that time, but neither he nor his assistant, Ekeberg, though in other respects good chemists, appear to have excelled as teachers. The practical instructions especially appear to have been of a very unimportant character. The pupils were allowed to work one day in the week in the laboratory, where they were employed in trifling pharmaceutical operations. With these Berzelius was at first disgusted; but he soon came to frequent the laboratory daily, although he had no right to do so, and occupied himself in experimenting and observing in his own way. Although he was thus entirely thrown upon his own resources, there can be little doubt that he must have indirectly acquired much information from observing the researches which were going on in the laboratory; for Afzelius, though his health was bad, and he rarely published any thing, was constantly at work, and was in reality a very skilful chemist. It may not, perhaps, be generally known that he had discovered selenium, independently of Berzelius. After his death, his apparatus and specimens came into the hands of Berzelius, and among them there was found a small packet of impure selenium, which was dated, and marked "metal from Skrikerum," and which he had doubtless extracted from eukairite, which occurs there, and in which Berzelius proved its existence. In his early years, Berzelius had also the advantage of an intimate acquaintance with Gahn, who first applied the blowpipe to chemical experiments, and to whose instructions he owed the skill which he acquired in the use of that instrument.

In 1798 Berzelius passed his philosophical examinations, and in the following year became assistant to Dr Hedin, superintendent physician of the mineral waters of Medevi. His residence here gave occasion to his first contribution to chemistry, which consisted in an analysis of the waters of Medevi, which he published along with Ekeberg. In 1801 he received the license to practise, and in 1804 took the degree of M.D.

On leaving Upsala he came to Stockholm, where he was appointed assistant to Andrew Sparrmann, who accompanied Cook in one of his voyages round the world, and held the professorship of medicine, botany, and chemistry in the Carolinska Institut, the medical school of Stockholm. He at the same time practised medicine, but only to a limited extent; and as he devoted himself more completely to chemistry, he at length abandoned it entirely. On the death of Sparrmann, in 1806, he succeeded him in his professorship, in the duties of which he confined himself almost entirely to chemistry. In the same year he published, in conjunction with Hisinger, the first volume of his "Afhandlingar i Fysik, Kemi, och Mineralogi," a periodical work, which was continued at irregular intervals till the year 1818. The greater part of these volumes, six in number, was the work of Berzelius, Hisinger's share being comparatively small, and a few papers by other authors, forming too inconsiderable a proportion to require mention.

In the years 1808, a new era in chemistry was commenced by the publication of Dalton's new system of chemical philosophy. Berzelius was among the first to adopt the views of the English chemist, and from that time he devoted himself to its development, and commenced the series of researches on the atomic weights of the elements, which he may be said to have continued almost to the end of his life. In 1810 he was made president of the Swedish Academy of Sciences, and in his address on resigning the chair, he took a complete view of the state of animal chemistry, which was afterwards published, and has been translated into English. Berzelius had now reached a high degree of eminence, and his researches were considered of such importance, that the Academy of Sciences, in 1811, voted him an annual sum of 200 dollars, about L.16, to assist in defraying their expense.

In the year 1814, he published his "Försök, at, genom användet af den electrokemiska theorien och de kemiska proportionerna, grundlägga et rent vettenskapligt system för mineralogien." In this important work, he pointed out that the atomic theory might be extended to all minerals; and showed that the greater number of them are true salts, in which silica is the electro-negative or acid ingredient. On these principles he deduced from his own analyses, and those of his predecessors, the true constitution of all the more common minerals; described a system of symbols by which they might be represented; and gave a sketch of a purely chemical arrangement of mineral species.

In the year 1818 he was appointed secretary to the Academy of Sciences, the highest scientific office which Sweden has to bestow, the duties of which he continued to perform till his death.

The number of cultivators of chemistry had now become so large, and facts accumulated with such rapidity, that the Academy of Sciences entrusted to Berzelius the duty of drawing up an annual report on the progress of physics, chemistry, and mineralogy. The first volume of this report was published in 1822, and it continued to embrace the whole of these branches of science up to the year 1839, when the physical portion was disjoined, and formed into a separate report, while Berzelius confined himself entirely to chemistry and mineralogy. The last volume which he published was that for 1847.

In 1831, Berzelius retired from the active duties of his professorship, and obtained the title of honorary professor. Up to this time he had remained a bachelor, but in 1838 he married the daughter of his old school-fellow, Staats Rath Poppius. At the same time he was made a baron, and the intelligence was communicated to him on his wedding-day, in a letter from the King himself. In 1843, having been for twenty-five years secretary of the Academy of

Sciences, a festival was given in his honour, the crown prince in the chair, and his portrait was presented to the academy. At this time, his health, which had hitherto been robust, began to give way; symptoms of an apoplectic character made their appearance, for which rest and change of scene were recommended. Berzelius accordingly spent the winter of the following year in Germany, and returned to Sweden in improved health. His constitution was, however, enfeebled, and early in last year he was seized with paraplegia. It was now obvious that his end was approaching, and he abandoned all interference with science. His mind, though perfectly entire, was incapable of prolonged exertion, and his last days were spent in hearing read to him works in polite literature, and in reviewing the occurrences of a well spent life. The natural vigour of his constitution enabled him to withstand, for a longer period than might have been expected, the ravages of his disease, and he died on the 7th of August last, in his 69th year.

Few men have embarked in the pursuit of science under more advantageous circumstances than Berzelius. The native of a country which has always been proud to honour, encourage, and reward her scientific sons, he commenced his career at the time when chemistry was rising up into a new science, and the recently established facts were beginning to be moulded into a philosophical form. He was fortunate also in bringing to its study a knowledge of the exact sciences; for, besides being well versed in physics, he possessed a greater knowledge of mathematics than usually falls to the lot of the general student, and the inductive spirit which these studies engendered, is apparent even in the very least of his works. To all these must be added an almost unequalled perseverance, which enabled him to pursue to a successful end investigations surrounded by difficulties which would have deterred most men.

Berzelius was a voluminous writer. Independently of those which we have already mentioned, he published, about 1808, a work on animal chemistry, in two volumes. His "Lehrbuch der Chimie," has gone through no less than eight editions. The earlier of these were in three or four volumes, and were published in Swedish and afterwards translated into German; but the later editions were translated from the manuscript of the author, and published in German, the Swedish circulation being too small to make so large a work remunerating. The seventh, which was completed in 1841, was in ten volumes; and the eighth, which was left unfinished by the author, and is to be concluded by Professor Wöhler of Göttingen, is expected to be still larger. Berzelius also published a work on the blowpipe, which has gone through five editions, and is admitted on all hands to be the most complete manual on the subject which has ever been published. Considering the extent of these works, one is surprised that Berzelius should have been able at the same time to pursue so many extended investigations, but he possessed a remarkable facility of composition. In illustration of which it may be mentioned, that his Annual Report, which is a closely printed volume of 500 or 600 pages, was the work of exactly a month. It was his habit to begin it every year on the first day of March, and it was presented to the Academy on the 31st, and during that month he never appeared to be particularly busy, but was as ready as at any other time to converse with his chance visitors.

Any detailed account of the numerous discoveries with which Berzelius has enriched chemistry, would extend this notice far beyond its assigned limits. He discovered the elements Selenium, Thorium, and Cerium. His researches on atomic weights have already been alluded to; and his investigations into the constitution of the compounds of the elements are too numerous to mention; and, in many instances, all our exact knowledge of these substances is derived from his experiments. But it was not merely to mineral chemistry that his researches were confined: his investigations of the different constituents of the animal body, are, in some respects, among the most remarkable of his works; for, though performed at a time when the whole subject of animal chemistry was in an almost embryonic state, there is scarcely a result which has not been

confirmed by more recent researches. Of this the discovery of lactic acid in the juice of flesh, is a remarkable instance. At the time when Berzelius made these experiments, the processes for preparing pure lactic acid were totally unknown, and its nature undetermined, yet, by a careful comparison of its characters with those of the acid existing in flesh, Berzelius was enabled to declare their identity ; and although Liebig for some time denied the possibility of lactic acid occurring in the animal fluids, his latest researches have confirmed the old results of Berzelius. He also established the fundamental facts of vegetable chemistry by a series of analysis of the principal vegetable acids, &c., and afterwards pointed out the analogy in constitution between certain organic substances and the oxides of the metals, and on this he founded the doctrine of compound radicals, in the elucidation of which Liebig has acquired his well-merited reputation, and which, though now likely to undergo very great modifications, has been mainly conducive to the recent rapid progress of organic chemistry.

As an experimentalist, Berzelius stands unrivalled for his accuracy and precision. In all his researches he aimed at the highest degree of refinement which was possible, and on all occasions he avoided any thing like show. His laboratory consisted of two small rooms in the *entresol* of the Academy of Sciences, to which was attached a third, containing his library. Here every thing was arranged with the neatness which characterised all his operations. His apparatus was the model upon which that of all the other laboratories in Europe was formed, as indeed almost all the methods employed in the refined modern analyses were contrived by himself. He especially introduced the system of operating on the small scale, and to such an extent did he carry this, that he was enabled to complete the investigation of Thorium, including the determination of its atomic weight, and analyses of its salts, with not more than 60 or 80 grains of thorina. It is odd enough to contrast this with the discovery of Tellurium, which Muller von Reichenstein, being in some doubt as to its being actually a new substance, sent to Bergman, then the great chemical authority, requesting him to examine it. This he did, and soon returned for answer that he thought it was, but that the quantity sent was too small to admit of a positive statement. The *residue* of Bergman's experiments afterwards came into the hands of Berzelius, and is a piece about the size of a small pigeon's egg !

In the personal appearance of Berzelius there was little remarkable. He was short and stout, with the appearance of radiant health. The man of genius was not seen in his exterior, but there was an expression of *bon hommie* in his countenance which made a favourable impression upon a stranger, an impression which was invariably confirmed by a further acquaintance. His hospitality and kindness were great to all, but more especially to the cultivators of his own science, and no young chemist ever made his acquaintance without being delighted with his readiness to discuss the doctrines of the science, and to communicate the stores of his chemical experience.

His loss to chemistry is great, as he was the authority in whom all chemists had confidence. His position rendered him in a manner the great commentator on the progress of his science ; and though his criticisms were in general temperate, no man could be more severe and sarcastic when he thought that any one had stept beyond the bounds of careful induction.

DEATH OF DR WILLIAM CAMPBELL.

Many of our readers will regret to hear of the death of Dr William Campbell, well known as an accoucheur and lecturer on midwifery in Edinburgh. He died on the morning of September 21, after a short illness, which latterly assumed an apoplectic form, most probably from ramollissement, or other disorganization of the brain. Ten days before, he had seemed to be in his usual health and vigour. Dr Campbell was a native of Argyleshire, and before settling in Edinburgh (now nearly thirty years ago), he had served for some years

in the medical department of the navy. During this period he visited the West Indies, and various other naval stations. Latterly he was a prisoner of war at Verdun; and he held a testimonial from Baron Larrey, granted for services rendered to the sick and wounded sent to the hospitals there from the wreck of the French army, after the retreat of Moscow. On the restoration of peace he took his degree in medicine at Edinburgh. In the winter of 1819-20, we believe, he gave his first course of midwifery, and from that time he continued to lecture on the same subject, with increasing reputation, down to the end of the summer session which has just closed. The museum which he collected for the illustration of his lectures stands almost unrivalled.

Dr Campbell was an author to no inconsiderable extent, and his productions show much ability, and an extensive acquaintance with the subject which he cultivated. In 1822 he published his "Treatise on Puerperal Fever;" in 1833 he published his "System of Midwifery," a second edition of which, much enlarged, he brought out in conjunction with his son in 1843; in 1842 he published his memoir on Extra-Uterine Gestation, a translation of which has since appeared in Germany. Besides these separate works he published a number of valuable papers in the Edinburgh Medical and Surgical Journal, the Lancet, and the Northern Journal of Medicine—of these the chief are, "On Hemorrhage after Instrumental Labour," E. M. S. J., vol. xvi; "On Transposition of Abdominal Viscera," ib. vol. xvii; "Observations on Laceration of Perinæum," ib. vol. xix; "On Laceration of Uterus and Vagina," ib. vol. xxix; "On the practice of accelerating Parturition in Deformed Pelves," ib. vol. xxxiii; "Case of Cæsarean Section," ib. vol. xxxviii; "On a new instrument, the Kephalepsalis," Lancet, vol. i., 1841–42; "Illustrations of Congenite Syphilis," Northern Journal of Medicine, May 1844; "Unusual Termination of a case of Congenite Hydrocephalus," ib. June 1844; "Cæsarean Section after the death of the Parent," ib. October 1844; "Statistics of 5754 Deliveries," ib. June 1845; "Report of the Obstetric Practice of the Royal Dispensary for 1845," ib. May 1846.

Dr Campbell was a man of indefatigable industry, and daily went through much exertion with little or no relaxation, and yet his health never appeared to suffer. He had hardly reached his sixtieth year.

PROGRESS OF CHOLERA.

From the 3d to the 4th of September, thirty-nine new cases of cholera occurred at Berlin; and from the 4th to the 5th there were sixty-four. The disease has also broken out at Spandau. The latest accounts from Constantinople, which are to the 25th ult., state that the cholera there continues much the same—from sixty to seventy cases per diem. At Adrianople its ravages have extended to 300 a day! At Trebizond it had diminished to ten or twelve cases daily. At Salonica it still rages to the extent of 1500 per week. Smyrna, too, is horribly afflicted: hundreds die daily; the dead literally bury the dead. Bazaars all shut, shops deserted, business suspended; more than 40,000 have left the city, which is now a prey to robbers. This is the description given of Smyrna by the last French steamer. The cholera has also appeared at Samsoun.

Germany.—The German papers contain returns of cases of cholera from the following places:—Berlin, the 8th instant; Stettin, the 5th; Magdeburg, the 6th; Vienna, the 8th; Moscow, the 26th of August; St Petersburg, the 1st of September; and Warsaw, the 5th. Up to the 8th instant there had been 784 cases of cholera in Berlin, 484 of which had terminated fatally; 205 patients are still under medical treatment, and the remaining 95 are reported as having favourably concluded. At Stettin, on the 6th, 32 new cases had occurred, and seven had died. The cases from the 8th of August to the 5th of September were 652, of which 433 had terminated fatally, 110 had recovered, and 109 remain ill. At Magdeburg, up to the 6th inst., there had been 79 cases, of which 35 had terminated fatally; from the 6th to the 7th, four new cases had

occurred, and five had died, making 83 cases, out of which 40 deaths; from the 7th to the 8th, 104 cases and 46 deaths; and from the 8th to the 9th, 17 new cases, and 10 deaths. At Vienna, the cholera had already broken out, and two deaths had occurred. At Moscow, on the 25th of August, there were 25 new cases and 16 deaths; on the 26th, 25 cases and 11 deaths. At St Petersburg, on the 1st inst., there were 53 new cases and 20 deaths. At Warsaw, from the 1st to the 4th of September, 214 cases, of which 53 cured, and 58 deaths. At Posen, also, the cholera had broken out.—*Examiner*.—More recent accounts state its arrival at Hamburg.—*Lancet*.

CHEAP REMEDIES.

An article with this title has appeared in a French medical journal, written by M. Recamier, ex-physician of the Hôtel Dieu! The remedies proposed, and to which almost infallible powers are assigned, are cabbage poultices for gout and rheumatism, and cobweb pills as a substitute for quinine in intermittent fever. The rare sagacity and close observing powers of the author are eminently displayed in the discovery, that the cobwebs found in the houses of the poor are less efficacious than those which festoon the dwellings of the rich!

BOOKS RECEIVED.

1. Handbuch der Heilmittellehre. Von Dr Fr. Oesterlen, Professor der Medicin an der Universität Tübingen. Tübingen. 1845.
2. Lehrbuch der Arzneimittellehre. Von Dr. G. G. Mitscherlich. Erster band. Zweite verbesserte Auflage. Berlin. 1847.
3. Traité de Thérapeutique et de Matière Médicale, par A. Trousseau et H. Pidoux. 3me Edition, 3 vol. Paris. 1847.
4. Traité Philosophique et Expérimental de Matière Médicale et de Thérapeutique. Par. G. A. Giacomini, Professeur de Médecine Clinique à l'Université de Padoue, &c. French translation by Moyon and Rognetta. Paris. 1839.
5. Etiological, Pathological, and Therapeutical Reflections on Asiatic Cholera. By A. Henriques, M.D., F.R.C.S.L. 8vo. London. 1848.
6. The Periodoscope: with its applications to Obstetric Calculations, &c. By W. Tyler Smith, M.B. 8vo. London. 1848.
7. Observations on Hospital Gangrene. By John Boggie, M.D. 8vo. London. 1848.
8. The Treasury of Natural History. By Samuel Maunder. 12mo. London. 1848.
9. Bulletin de L'Académie Royale de Médecine de Belgique. Tome VII. No. 7. Bruxelles. 1848.
10. Sanitary Questions, Observations, and Suggestions on the Wolverton Well Water. By George Corfe, M.D. 8vo.
11. Defects in the practice of Life Assurance, and Suggestions for their Remedy. 8vo. London. 1848.
12. A Treatise on the Advantages and Necessity of frequent Bathing. By Edward Perry. 8vo. Wolverhampton. 1848.

TO READERS AND CORRESPONDENTS.

Communications have been received from Dr Waters, Dr Douglas Maclagan, Dr Thomson, and Dr Barker.

Owing to the press of original matter in the present Number, we have been unable to insert the Reviews now ready on the works of Burnet, Morrison, Golding Bird, Matteuci, &c.

In the department of Medical News, we have been obliged to postpone articles on "Surgery in China," "Public Health Act," "The Hemlock Case of Poisoning," &c. They will appear in our next Number.

NOTICE TO SUBSCRIBERS.

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Part First.

ORIGINAL COMMUNICATIONS.

Article I. — *Contributions to Toxicology. Cases of Poisoning, with Remarks.* By Douglas Maclagan, M.D., F.R.S.E.

The following cases are selected from among those in which, during the last few years, I have been consulted by the legal authorities. It is presumed that no apology is necessary for their publication. Even when they involve no very new or striking peculiarities, the details of medico-legal cases are important, as contributions to our store of experience.

Arsenic; Imputed Poisoning; Distinction of Arsenic from Antimony.

The last capital conviction in Scotland for poisoning with arsenic, was in the case of Thomas Leith, who was executed at Dundee last autumn, for the murder of his wife in April 1847.

The history of this case has been made known to the readers of the *Monthly Journal*, by a report of the trial in the Number for October 1847. As regards the poisoning of the murderer's victim, although the facts are merely those of an ordinary case of death from arsenic, it may not be superfluous to state a few of the details which are not given in the report in the *Journal*.

Leith, besides being conjugally unfaithful, had for some time pursued a system of cruelty towards his wife, one of the manifestations of which was, his depriving her of money, and thus curtailing her means of feeding herself and her children, from whom he was at the time of her death living separate.

About two months previous to her death, he endeavoured to fasten upon her the accusation of an attempt to poison him, by

mixing arsenic with his tea. His motive in imputing this crime was supposed by Mrs Leith herself to be to get her out of the way, by bringing her under the pains of law for the attempt, and thus to facilitate the adulterous intercourse which he was at the time carrying on with a servant girl; but it more probably was only a part of his deep laid scheme to poison her, for he more than once insinuated after this, that something would occur which would show that his wife was the real trafficker in poison, and not himself. He said to one witness, who had accused him of putting the poison himself in his tea, "Suspend your opinion for a short time, and something will soon take place regarding that woman, which will convince you beyond doubt that I have had no hand in the matter you charge me with, and that it will appear to you all that she has been the cause of all that has taken place."

On the fatal morning Mrs Leith was sorely pressed for money, and applied to her stepmother to procure for her a supply from her husband. Having received a small sum, she purchased some oatmeal and made porridge for herself and her children; but the quantity being insufficient, she had recourse to some barley-meal which had been for some time in the house, and she made it into porridge also. This barley-meal had been used for baking *scones* on a former occasion, and had been thought bad because it was sandy, but it produced at that time no effects resembling those of poison. Her motherly care seems to have led her to retain for her own use the porridge made of this inferior meal, and to give the newly purchased oatmeal to the children; and it was with this barley-meal, to which it was proved that Leith had access some days previously, that the arsenic had been mixed. She herself breakfasted on the barley porridge, but she left a little, and it was tasted by her four daughters. Her son, however, who came home from his work to breakfast, did not taste any of the barley-meal, but breakfasted on the oatmeal porridge alone.

The barley-meal was easily discovered to be the poisonous article; for, whilst the boy escaped who had eaten none of it, all the others who had partaken of it were more or less affected, and to an extent proportionate to the amount of it which they had eaten—Mrs Leith being worst, and her daughter Helen, who had eaten more than the others, being worse than any of her sisters, who escaped with comparatively slight symptoms.

The well-known observation of Christison, that no peculiar taste is in general perceived by the victims of arsenical poisoning, was amply confirmed in this case. Not only was no complaint on this point made at the time by any of the five individuals; but when the question was directly put in precognition to one of the daughters, she answered, that she did not "feel any ill taste that they (the porridge) had."

The interval that elapsed between the taking of the poison and commencement of the symptoms was not ascertained with pre-

cision; but it was amply within limits which make it a perfect example of that most important part of the general evidence of poisoning, the commencing immediately or soon after a meal. The impression on the minds of the sufferers was, that the symptoms had commenced in ten minutes, and they stated so to the medical attendants. But this statement can hardly be relied upon as fixing this time with precision. The girls, all under eleven years of age, might easily be in error as to the precise lapse of time, and the mother was not in a condition to be very clear in her statement to the doctors; for by the time they saw her she was "apparently moribund," "nearly speechless, and complaining of loss of sight, pain, and confusion of head." The interval was more probably longer, as the following narrative will show. Mrs Leith called on her stepmother Mrs Welsh at half-past seven A.M., and their interview does not appear to have lasted longer than was necessary for persuading the latter to call upon Leith and ask him to send some money to his family. Mrs Welsh's interview with Leith does not seem to have been of any great duration, and she returned straight from his shop to the house, and then found Mrs Leith supping the porridge. Even supposing each of these conversations to have lasted half an hour, which is certainly more than was the case, Mrs Leith must have eaten the porridge at little later than half-past eight. At a few minutes after nine her son Andrew came in for his breakfast, and at that time his sisters were out about the back of the house, and his mother was not making any complaint of illness. Before, however, the boy had begun to take his breakfast, which was waiting ready for him, his sisters came into the house, "spitting and complaining about the barley porridge, and some of them said they had been vomiting." To this Mrs Leith replied, that it was she who might spit and complain, as they had only tasted the porridge, meaning obviously by this, not that she felt any bad effects, but that she supposed that they were complaining of the quality of the meal, which they had tasted and condemned as being sandy three weeks previously. The boy remained at home till ten A.M., when he went to his work. His mother had begun to complain before his departure, but he saw nothing serious before he left, and his mother merely told him that she thought she was taking influenza.

The symptoms here manifested themselves earliest in the more irritable systems of the children, although they took the poison after their mother, and swallowed comparatively insignificant doses of it, one of them having taken no more than two spoonfuls. Previous repletion of the stomach seems to have had no effect in delaying the action of the poison. They who were first affected had immediately before taken their breakfast of oatmeal porridge; whilst the mother, who was the last to complain, had taken nothing but the poisonous food.

It appears then probable, in this instance, that the symptoms in the children had commenced in less than half an hour from the tak-

ing of the poison; that in the mother, the illness did not commence at all till at least an hour, and that no serious symptoms had manifested themselves when the boy left, about an hour and half after the time when she appears to have supped the porridge.

There was nothing in the subsequent progress of the case requiring recapitulation here. The children all recovered. Mrs Leith died at three P.M., retaining her mental faculties till within a short while of her death. No treatment was followed beyond washing out the stomach by the pump, as, by the guilty delay of Leith, the medical men were not called till she was in the hopeless condition mentioned above. At the period of their mother's death, the three eldest of the daughters were still vomiting, but nothing further of their history is on record after that.

Of the moral and general evidence bringing the proof of the crime home to Leith, I need say nothing here, as an excellent abstract of it has been given in the former report in this Journal. It may not be amiss, however, to give a little more detailed consideration to the attempt made by Leith to fix upon his wife the imputation of trying to poison him some weeks previously.

It was quite obvious, both from the general, moral, and medical evidence, that, to use the words of the reporter of the trial, "it is one of the established facts of the case, that he put the poison himself in the tea."

Leith, at the time of this imputed crime, lived in a great measure separate from his wife; but his victuals were cooked in his own house, and sent down to him to his shop, where he generally stayed. On the occasion in question, tea was sent to him in a flagon. It was prepared, as usual, by his wife, and sent to him by the hands of one of the children. He was seen by his shopboy to drink about a cupful of the tea when it was first brought to him; but immediately thereafter he ordered the boy to take down some articles which were hanging at the shop-door, and during this proceeding he carried the tea with him into the back shop. He thus did not lack opportunity of putting poison, if he had it, into the tea. About half an hour after this the shopboy's father called on Leith, and found him in his back shop. He was retching at the time, but this witness saw only marks of spitting on the floor, but no traces of vomiting. Leith said he had pain across his chest, but made no complaint of being sick, or of having pain in the stomach; and he did not actually vomit till he had chewed some tobacco and taken some warm water. His having tobacco in his possession at all was an unusual circumstance, for he was not in the habit of using it. To this witness he did not appear anxious or alarmed, nor did he receive the proposal to send for medical aid with that avidity which would have been manifested by a man who thought himself poisoned. He stated that he had taken nothing but the tea, and that if any thing had done him harm it must have been that. The shopboy's father then looked into the flagon, and saw "white ticks floating on the top,"

and, on pouring off the fluid, found a white stuff lying at the bottom.

It is well known to every one who has made solutions of arsenious acid, that a considerable amount of the fine particles floats for some time on the surface of the liquid, and does not subside until the fluid has been well agitated. It would be wrong to assume that it was impossible that any should remain floating, after the agitation to which the tea must have been subjected whilst it was being carried by the little girl from the house to the shop, had the poison been mixed with it previously; but the fact of so much remaining on the surface as to attract the immediate observation of the witness, may be taken as corroborative evidence that the arsenic had been recently put into the fluid; and it is remarkable that it should not have been observed by Leith himself, especially as his pretended belief, that his wife intended to do him some mischief, ought to have made him more than usually vigilant.

A medical man, Dr Lyall, was at last brought to Leith's assistance by the shopboy's father. To the doctor, Leith stated that he had pain in the stomach and bowels—most probably in reply to questions; for he had not complained of these symptoms previously, nor did he at any time manifest any symptom of diarrhœa or tenesmus. He stated that he had drunk about a cupful of the tea; that he had then gone to wait upon some customers, which detained him half an hour; and that then he began to be sick. Dr Lyall, on looking at the tea, found a white powder at the bottom, which afterwards proved to be between forty and fifty grains of white arsenic.

It must be observed that, even with all this quantity in the vessel, he might have, after all, had only a very small dose of the poison, from the sparing solubility of arsenious acid in such a fluid as tea—a cupful of which, at a temperature of 200°, in one experiment by Christison, did not dissolve two grains in half an hour. Still, had the arsenic been in the tea as he pretended, Leith must have taken enough to have caused more decided symptoms than those which he manifested, especially as the tea was purposely made weak, to suit his taste, and ought, therefore, to have dissolved more of the poison than usual. If the symptoms were not equal in degree to what might have been expected, neither were they in duration. The vomiting had ceased before Dr Lyall came, and, although he ordered an emetic, it does not seem to have been taken, for Leith vomited no more after that; and when the doctor called to inquire for him two hours afterwards, he was free from symptoms, described himself as being much better, and was speaking with his accused wife, who, on hearing of his illness, came down to inquire for him. This rapid disappearance of symptoms—sometimes a most important sign of feigned illness—affords a remarkable contrast, even allowing for differences due to age, to the subsequent cases of the children, one of whom, at least, had taken only two spoonfuls of the poisoned porridge, and yet continued very ill from nine A.M. to at least three P.M.

No one on reviewing these symptoms will hesitate to conclude, that Leith had not taken the poison which he pretended to have got, and that the vomiting, which formed after all the only palpable sign of illness which he presented, was the premeditated result of his chewing the unwonted tobacco, and subsequently swallowing warm water. The moral evidence, into which it is not my province to enter at length, strongly corroborated this view of the question. Instead of preserving the vomitings for the inspection of the doctor, Leith ordered them to be thrown out. Instead of being anxious for medical aid, he seemed indifferent to it, and was not so alarmed as his visitor, who found him sick in his shop. His wife, instead of preparing the tea in a secret or mysterious manner, took a portion of that which she and her children had used, added water to it, because he liked it weak, took down the flagon in presence of a female neighbour, looked into it to see that it was clean, and in doing so, held it, by chance, so near the other woman that she could see into the vessel, and immediately sent it away by the hands of her little daughter. That the latter could have put the poison in the tea was not insinuated. Leith, previous to the "tea story," was proved to be in possession of poison—his wife was not known to have had any dealings with such an article. She, instead of shunning her pretended victim, at once went to see him when she heard that he was ill. She, whilst she strongly protested her innocence, an assertion which she solemnly repeated to the clergyman when on her deathbed, and though distressed to tears by the accusation, yet urged him, in presence of witnesses, to find, if he could, proofs of her guilt. He, on the contrary, although he often repeated the accusation himself, showed great anxiety that the doctor, the police, and the neighbours, should make no further inquiries regarding it. Lastly, the respective characters of both parties operated most powerfully in forming public opinion regarding the transaction. No one seems for a moment to have thought that Mrs Leith was capable of such a nefarious attempt—every one, on the contrary, who spoke to him about it, seemed to jump to the conclusion that it was himself who put the poison in the tea. The doctor, it is true, knowing nothing of the previous history of Leith and his wife, seeing only a man complaining of symptoms of poisoning from taking tea prepared by his wife, and found to contain arsenic, was led to form, at first, an opinion against her; but this he soon gave up on seeing her composed demeanour. Perhaps the very greatness of the quantity found in the tea, may be taken as unfavourable to the supposition that Mrs Leith put in the poison; for, although murderers do not in general trouble themselves with any nice inquiries as to the solubility of toxic agents, it is but reasonable to suppose that, in putting poison into a fluid like tea, and into a limited supply of it which she might calculate on being all consumed, the murderer would have thought of observing whether such a quantity as forty or fifty grains—about the third part of a teaspoonful, according to Taylor—might not be

lying at the bottom undissolved. It is probably from the very fear of discovery in this way, that murderers generally employ thick fluids, such as gruel or soup, as vehicles for the administration of arsenic.

I now revert to the history of the fatal poisoning of Mrs Leith. I need say nothing of the morbid appearances, the essential details of which are given in the report of the trial in this Journal. They were quite correspondent with the supposed cause of death. The nature of the poison was also amply substantiated by chemical proof. Mr Hamilton, chemist at Dundee, assisted by Dr Nimmo, and in presence of Dr Crichton, detected arsenic, by all the usual methods, in the barley-meal, in the contents of the stomach *post mortem*, and in the matters removed from the stomach during life by the pump.

The various articles were subsequently sent to me for a corroborative analysis, without the results obtained at Dundee being made known to me, and I made the following report upon them to the authorities :—

"I certify that, on Thursday 29th April 1847, I received from Mr Wilson of the Crown Agent's Office a box, containing articles for chemical examination in the case of Thomas Leith, together with an inventory of the contents of the box. The box was opened in presence of Mr Wilson, and found to contain four bottles, a wide-mouthed glass jar, and two paper packets, corresponding with the inventory. The bottles, jar, and packets, were duly sealed; the seals were unbroken. The jar, on being opened, was found to contain the larger portion of the œsophagus, the stomach, and the greater part of the small intestines of a human adult. The mucous membrane of the stomach and bowels throughout, presented appearances indicating extensive inflammation. The surface of the mucous membrane of the stomach, which had previously been opened, had a tenacious mucus adhering to it, among which there were found minute crystalline grains, having the form and appearance of arsenious acid. The viscera were free of putrid smell. The jar likewise contained about three fluid ounces of a ropy bloody fluid, with a white mucous deposit at the bottom. This fluid, with the deposit, was examined chemically for arsenic. The process followed was that of Reinsch. [The process was here described in the report.] The copper, on being removed from the fluid, was covered with a steel grey crust. It was washed, dried, and heated in a glass tube, with free contact of the air. The heat produced a copious crystalline sublimate, the appearance of which, under a magnifying glass, sufficiently showed it to be white arsenic; but this was unequivocally established by dissolving the sublimate in distilled water, and subjecting the solution to the tests of the ammonio-nitrate of silver, ammonio-sulphate of copper, and sulphuretted hydrogen—all of which gave at once the reactions characteristic of arsenic.

"The presence of arsenic in the contents of the jar being so indubitably established, I deemed it unnecessary to make any experiments with the textures of the stomach and intestines.

"The bottle marked No. 1 in the inventory, and labelled as containing what was removed by the stomach pump from the stomach of Mrs Leith, was uncorked, and found to be full of a pale yellow slightly turbid fluid, with a feeble sour smell, and an acid reaction. Twelve fluid ounces of this were mixed with pure muriatic acid, and boiled with a piece of polished copper. The copper, after boiling, had a distinct grey crust, and when heated in a tube, as described above, gave also a crystalline sublimate of small amount, but sufficient, when dissolved in distilled water, to give all the characteristics of arsenic with the tests formerly mentioned.

"The paper packet, No. 6 of inventory (labelled as containing barley-meal found in Mrs Leith's house), was found to contain about half an ounce of a white powder, abounding in starch, and therefore apparently flour or barley-meal. About an eighth part of this powder was subjected to Reinsch's process, and gave a very copious crystalline sublimate, which, when dissolved in water and tested, gave the reactions characteristic of arsenic.

"I did not think it necessary, after the above results, to subject any of the other articles to experiment."

The other articles were merely more of the matters removed by the stomach pump, the washings of the tub in which the vomitings had been put before they were thrown out, and the bag in which the barley-meal had been contained.

The appearance of the stomach and intestines before any analysis was commenced, decidedly pointed towards arsenic, and led to that poison being at once specially sought for. The bright red inflamed appearance of the viscera, the absence of corrosion, softening, or blackening, and the complete absence of all appearance of decomposition after eight days of tolerably warm weather, indicated irritant poisoning, and the peculiar irritant to be arsenic. The antiseptic powers of arsenic are now thoroughly established, and constitute one of its most marked peculiarities. In this instance there was not only no positive putridity, but even that feeble but somewhat persistent cadaveric odour, which is familiar to every one who has dissected fresh bodies or organs, was awanting. As a general rule, where viscera with signs of inflammation are firm in texture, and in an unusually good state of preservation, arsenic may be suspected to be present, and to be in considerable quantity.

Before quitting the present instance of arsenical poisoning, I beg to offer a few remarks on the two most approved processes, those of Reinsch and Marsh, now in use for detecting arsenic in medico-legal analyses. The former may be said to be distinguished for its facility and celerity, the latter for its delicacy.

In any case, such, for example, as the present, where there is reason to expect a considerable proportion of arsenic, and where we have plenty of material to work upon, it is always best to have recourse at once to Reinsch's method, on account of its easy and rapid execution. As I have frequently found, not only in experiment, but in actual medico-legal investigations, the whole process, including the preparation of a tube for the experiment, may be satisfactorily done in less than half an hour. Where, however, the quantity of material at command is small, and the proportion of arsenic probably minute, the more delicate method of Marsh ought to be employed at once. Reinsch's method cannot be said to be remarkably delicate. I have failed to obtain any satisfactory evidence of the presence of arsenic when it was applied to a thousandth of a grain in two fluid ounces of liquid. Dr Christison says, that it will detect "at least a 250,000th part of arsenic in solution;" but this applies to the state of dilution, without reference to the quantity of arsenic present. Mr Taylor says it will act when a 3000th of a grain is used under a dilution of

90,000 times its weight of water; but the action in his experiments appears to have been limited to the mere production of a stain on the copper. This is acknowledged to be no true characteristic. The bright copper is frequently tarnished when there is no arsenic present, and where all the necessary precautions in conducting the experiment have been attended to. The circumstances which occasionally lead to the formation of a stain very like that of arsenic, do not seem to be accurately determined. It will always almost occur in presence of organic matters, if the copper is put into the acidulated fluid before it is duly heated. Reinsch's process cannot be said to have acted satisfactorily for medico-legal purposes, unless it produces first a stain on the copper, and then a distinctly recognisable white sublimate. It is, however, a method capable of demonstrating very small quantities of arsenic. Mr Taylor detected the 144th of a grain in two fluid drachms of liquid. I have obtained all the indications of the presence of arsenic from 100th of a grain in two fluid ounces of thick soup.

The length of time during which the ebullition should be continued, has been, I think, rather overstated by both Christison and Taylor. The former says (4th edition, p. 272), "In the feeblest solutions, from ten to fifteen minutes elapse before arsenic is visibly deposited, and forty minutes should be allowed for complete deposition; but in strong solutions the action takes place in a few seconds." Mr Taylor says (on Poisons, p. 353), "one caution is to be observed, *i. e.*, not to remove the copper from the liquid too soon. When the arsenic is in minute quantity, the deposit does not take place sometimes for a quarter of an hour." As the amount of arsenic meant by the terms "feeblest" and "minute," has not been specified, I may mention an experiment bearing on this point, made with a definite quantity of arsenic, not, however, probably so small as to bring it strictly within either of these categories.

One hundredth of a grain of arsenic in two fluid ounces of acidulated water, was boiled in the usual way with a piece of copper. After having been boiled for four minutes, the copper was found to have a slight grey crust, in eight minutes the crust was distinct, and in ten minutes the process was stopped. It was found, after this amount of boiling, not only that the copper afforded distinct evidence of arsenic, but that the whole had been removed, as none could be detected in the liquid by Marsh's method. A similar experiment, made with the same amount of arsenic in an equal quantity of thick broth, gave equally satisfactory results after ten minutes' boiling.

There does not appear, however, to be any practical disadvantage in prolonging the boiling. The mere coloration of the copper by the prolonged action of the acid, is, as Mr Taylor has accurately stated, not a source of fallacy, and the scaling off of deposited arsenic by prolonged ebullition, has appeared to me to occur only when the arsenic was in large quantity; and in such a case the long boiling is not

required. In any case, where, after ten minutes' boiling, I find, on examining the copper, that it does not appear coated with arsenic, I remove the copper, boil the materials with the acid for some time longer, filter off the fluid, and transfer it to a Marsh's apparatus.

The peculiar form of the piece of copper, provided it be bright and clean, is not a matter to which I attach any importance. I give the preference to a piece of copper wire, of about the size called No. 24 by the dealers, made bright by rubbing it with a piece of sand-paper, and rolled into the form of a loose spiral coil, of about an inch long, by twisting it round a small pencil or glass rod. When of this form it is more easily caught and removed when it is immersed in a thick mixture of organic matters than a "slip of copper foil or wire" [Taylor], or "copper worn thin by the action of nitric acid" [Christison]. It affords, in a piece of moderate length, an extensive surface for the deposition of the arsenic, and thus expedites the process; and it is more readily washed, and seen to be free from adhering organic matters than copper gauze. In operating by Reinsch's method, I never make any preliminary filtration of the decoction, but boil the whole solids, if any be present, broken down as much as possible, with the acid, adding water if necessary, and at once immerse the wire coil in the mixture.

Marsh's process is, by universal consent, the most delicate method of ascertaining the presence of arsenic. It is no part of my object to comment upon the innumerable forms of apparatus which have been devised. By far the most convenient is the common Döbereiner's lamp, as figured by Christison.

The purity of the acid, sulphuric or muriatic, and of the zinc and water, having been duly ascertained, and the stop-cock and ground neck of the apparatus having been found by experiment to be air-tight, so as to avoid any risk of loss of the gas, the suspected materials are to be boiled in water along with a portion of acid, and filtered through a piece of well-washed calico. It is then quite fit, in almost every instance, for being placed in the apparatus. The proportion of acid should not be great—the slower the gas is evolved the better. It is convenient to set the process going at the end of the day, and leave it all night. The apparatus is found full of gas, and ready for the decisive trial in the morning. The frothing up of the fluid, which has been such a bugbear to many experimenters, and has led to the troublesome and hazardous process of incineration, is productive of no practical inconvenience if this precaution is attended to.

The gas is now ready for being decomposed, so as to separate the arsenic. If the arsenic be in large quantity, it is of little consequence by what method the reduction of the arseniuretted hydrogen is accomplished. When the amount is minute it is best to adopt the method first proposed by Berzelius, of passing the gas slowly along a small glass tube, raised at one part by a spirit lamp to a low red heat, and thus obtaining a metallic ring, or more correctly incrustation, for it is seldom truly annular, in the interior of the tube.

In procuring stains on glass or porcelain, without considerable dexterity on the part of the operator, the quantity of arsenic, if very small, may be entirely lost; but in the use of the tube, if the gas is only passed along with moderate slowness, no loss can be experienced.

The most convenient tubes for this purpose are of German hard glass, about a twelfth of an inch in bore, and need not be more than five inches long. They are drawn out and turned up at the point, leaving in the apex a very small orifice, at which the disengaged hydrogen may be burned, and thus afford, by the size of the flame, a criterion of the rate at which the gas is passing. The reduction tube is connected with the nozzle of the apparatus by a portion of wider tube about two inches long, filled with dry cotton wadding, as recommended by Christison. The tube is to be heated at a little beyond its middle, till it appears feebly red when in shade. The tube, if of good glass, ought not to bend, and any tube which does so readily, or blackens when simply heated, from being made with lead, ought to be rejected. This method of operating, the simplification of which is due to Dr Christison, is so simple, easy, and sure, that all porcelain plates and watch glasses, and the ingenious tube devised by Dr Christison himself, for burning the gas and collecting the products of combustion, may be dispensed with.

It is however notorious, that the duty of the medico-legal analyst does not terminate with the procuring an incrustation on his tube or porcelain plate. What has been done is merely the elimination of something, the arsenical or non-arsenical nature of which remains to be determined.

Various matters have been indicated as being capable of producing deposits which may be mistaken for arsenical stains; but most medico-legal writers are now agreed, that antimony is the only one which offers any really practical source of fallacy, and hence a variety of methods have been described for distinguishing with rigorous precision antimonial from arsenical stains.

When the metallic stain is large this question is easily settled. Sublimation will in such a case yield enough of arsenious acid, if the deposit be arsenical, to afford precise characters by the liquid tests. But we may have to distinguish between arsenic and antimony, where the quantity would not furnish enough of solution for testing with two or three separate re-agents. The action of nitric or nitro-muriatic acid is that which has commonly been had recourse to for distinguishing stains of small extent. This, as is well known, converts the metallic arsenic into the very soluble arsenic acid, which gives a brick-red precipitate with nitrate of silver; whilst antimony so treated gives an insoluble product, and makes a grey stain with the silver test. But this proceeding is not easily applied, except to stains on a flat surface; and in procuring these there is the risk of a considerable or total loss of the arsenic, if the quantity be very minute, and the operator not very dexterous. The most complete me-

thod of distinguishing arsenic from antimony is that of Devergie [Ann. d'Hygiène, xxxvi. 121], which may be applied to stains either in a tube or on a flat surface, and has the advantage, that the whole series of reactions may be applied to a single stain without the necessity for dividing it, for the purposes of experiment, into separate portions. This method consists in, 1st, exposing the deposit to chlorine gas, which causes the arsenical stain to disappear by converting it into chloride of arsenic. 2d, The chloride so formed is exposed to sulphuretted hydrogen, which produces the pale yellow sulphuret of arsenic. 3d, This is treated with a very weak aqua ammoniæ, which makes with it a colourless solution; and this in its turn, being gently heated, reproduces the yellow sulphuret as the ammonia volatilizes. 4th, This yellow sulphuret is treated with a few drops of nitric, containing one drop of muriatic acid, and on evaporating this to dryness, white rings of arsenic acid are left, which, from their deliquescing under the moisture of the air, speedily become invisible. 5th, The spot moistened by the deliquesced arsenic acid is touched with nitrate of silver, which produces the dirty red stain of arseniate of silver.

This series of actions, if successfully evolved, is entirely conclusive for distinguishing a purely arsenical from a purely antimonial stain; but by M. Devergie's own showing, its success is a good deal dependent on the dexterity and nice manipulation of the operator. Devergie has, to a considerable extent, rebutted some of the objections which have been urged against his method. They are hardly valid where the stain is tolerably large; but he has given no positive statement as to the delicacy of his series of tests, or of how small a quantity of arsenic or antimony may be distinguished with certainty by its use.

There is one distinction between arsenic and antimony to which enough of attention has not been paid, and by which, I believe, they may be easily and readily discriminated, even in very minute quantity, viz. the difference of temperature at which they respectively undergo sublimation. That such a difference exists, is alluded to in general terms in most medico-legal works; but I am not aware that the comparative effects of a regulated temperature on the metallic stains has been adopted as a means of distinction, and adduced in evidence, except by Mr H. H. Watson, in the case of the *Queen* v. *Johnston*, Liverpool Lent Assizes, 1847. In the quotation of this evidence, as given by Mr Taylor in his appendix, Mr Watson says —"I also exposed some of these metallic deposits on glass, to a temperature ranging from 355 to 565 degrees, by which they were volatilized and left the glass. This is another proof that the metal is not antimony, but arsenic; antimony does not volatilize at the temperature mentioned, but remains permanent, while it is one of the properties of arsenic to become volatilized at that temperature."

It appears to me that, when this property is satisfactorily observed, there can be no possible mistake as to the stain being arsenical, and

not antimonial. Mr Watson does not state how he applied and regulated the heat; but I presume that it was by the oil bath. My experiments lead me to the conclusion, not only that it is the easiest and simplest way of distinguishing arsenic from antimony, but that it is a perfectly satisfactory method of separating them, and that they can thus be distinguished and separated in an unmistakeable manner, whether the stain be large or small.

The exact point of the thermometer at which metallic arsenic and arsenious acid sublime, is still *sub judice*. The arsenious acid is commonly stated to sublime at 380°, but, according to Dr Mitchell of Philadelphia, it requires a temperature of 425°. My own observations lead me to fix on 380° as the temperature at which, in a narrow tube, open at one end, arsenious acid begins to sublime. The metallic arsenic, commonly said to sublime at 356°, does not, according to Mitchell, volatilize except at a low red heat, luminous in the dark. I have not made special experiments for determining this point; but I have never been able to observe it sublime below 500°, unless it became oxidated. In a medico-legal point of view, however, this is not the present question. What we have to consider is, if we can, by a simple means, obtain a regulated heat at which metallic arsenic will sublime and become oxidated, whilst antimony will undergo no such change.

This we can easily accomplish by means of a bath of olive oil, which may be urged even to its boiling point without at all affecting an antimonial stain, whilst the heat so employed will entirely sublime an arsenical crust into a crystalline sublimate of arsenious acid. It will always be best, if it is possible, to have a thermometer in the oil bath, that the extreme temperature employed may be stated in evidence if asked for. But this is not indispensable; olive oil does not begin to boil till the heat rises above 600°; and this heat, however long applied, does not cause antimony to volatilize. Stains which are so faint as not to appear distinctly metallic till the tube is held over a sheet of white paper, may be distinguished in this way. The pure arsenical metallic stain entirely disappears from the spot where it was deposited, the pure antimonial remains unchanged; whilst the mixed arsenical and antimonial becomes visibly less, a portion has undergone sublimation, and the residue, however long the heat may be prolonged, remains fixed. If, in addition to the disappearance of the stain from the portion of the tube immersed in the oil, we can observe the formation of a crystalline sublimate in the upper portion of the tube, the proof may be said to be absolute. Very small quantities of arsenic may be rendered distinctly visible in this form. I have operated upon stains produced from a Marsh's apparatus, which contained less than a thousandth of a grain of arsenic, and have yet been able to see distinctly the crystalline character of the sublimate. The gentle and gradual way in which the heat is applied in the oil bath, causes the sublimate to deposit itself in fewer but much larger crystals, and thus makes it much more appreciable

by the eye or lens than could be supposed by those who have been in the habit of subliming small stains of arsenic by a spirit lamp flame. In the case of some poisoned swine which I examined, and where, from one of the articles, I could obtain in the tube a mere shade of brown, and when, by the spirit lamp, this was sublimed into a mere white cloud, I was able, by again heating in the oil bath, to obtain a sublimate distinctly of crystalline appearance to the naked eye.

In order to test practically the value of this method of distinguishing arsenic from antimony, I made the following experiment, in which I was kindly assisted by my colleague, Dr Anderson. I requested him to prepare for me, in tubes, a series of stains from Marsh's apparatus, some of which should be arsenical, some antimonial, and others a mixture of both, and I proposed to distinguish these by the unaided operation of the oil bath.

Dr Anderson accordingly prepared for me six such tubes, which he duly numbered and noted.

No. 1 was accidentally mismanaged. Unreasonably trusting to my friend having taken the trouble to seal the tubes, I plunged this one into the oil with the point open, and it was of course filled with oil. Had such a misfortune occurred in a medico-legal investigation, it would have been easily rectified by immersing the tube in pure ether, which would dissolve out the oil; but as the crust in this case was large, probably the fiftieth of a grain, I felt so confident of success that I contented myself with merely blowing out the oil from the tube, then sealed up the point, and again placed it in the oil. The distinct crystalline sublimate, which speedily formed, showed at once that it was arsenical, and as it in time entirely volatilized, I pronounced against the presence of antimony.

Tube No. 2 presented a faint brown stain about two lines in length. Its brown metallic appearance was distinct only when it was held over white paper. It was pronounced to be arsenic alone, because it entirely disappeared, but it was so small as not to afford an unequivocally crystalline sublimate.

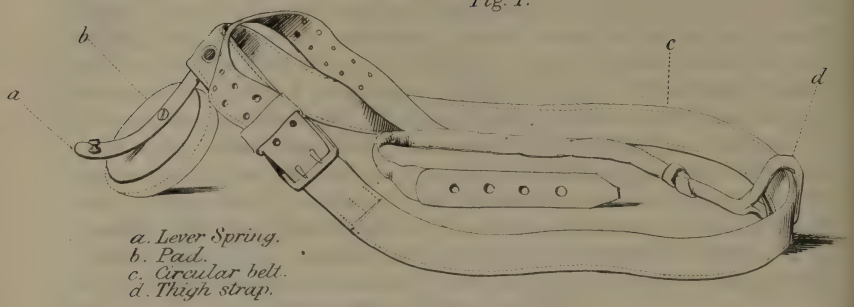
No. 3, on being heated for about ten minutes, presented at the upper part of the tube a faint crystalline sublimate, but a large metallic stain remained below. It was subjected to a prolonged heating, and no more sublimation could be observed. It was pronounced to be antimony with a small proportion of arsenic.

No. 4 gave a copious distinctly crystalline sublimate in a few minutes, and the whole stain eventually sublimed. It was recognised as a large arsenical stain, as in No. 1.

No. 5. The stain in this tube was so faint as to appear greyish-white when held to the light, but it was observed to have a faint brown tint when held over white paper. It was heated for a length of time, but underwent no apparent diminution. It was pronounced to be a trace of antimony.

No. 6, a large stain was not affected by heating for above half an hour. It was evidently a large antimonial stain.

Fig. 1.



a. Lever Spring.
b. Pad.
c. Circular belt.
d. Thigh strap.

Fig. 2.
Evans's Mor- main lever truss applied.

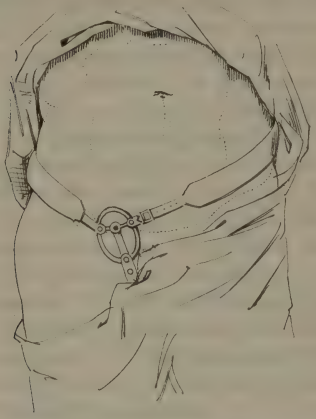
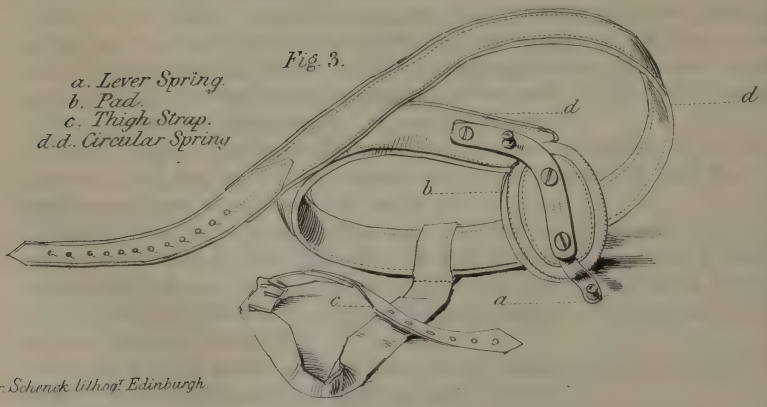


Fig. 3.

a. Lever Spring.
b. Pad.
c. Thigh Strap.
d.d. Circular Spring



Fr. Schenck lithog.r Edinburgh

ILLUSTRATIONS OF Dr THOMSON'S PAPER ON VARICOCELE.

to face page 295.

In each of these instances I found that I had pronounced correctly. The minute quantity distinguishable by these simple means, may be inferred from this, that, in No. 5, the Döbereiner's lamp contained only three-thousandths of a grain of tartar emetic, and as this salt contains only 35·9 per cent. of metallic antimony, there could not have been in the tube more than a-thousandth of a grain of metallic antimony. But as only one charge of gas was used, it is most probable, from the faintness of the stain, that only a portion of this was collected. In order, therefore, to determine whether a stain be arsenical or antimonial, all that is required is to operate with Marsh's apparatus and the narrow glass tube; if a stain is procured, to seal up the point, immerse it in the oil bath, and heat this steadily. The heating does not require to be prolonged. Ten minutes after the temperature has risen to about 500, will have affected the stain if it is arsenical. It is well remarked by Taylor, that, in such investigations, we have to determine the presence of arsenic in antimony, not of antimony in arsenic; and therefore, if any sublimation in the oil bath can be observed at all, the question as to the presence of arsenic is settled. To enable us to observe this more readily, it is a good plan to make a small scratch on the tube at each limit of the stained portion before heating it, and thus, by its diminution, we may pronounce upon its nature, although no sublimate should be distinctly visible. Should any peculiar case occur, in which it might be of importance to determine that antimony was present, as well as arsenic, the heat must be continued for a longer period. I have found that a large pure arsenical stain, weighing on a delicate balance 0·036 grain, required an hour and a half of heating at 500 in a narrow tube to sublime it entirely. But long before one-third had been sublimed, the tube was lined with splendid crystals of arsenious acid. The heat, to sublime the whole arsenic, need never be raised beyond 520, but, even if the oil boils, it does not affect the correctness of the experiment.

I believe, therefore, from what I have been able to observe, that the nature of a metallic stain may, in this way, be accurately determined without the employment of any chemical re-agent whatever.—(*To be continued.*)

Art. II.—*Varicocele treated by Pressure.* By Lawrence Ramsay Thomson, M.D., Edin., Dalkeith.

The following pages have been written with the view of inducing surgeons to try a plan of treatment for the cure of varicocele, the nature and success of which seem little known to the profession in general. That it *is* little known, I am led to believe from conversations with our principal surgeons in Edinburgh on the subject; and from the fact that, in the medical journals of the day, little mention is made of this safe and successful method of cure. On the other hand, scarcely a periodical appears that has not a description of one or other

of the numerous and hazardous operations that are continually practised for the radical removal of this complaint.

Mr Curling of London, so far as I am aware, is the only one who has published cases of varicocele cured by this plan: and here I gladly seize the opportunity of acknowledging the benefit I have derived from what he has written on the subject.[1]

In the monthly *Retrospect* of this Journal for August last, there is given a case of cure by M. Vidal's process, which, with the exception of M. Breschet's plan, is perhaps the most generally successful of the seven or eight different methods which have been resorted to for the obliteration of the spermatic veins, when affected by this disease. But we not unfrequently hear of disastrous consequences following even this operation. A few days ago, Mr Syme mentioned to me that he had seen lately, in London, a melancholy case of fatal phlebitis following an operation for obliteration of these veins.

In having recourse to any means for the relief or cure of varicocele, it ought always to be borne in mind that the affection itself is not at all a serious one; and, therefore, that no surgeon is justified in having recourse to any dangerous expedient for the removal of a disease only troublesome, not dangerous in its effects. If warranted in any extreme case—as when much pain is experienced, and when injury to the testes may be threatened—an operation ought only to be had recourse to when palliative measures and the method I am about to explain have failed in giving relief. Mr Syme, and other judicious surgeons who have not yet tried this plan, seldom or never now have recourse to such dangerous procedures as those of Velpeau, Breschet, Ricord, &c.—thinking it better to be content with palliative treatment than to risk the lives of their patients, by performing operations fraught with much danger. This palliative treatment consists of frequent bathing of the parts in cold water, together with the use of a suspensory bandage, or, in urgent cases, excision of a portion of the scrotum, to serve the same end more completely, and with less trouble to the patient. The curative measure we now propose is not more troublesome than an ordinary suspensory bandage, and certainly greatly to be preferred to the latter of these modes of trussing. We hope the day is not far distant when the details of such doubtful and hazardous operations as those of Velpeau, &c., will only be interesting as matters of surgical history. As a means of accomplishing this desirable end, we confidently look forward to the *pressure* plan of treatment doing much.

The plan appears *a priori* opposed to the intentions to be fulfilled in the treatment of varicose veins in general. I hope, however, to be able to show in the observations to be made on the following case, that the plan is not only sound in practice, but intelligible in theory.

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[1] "Nor am I aware of any one else having written on this subject." Mr Curling's letter, hereafter quoted, August 22, 1848.

In several instances I have recommended this practice to other practitioners; but only in a few instances, as yet, have I personally superintended this method of treating varicocele. The following is the only case that has been under treatment sufficiently long to render it worthy of detail.

CASE.—*Large Varicocele on the Right Side, upwards of Twenty Years' standing, completely relieved by Pressure.*—This varicocele, which consisted of a plexus of dilated veins wholly surrounding the body of the testicle, and extending up the inguinal canal, gave rise for many years to great uneasiness, consisting principally of a disagreeable sense of dragging and weight from the loins, and occasional sickness after much exertion. Standing long in one position gave rise to the same; and sometimes to a dull aching pain in the affected parts. The testicle of the affected side was not smaller, as often happens in such cases, but felt somewhat softer than that on the sound side. Two surgeons, who saw the case about four years ago, were of opinion that it was one of scrotal hernia; and accordingly recommended a spring truss to be applied. This was done for a time, but could not be persevered with, as the ordinary hernial truss did not prevent the tumour re-appearing—when pain was produced to such an extent as to require immediate removal of the instrument. About this time Professor Syme was consulted, who soon detected the real state of matters. He immediately forbade the use of the truss, and ordered the local bathing with cold water once or twice a-day—and that the trowsers of the patient should be worn well *braced up*. By this means, the trowsers being worn so as to exert not only pressure over the scrotum, but considerable pressure on the dilated veins at the external ring, great relief was given to local and general, including mental, uneasiness.

As is common in cases of varicocele and other secret disorders in early life, there was great mental distress in this case—out of all proportion to the actual disease. At times the depression of spirits had been such as to interfere with all mental exertion of any useful description. Much of the relief afforded in this last respect, I have no doubt, was due to the assurance given, that the affection was not a serious one—at the worst only an inconvenience. But that the relief mainly depended on the pressure exerted in the manner described, was proved by the circumstance, that when trowsers were used that did not admit of this *bracing*, the pain and many of the other symptoms immediately returned. Here it may be stated, that no sensible benefit had been derived from the use of the ordinary suspensory bandage. It was my knowledge of the effects of the bracing on the dilated veins, that led me to recommend for this case a simple contrivance, that might exert the necessary pressure independently of the trowsers. This bandage (see Fig. 1, of Plate) consisted of one strap or belt to encircle the abdomen, with a pad projecting from the right side of it, to be retained over the tumour in the inguinal canal, and the pressure there regulated by means of a thigh-strap. This afforded relief, but still was not sufficient to remove the disagreeable sensations already described.

Some time after this I was much gratified to find, on reading Mr Curling's valuable work "On the Testis," that he had recorded there two cases of varicocele, one of which had been completely relieved by pressure—exerted by Evans's patent lever truss—in twelve months, and the other, as subsequent observations[1] have shown, completely cured in fifteen months. Although opposed to preconceived notions of the indications to be fulfilled in the treatment of varicose veins, yet from what I had seen of the benefits of pressure, as already stated, I had no hesitation in giving Evans's truss a fair trial. Before applying it, Mr. Curling was consulted, when, on examining the case, he

[1] London Med. Chirurg. Transactions, vol. xxix.

at once agreed with me in thinking it one of decided varicocele, and, moreover, very favourable for treatment by pressure. Accordingly, Evans's patent (moc-main) lever truss (Fig. 2) was applied over the external ring. The patient, as the result of this trial, was not more astonished than delighted to find that, for the first time in his own recollection, no tumour appeared in the scrotum on rising from the recumbent posture. Now and then, however, the tumour reappeared when violent exercise was taken on horseback, or otherwise. From this circumstance, and other reasons that will be mentioned in the subsequent observations, if space permit, I was led to invent a more efficient truss for this case of old standing varicocele. This instrument (Fig. 3) consists of a small circular spring, with a pad attached to it, so constructed that it can be made to extend as little and as much beyond the external ring as may be thought necessary for the extent of the affection, or as the shape of the pelvis may require. On the outer surface of the pad is fixed the same simple lever spring that I had made for the first truss, and is nearly the same as that on the moc-main truss—which I would recommend only for slight and recent cases of varicocele. To the free end of this spring is attached the ordinary thigh-strap, by which, while steady pressure is kept up over the inguinal canal, the pressure can be increased or diminished at will over the external ring. Since the employment of this last instrument, now ten months ago, though most violent exercise has been taken, preternatural dilatation of the veins has not once occurred; and, during the whole of that period, perfect immunity has been experienced from all the distressing annoyances of a varicocele. The patient is now apparently free from the disease, and both testicles feel alike natural.

This case is interesting in several particulars. The varicocele was on the right side, had been of long standing, had occurred long before puberty, and had been mistaken for a hernia by two experienced surgeons. Before touching on any of these points, it will be as well here to explain as far as possible the *modus operandi* of the *pressure* plan. As already remarked, it is opposed to former belief; namely, that all pressure exerted on a vein, short of causing obliteration between a varicose enlargement and the heart, must increase, not diminish such enlargement.

I am convinced that many surgeons have been deterred from trying the plan I am now advocating, from the erroneous impression that the force applied must be such as to cause obliteration of the veins. Professor Miller, in his treatise on practical surgery, mentions the possibility of applying such force by means of a spring truss; but, at the same time, informs his readers that it is the "most objectionable of the modes of cure."[1] Fortunately, the plan I am advocating does not aim at obliterative pressure—it only seeks to restore the diseased vessels to their natural tone and calibre. This is to be effected by maintaining such an amount of firm, steady, and equable pressure over the external abdominal ring as shall not permanently obstruct, merely afford the weakened vessels proper support at that

[1] Mr Miller recommended in the case given in the text *gentle* pressure, to be applied by means of a spring truss, not as a curative but palliative measure, and mainly to prevent herniary protrusion. This last we think an important end to be gained by the use of the truss in cirsocele; for we cannot help thinking, as stated by Mr M., that varicose veins extending up the inguinal canal may pave the way for hernial descent.

point, so as to remove the superincumbent weight of the blood from the distended veins below. That the pressure shall temporarily supply the place of valves, rendered incompetent from over-distension, is the important desideratum. When the vessels shall have regained their wonted strength and size, then the valves will resume their proper function of resisting hydrostatic pressure. After this end is gained, a cure is established. When properly applied, there is not the slightest risk of the pressure acting injuriously on the testicle, by interfering with the circulation of the spermatic artery. So far from such a result occurring, the testicle will resume its natural size under the influence of pressure, though formerly atrophied on account of the affection. The venous return will be carried on by the vessels not affected with the disease, and probably also to a certain extent by some of the diseased vessels themselves. We are aware that some difficulties in relation to the forces moving the blood, and the pressure exerted by that fluid on the vessels, are presented to the clear comprehension of this theory. And we would here observe, that we rest the claims of this mode of treatment on its practical results, and not on the theory advanced to explain it.

It will be seen from the foregoing statements that a nice adjustment of pressure is necessary. The instrument used by Mr Curling for this purpose is what has been called the moc-main[1] lever, or Evans's patent truss. This instrument, as will be seen from the drawing, has only a lever spring. Though devoid of a circular spring, I believe it to be sufficient for the cure of most cases of cirsocele, but not without causing great discomfort, when much pressure is necessary to support the superincumbent weight of the blood. In cases of old standing, such as the one given in this communication, it may even prove unequal to this task. The discomfort does not arise from the pressure exerted by the pad, for it seems superior to others in this respect (see foot-note), but from the tightness with which it is necessary for the encircling belt to be drawn when much pressure is wanted. The moc-main truss being faulty in these two particulars, *i. e.*, not always able to keep up the varicocele, and giving rise to uneasiness from the tightness with which it is sometimes necessary to fasten the encircling belt—I was led to contrive a different and more efficient kind of instrument. This is a combination of the ordinary spring and lever truss; it has therefore a circular and lever spring, to the former of which the pad is attached (as described at p. 298) in such a manner as to admit of slight elongation. The pad may or may not be stuffed with moc-main; the simple cork and

[1] The truss is so called after the name of the material with which the pad is stuffed. "Moc-main" is the produce of the silk cotton-tree, *Bombax heptaphyllum*, a plant which grows abundantly in the East Indies. It is of a white, shining appearance, and possesses great lightness and elasticity, on account of which the pad stuffed with it "allows," as Mr Curling has stated, "of the application of greater force than could be tolerated under other circumstances."

flannel pad will be easily borne in most cases. The length of the pad should depend upon the extent of the disease. Some months ago I was consulted by a person labouring under a varicocele of considerable size, accompanied with oblique inguinal hernia. In this case I caused the pad to be made a little longer than usual, so as to press firmly over the internal as well as external ring; and also, to have the encircling spring made stronger than usual in the truss I advise for varicocele. Cases of this kind are continually neglected, from ignorance of the plan of cure I am recommending, inasmuch as for their relief only a palliative measure is generally prescribed. Very lately I was told of a case of large varicocele, complicated with reducible hernia, where the patient suffered much, and where the surgeons consulted advised only a suspensory bandage, thinking that the condition of matters would only be aggravated by the application of a spring truss. I need scarcely say, that when the case was mentioned to me, I recommended the person should procure a truss with such a pad as I used in the case just alluded to. In the formation of the pad, care must be taken that it be not made too conical; for, if so, the veins may be separated instead of supported. Between the skin and the pad there should always be interposed a piece of chamois leather, which can be changed as often as necessary. We here recommend, as an important adjunct, that the trowsers be worn so as to take off the weight of the testicles from the cord. The thin circular spring (not required so strong as in hernia) of this truss, if well fitted to the body, must be much more agreeable than a belt tightly encircling the pelvis. But the instrument I have contrived has other important advantages over that hitherto used by Mr Curling. From the oblique direction of the ends of the encircling belt of that used by Mr. C., when attached to the pad and on the body, there is a continual striving for displacement of the pad upwards, which, to a certain extent, does away with *direct* pressure over the spermatic veins; also, if the thigh-strap of the moc-main truss become disengaged (as often happens), the whole pressure is instantly removed. In the truss I recommend for varicocele, the responsibility of the pressure is divided between the circular and lever springs; so that, even if the latter should become temporarily useless from disengagement of the thigh-strap, the pressure of the former is still sufficient for a time to prevent distension of the veins. Again, the moc-main truss requires the thigh-strap to be always more or less tight, for upon it its usefulness entirely depends; whereas the thigh-strap in my truss need not always be tight,—in fact, may occasionally be dispensed with altogether. Pressure is kept up over the important point (the external ring), to a certain extent, without the thigh-strap. It is only, therefore, when much and violent exercise is taken, that it need be tightly applied.

Diagnosis.—A knowledge of the *pressure* plan of treatment is highly important in the diagnosis of varicocele. Ignorance of it caused the above case to be mistaken for scrotal hernia. The

surgeon who did so stated, when consulted,—"You see that the tumour disappears on the patient assuming the recumbent posture, but does not reappear, while my fingers remain firmly pressed over the abdominal outlet, on the patient rising to the erect posture. If it had been a varicocele," said he, "it would have reappeared; being a hernia it does not." The same doctrine is very generally taught in systematic works. To show this we make no selection; we only quote from the works that happen to be on our table. "Pressure is then made at the ring and the patient rises, when the swelling will reappear if depending upon vascular enlargement, as the blood cannot thus be prevented from finding its way through the arteries; but the tumour will not return if of a hernial nature."[1] "Unlike hernia, there is return of swelling on resumption of the erect posture and on abdominal exertion, though the thumb be kept accurately and firmly placed on the abdominal outlet."[2] Although the first quotation does not say positively, like the second, that the pressure must be *firm*, yet it is sufficiently calculated, in many instances, to mislead. Mr Syme himself admits, that the test, as given in the above quotation, is "frequently ambiguous;" he was therefore too much the practical surgeon to trust to it in the above case. He had met with a few cases where, by pressure, he said, it was quite possible to keep up a varicocele for a short time, and he looked upon this case as one of the same kind. Had the effects of pressure, exerted by a truss such as I have recommended, been tried in these cases, it would have been found that the veins might have been kept *permanently* free from morbid dilatation. It is in cases of this kind—namely, in those where the veins are found to be relieved by the firm application of the fingers to the external ring—that the treatment is most applicable. Indeed, we feel convinced that, in all cases, other things being equal, in which firm pressure exerted by the fingers is sufficient temporarily to keep up a varicocele, a cure is to be regarded as certain when adequate pressure is applied permanently by means of a truss. The case I have given goes to prove this, as do those published by Mr Curling.

It is very frequently stated in books, that the diagnosis between varicocele and hernia is an easy matter. The case detailed proves the contrary. To make a correct diagnosis is of great consequence to the patient, as the application of an ordinary oblique hernial truss, where the principal pressure is wanted over the internal, and not, as in the case of varicocele, over the external ring, may seriously inconvenience the patient, and aggravate the disease. We have the testimony of Mr Syme as to the difficulty of diagnosis when the dilatation occurs high in the cord, in which case the dilated veins receive an impulse on coughing, and in other respects resemble an inguinal hernia, particularly one consisting of omentum. As the

[1] Syme's Principles of Surgery. 1837. P. 362.
[2] Miller's Practice of Surgery, p. 366.

most certain means of ascertaining the truth, the same author directs the surgeon "to compress the neck of the swelling while the patient stands erect, when, if composed of dilated veins, *it will become more tense.*" There is a statement made by Mr Curling,[1] which would show that this plan may also prove fallacious. He says, "In a patient affected with this disease, if the spermatic cord be pretty firmly compressed between the fingers whilst the patient is in the recumbent position, and the vessels are empty, it will be found, on his assuming the erect position, that the vessels, instead of swelling as before, still remain empty and contracted. Even, too, when the patient is standing and the veins are full, if firm pressure be made on the cord, the vessels below, being thus relieved of the superincumbent weight of the blood, will gradually become emptied of their contents."

Notwithstanding this latter statement—for the truth of which in every case, if in any, we would not vouch—the plan of grasping the cord firmly between the fingers—pinching and rubbing the vessels between finger and thumb—is, in the hands of the experienced surgeon, perhaps the most certain means of diagnosis; not because in this way the tumour becomes more tense (which it may or may not, according to the amount of pressure exerted), but because educated fingers can best by this means distinguish the tortuous and dilated vessels of a varicocele from other tumours of these parts. The student may still be taught the ordinary plan, as given in books, provided he be made aware that the pressure must be *gentle* and not *firm*, as most authors have it; for, as has been shown, if the pressure exerted be great, the veins may remain empty and contracted. Gentle pressure in most cases will suffice to keep up a hernia, but not a varicocele: firm pressure in most cases will keep up both. I would recommend the following manner of stating this method of distinguishing between the two diseases. Place the patient recumbent, and raise the scrotum until the swelling disappear: let gentle pressure be now applied at the abdominal outlet, and the patient made to assume the erect posture, when, if a varicocele, the tumour will reappear, but not so if a hernia. The tumour will be seen to reappear from below if a varicocele, and, on all pressure being removed, from above if a hernia.

In concluding these remarks, we would repeat the great principle of treatment to be, the application of such an amount of pressure at the external ring as shall take off the weight of the column of blood from the distended veins below, and, in this manner, permit of their returning to their natural tone and dimensions. I am convinced that the chief element in this method of cure must be the relief afforded to the over-distended vessels by the removal of *superincumbent* pressure. That the superincumbent weight of the blood does act injuriously, and that its removal must act beneficially on a varix,

[1] On Diseases of the Testis, p. 469.

may be easily demonstrated. To illustrate the point, we prefer quoting a great authority to giving any statement or explanation of our own. The illustration was made on a patient who had an unusually large cluster of varicose veins on the inside of the leg, "while the *vena saphena major* was of enormous diameter. If I put on a bandage," says Sir Benjamin Brodie, "and squeezed the blood out of the veins below, and then put my thumb on the *vena saphena* above, so as to stop the circulation through it, I found, on taking off the bandage, the patient being in the erect posture, that the cluster of veins below filled very slowly, and only from the capillary vessels." But on removal of the pressure, the valves of the vein being useless, "the blood rushed downwards by its own weight, contrary to the course of the circulation, and filled the varicose cluster below almost instantaneously."[1]

How long it may be necessary to keep up pressure by a truss, to allow the veins to resume their wonted healthy condition, the case I have given does not show; for the truss, now in use ten months, has not yet been discontinued. From seven to nineteen months would appear to be the period necessary, according to Mr Curling's experience. Much will depend upon the age of the patient, as the same authority has pointed out in his work on the *Diseases of the Testis*, in which he states that the plan is particularly applicable in young persons, whose reparative powers are sufficient to restore the veins to their normal state. Cases occurring in old persons, will, for the most part, only be relieved, not cured. When there is a general lax habit of body, general treatment, as a means of increasing the reparative power, ought not to be neglected. Medicines calculated to give tone to the system, such as iron, quinine, salicin, &c., ought to be given in conjunction with nourishing diet and cold bathing. A cold water *lavement*, practised frequently, is also calculated to be of great service, by unloading, without weakening, the coats of the lower bowel, distension of which is well known to aggravate this complaint. Since directing my attention to this subject, it has been a question with me whether the *pressure principle* might not be applied to varicose veins of other parts. The other day I took advantage of a patient visiting me—upon whom I had operated some time ago according to M. Velpeau's process,[2] and upon whom the effects described by Sir Benjamin Brodie could readily be produced—to recommend a pad of cork, covered with chamois leather, to be applied in the same manner, but not so tightly, as a tourniquet, over at least two inches of the internal *saphena*, as it passes over the back part of the inner condyle of the femur. My object in applying pressure

[1] "Lectures on Pathology and Surgery." By Sir Benjamin Brodie, Bart. 1846. P. 186.

[2] I inserted four or five needles in this case, behind a large vein leading from an enormous varicose cluster. This cluster had nearly disappeared, but I saw that another of nearly the same size had formed further down the leg.

over two inches of the vein, is to avoid the effect of a common garter, which is generally believed to be injurious. A short period will show the result of this trial. On the evening of the same day that this man called, I received a letter from Mr Curling, mentioning the fact of relief having been obtained in this way of varicose veins in the upper part of the thigh. I was not aware, when I recommended the plan, that it had been ever tried; but it might easily suggest itself as a feasible plan to any one conversant with the results that have been obtained in varicocele. I cannot refrain from inserting here a portion of this letter, emanating, as it does, from a distinguished ornament of medical literature, as well as from one to whom the profession is indebted for first carrying out and giving publicity to this method of curing varicocele. After stating that he continues to entertain the same favourable opinion of the treatment that he had expressed in the *Medico-Chirurgical Transactions*, he goes on to say,— * * * "In no case of painful varicocele in which the patient has remained under observation, have I failed, sooner or later, in giving relief, by persevering in the treatment. As your experience teaches, success depends upon the proper adjustment of the truss, and a due amount of pressure. Some surgeons, not fully appreciating the principle of this treatment, have hesitated to employ it from apprehension of adding to the evil; but it is a curious circumstance, that, amongst the large number of cases of hernia in which a truss has been worn that have come under my notice, I cannot call to mind one complicated with varicocele."—(A most convincing proof of the efficacy of pressure, this statement, coming from one enjoying large opportunities of examining cases at dispensaries, hospitals, &c.)—"If the pressure operate injuriously on the spermatic veins, that might be expected to be a common complication. I may mention that, in two or three cases of large varicose veins at the upper part of the thigh, great relief has been obtained from the pressure of a truss. I am glad to learn that you intend making this plan of treating varicocele better known in the north, and I shall look forward with interest to a perusal of your observations on the subject in the excellent *Monthly Journal*.—I am, &c.

"T. B. CURLING."

Perhaps it may be thought by some that too much is made of the subject of varicocele. This may naturally be thought by the practitioner in private practice, who is seldom consulted on account of the disease. The conclusion very generally come to by such is, that this complaint is of unfrequent occurrence, and not worthy of much attention when it does happen. That the first opinion is erroneous we will immediately prove; and that the second is equally so, may be inferred from the circumstance, that varicocele is an important cause of rejection in both the naval and military services, as well as from the fact, that severe and dangerous operations have been long and frequently had recourse to for the removal of this disease; and that even the most distinguished surgeons (amongst whom Sir B.

Brodie) have thought themselves occasionally warranted to perform the operation of castration, to rid their patients of severe and intolerable suffering.

Through the kindness of my esteemed friend, Mr Marshall, deputy inspector-general of army hospitals, I have been put in possession of some valuable statistics on this subject, not yet published. The returns of Edinburgh and Glasgow, as given below, are, I believe, unique, in respect of their pointing out the relative frequency of varicocele on the right side as compared with the left.

Statistics of Recruiting in Great Britain and Ireland during the Year ending 31st March 1844.

Total number of recruits medically inspected	17,540
Ditto rejected	6,026
Found fit	11,514

Some of the causes of Rejection.	Total Rejections of each Class.
Hernia (inguinal, femoral, umbilical)	229=36·3 per 1000 rejected.
Varicose state of the veins of the *spermatic cord*	424=70·3 per 1000 rejected, or 24 per 1000 inspected.
Varicose state of the veins of the lower extremities	582=96·5 per 1000 rejected.

Statistics of Recruiting in Great Britain and Ireland during the Year ending 31st March 1845.

Number inspected	13,370
Ditto rejected	4,146
Approved	9,224

Some of the causes of Rejection.	Total Rejections of each Class.
Varicocele	264=63·6 per 1000 rejected, or 19·7 per 1000 inspected.
Hernia	227=54·7 per 1000 rejected.

Return of the Number of Recruits for the Army inspected at Edinburgh, from 25th March 1817 to 31st December 1822, divided into annual periods, wherein the Number deemed fit for the Service are distinguished from those considered to be unfit, with a Specification of the causes of Rejection.

661	Number inspected for	1817
614	„ „	1818
597	„ „	1819
927	„ „	1820
629	„ „	1821
1041	„ „	1822
4469	Total inspected.	

Rejected for varicocele on the right side	6
Ditto on the left side	132
Ditto on both sides	2
Total rejected	140

The following were inspected at Glasgow, from 1st January 1817 to 20th June 1823.

613	Number inspected for	1817
593	„ „	1818
805	„ „	1819
1,138	„ „	1820
788	„ „	1821
1,122	„ „	1822
696	„ „	1823
5,755	Total inspected.	
4,469	Total inspected at Edinburgh.	
10,224	Total inspected at both places.	
	Rejected for varicocele on the right side	3
	Ditto on the left side	89
	Ditto on both sides	6
	Total rejected	98
	Total rejected at Edinburgh	140
	Total rejected at both places	238

Of the 10,224 inspected at both places, 238 were rejected for varicocele = 23·2 per 1000 of inspected.

It will be seen from these statistics how large a proportion of recruits are rejected on account of varicocele. Moreover, it ought to be borne in mind that only those afflicted with this disease to such an extent as to interfere with their usefulness as soldiers would be rejected, and that, therefore, many slight cases of cirsocele may have been admitted. As a reason for submitting slight cases to the treatment I have explained, may be here stated the great tendency such have to increase.

The disproportion between the number of cases on the right and left sides is well seen in the Edinburgh and Glasgow returns. Mr Marshall says, "Cirsocele seldom occurs except on the left side. I do not recollect having ever seen a well-marked case of it on the right side, although I have examined nearly 30,000 recruits."—(Marshall *On the Enlisting, &c., of Soldiers.* 1839. P. 32.) The case I have given is interesting, in that it details a well-marked case on the right side. And the special claims of the above communication to notice, consist in its pointing out a source of fallacy in the ordinary mode of diagnosis between hernia and this affection, and in its giving the description of a more efficient truss than has yet been used for the cure of varicocele.

ARTICLE III.—*On the Occurrence of Carbonate of Lime in the form of Pearls as a Morbid Product in the Cow.* By THOMAS ANDERSON, M.D., F.R.S.E., Lecturer on Chemistry, Edinburgh.

THE number of the *Medical Gazette* for the 15th September contains a notice of the occurrence of pearls in the urinary bladder of

the bullock; and, as this is an extremely rare morbid product, I have thought it would be advisable to place on record another instance in which they have been observed, and which is in so far different that the gall-bladder was the source from which they were obtained.

The concretions which I examined were given me some years ago, and were marked, "Biliary Calculi from the Cow, from Antioquia, South America." Soon after obtaining them, I made an examination, which showed that they consisted entirely of carbonate of lime and animal matter, and possessed in every respect the structure of pearls; but I hesitated to publish it, as the calculi had already gone through several hands; and not having seen any observations of similar substances, I thought that objection might be taken to the evidence upon which they were assumed to be morbid products from the cow. Now, however, that difficulty is removed by the publication of Mr Taylor's notice.

The calculi were very numerous—I obtained, I should think, as many as sixty or seventy, and a number were also retained by the gentleman who gave them to me. They varied in size from about a tenth down to perhaps a fiftieth of an inch in diameter, and their form was perfectly spherical. They possessed a brilliant golden lustre, and had exactly the appearance of gold beads which had not undergone the process of burnishing. When introduced into nitric or hydrochloric acid, they rapidly dissolved with effervescence, and retain, until completely dissolved, their spherical form and golden lustre unimpaired. The solution is found to contain nothing but lime. A red heat indicates the presence of organic matter. I determined the quantity of organic matter in the usual way by ignition, and then converting the lime, which had become caustic, into carbonate, by digestion with carbonate of ammonia. In this way I found that 2·014 grains lost 0·270, which gives the composition:—

Carbonate of lime	86·6
Animal matter	13·4
	100·0

It will be observed that this is a much smaller amount of animal matter than that observed by Hatchet in the oyster pearl, in which he obtained no less than 34 per cent. It is obvious, however, that the concretions which I examined were true pearls, differing only from those of the oyster in possessing a golden lustre —a peculiarity dependent, not upon the presence of any colouring matter, for their solution in acids was perfectly colourless, but solely on their mechanical structure. The pearly lustre is well known to depend on the existence of a fine lamellar structure, and may be imitated by the superposition of a number of watch-glasses; and the modification in colour is dependent on the degree of thinness of the plates composing the concretion.

The occurrence of these calculi in the bile as well as in the urine is of some interest, as indicating their probable source. It is not probable that they should be formed in the urinary bladder from the true constituents of the urine; but the likelihood is that they must have a common origin both there and in the gall-bladder. Now Schmidt has recently endeavoured to show that oxalate of lime is secreted by the mucous follicles, and has established its existence in the mucous membranes of the gall and urinary bladders, and of the uterus. It is more than probable, therefore, that the pearls are formed in consequence of a morbid change in these follicles, whereby their oxalate is converted into carbonate of lime, If this be the case, it is possible that they may be sometimes found impacted in their follicles; and in any case in which they may be found loose in the bladder, it would be desirable to look for them there also.

I may observe, in conclusion, that true biliary calculi are very rare in the cow. Out of a considerable number of gall-bladders which I have opened, I have seen one instance only in which a thin tubular concretion occurred, lining the bile duct for about three-quarters of an inch, near the mouth of the gall-bladder. It consisted of cholepyrrhin.

ARTICLE IV.—*Note on a Camphor and Chloroform Mixture.* By T. and H. SMITH, Chemists, Edinburgh.

THE great difficulty, or rather the utter impossibility, of administering camphor in a state of solution in doses of sufficient potency, has been long felt by the profession to be a serious evil. The form of pill, which forces itself on the physician as the only mode of giving large doses of this medicine, is objectionable in many cases, and in others altogether inadmissible. The camphor, being merely in a state of mechanical division, on being set free in the stomach, from its extreme lightness quickly separates and floats about, thus producing in many cases much local irritation in that organ, instead of soothing or arousing the general system.

We have, therefore, much pleasure in laying before the readers of the *Monthly Journal* a formula for exhibiting camphor in doses of almost any amount of strength—certainly as large as any case can require—and that in a state of perfect solution: thereby allowing of a nice adaptation of the dose to the circumstances of each case.

The formula is as follows:—Three drachms of solid camphor are dissolved in one fluid drachm of chloroform. This is, perhaps, one of the most remarkable cases of solution the whole range of chemistry presents to us. The solution is most *rapid and complete*, and the bulk of the liquid is now increased from one to fully four fluid drachms. This solution, rubbed up with the *yelk* of one fresh egg, may be formed into an extremely elegant emulsion by the addition of water, without the slightest separation of the camphor

or chloroform; in fact, no separation of any kind takes place. If to the proportions given above as much water be added as to make a four-ounce mixture, each teaspoonful of the mixture when formed will contain about five and a half grains of camphor, and about two minims of chloroform. The capability of the formula being varied, so that either the camphor or chloroform may constitute the predominating ingredient, must be quite obvious. This mixture can be administered in any ordinary vehicle, such as water, without the occurrence of any separation; indeed, the mixture is as readily and completely effected as cream with tea or coffee. We have tried the effect of several medicinal substances on the mixture. With none of them has any separation been caused.

A weak saline solution, composed of common salt, phosphate of soda, and an alkaline carbonate, mixed readily, as well as a solution of muriate of morphia and sulphate of zinc. With the volatile alkali and acid liquids—such as a weak solution of acetic and muriatic acids—the mixture seems to become more intimate and stable. The mixture with ammonia has stood since its preparation—now fully a week—without any separation. With water alone, however, the chloroform solution of camphor separates in a few days, but they readily unite again when slightly agitated. The solution of camphor in chloroform, although insoluble in water alone, appears in this mixture to be in as complete a state of mixture as the butter in milk when newly drawn from the cow.

It now remains with the physician to ascertain the therapeutic value of the formula. We hope that by its means our knowledge of the action of camphor as a remedial agent may become more full and precise than hitherto.

ARTICLE V.—*Observations on Gunshot Wounds, made in Paris during the Summer of* 1848. By EDWARD WATERS, M.D., F.R.C.P., Edinburgh, formerly Senior President of the Royal Medical Society of Edinburgh.—(Continued from p. 264.)

TREATMENT OF FRACTURES OF THE EXTREMITIES IN CONNEXION WITH GUNSHOT WOUNDS.

b.—Treatment and Apparatus.

DR KIDD of Limerick, in a communication to the *Medical Times*,[1] alluding to the treatment of the wounded in the Parisian hospitals, writes—"The surgery in France is simpler and better, I think, than in our own country: this is what first strikes you going round." With whatever justice this remark may apply to the hospitals he refers to, its groundlessness with respect to the practice of the Edinburgh school is undeniable. Ever since the advent of Syme and

[1] Medical Times, August 26, 1848.

Liston, the utmost simplicity, compatible with efficiency, has characterized the treatment there pursued with respect to fractures; and the application of simple dressings to wounds in conjunction with them, where such exist, in a manner which involves no disturbance of the limb on their being changed, has been uniformly enforced. In the Parisian hospitals this is far from being in the majority of instances the case. In the wards of Roux, Blandin, Boyer, Laugier, Giraldès, and others, the practice which has so long distinguished the French surgeons, continues to be rigidly followed. A piece of greased linen is first applied to the wounds, and over this some loose charpie; then the "*linge troué*," which is also covered by more charpie with the view of absorbing the pus; compresses follow, and, after them, the many-tailed bandage, enveloping the whole limb; two lateral splints are next rolled up in the borders of a sheet passed under the fractured extremity (*le porte attelles*), until they come into apposition with its sides, leaving only sufficient space for the interposition of cushions of corresponding length and breadth, and composed of bags stuffed with oaten chaff; a third splint and cushion are applied on the anterior surface of the limb: this apparatus is firmly bound together by separate bands of tape, applied and fastened at intervals. In some services, a piece of oil-skin is interposed, to obviate the soiling of the bed and the more permanent portion of the bandages by the discharges. In some wards, this mode of dressing has been applied, not only to fractures of the thigh, but also to those of the leg, fore-arm, and arm; in others, the double inclined plane has been used in fractures of the leg; in those of Giraldès, *gutta percha* has been adopted in several cases with advantage, particularly for fractures of the superior extremity; this substance being softened in warm water and then adapted to the limb, and apertures being practised, corresponding to the wounds, by which a free exit for the products of suppuration is afforded. In fractures of the thigh, the apparatus first described has been used in the service of all the surgeons mentioned, though the loaded state of the limb, the enormous proportions which the dressings, when accomplished, presented, seemed but ill calculated for patients in crowded wards during the heat of summer. Each time the dressings were changed, the raising of the limb became necessary for their removal and renewal, constituting a daily source of disturbance, generally occupying at least half an hour, and attended with much suffering to the patients. The accumulation of the discharges, confined to the neighbourhood of the wound by the multiplicity of the applications, generated no trifling amount of effluvia, and apparently offered the most favourable condition for the development of purulent infection, the most frequent cause of mortality in the Parisian hospitals. No extension was practised; the length of the external splint corresponded to that of the limb, and it was not a solitary occurrence to find the limb in one direction and the body of the patient in another, when the time for dressing arrived.

The practice of M. Jobert, in these fractures, is, on the other hand, remarkable for extreme simplicity; extension by pelvic and foot-bands, and the adaptation of pillows, comprising the whole of the apparatus, and permitting the application of dressings to the wounds with the greatest rapidity, and without in the slightest degree disturbing the patient; splints are rarely employed by M. Jobert, reliance being almost entirely placed on position.

In the earlier periods of treatment, M. Malgaigne followed the same practice, with the addition of a splint in the neighbourhood of the fracture. In a case where the shaft of the femur was fractured near its neck, and where union was effected through the medium of the provisional callus, with shortening to the extent of nearly an inch, a case admitted into his service from one of the temporary hospitals on the 11th of July, he adapted the long splint of Desault to the limb, in order to keep up constant extension; the fracture had been previously treated by the double inclined plane.

M. Baudens had recourse to his fracture box in fractures of the leg. This box is open superiorly, and at the side where the limb enters; its two lateral and distal sides are fixed to the bottom or base of the box by hinges; these sides are perforated by two ranges of holes at fixed distances. The fractured limb was fixed on cushions laid in the bottom of the box, and extension maintained, and the position of the fragments regulated by means of bandages passing through the holes in the sides, and then secured. In this apparatus, the condition of the limb was always open to inspection, and the patient could change his position without risk of deranging the fractures; it also afforded every facility for changing the dressings and for the removal of the discharges.

In some instances of fracture of the superior extremities, the position of the member and the apposition of the fragments was maintained by the "*gouttières en fil de fer*" of M. Mayor,—incomplete cylinders formed of iron wire, tinned to prevent oxidation, and embracing about two-thirds of the circumference of the limb; the meshes formed by the wire are large, and permit the removal of the charpie or wadding with which they are padded, as occasion may require. They possess every necessary variety of form, and constitute a firm and light apparatus.

With the exception of the first, each of these various methods of treatment presents advantages which may render its adoption advisable in certain cases. The first is, however, that in most general use; although the cumbersome nature of the apparatus, the confinement of the discharges to the neighbourhood of the wound, and the disturbance of the limb at each successive dressing, are objections which suffice, in the minds of those who are unfettered by the prejudices resulting from a too exclusive course of education, to banish its employment in all cases of compound fracture, and especially in cases resulting from gunshot wounds, which, sooner or later, are always attended by profuse suppuration.

That the long splint of Desault, so admirably adapted for fractures of the thigh, should be comparatively neglected in the country where it originated, while a conviction of its utility is extending itself elsewhere, is a circumstance not unworthy of observation. M. Nélaton is one of those to whose practice this observation does not apply; his mode of putting up fractures of the thigh, where he yielded to the hope of preserving the limb, is shown in the following case:—

A printer, aged nineteen, an insurgent, was admitted into the Hôpital St Antoine, the 24th of June, with a comminuted fracture of the femur, occupying the inferior portion of the shaft; the splinters were neither numerous nor large, and their extraction was effected without dilatation of the wound; the ball had passed out. The long splint of Desault, properly padded, was applied on the outside of the fractured extremity, a corresponding splint on the inside, and a padded foot-piece was fixed between them, to which the foot was firmly attached. The wounds were simply dressed, without compresses or the many-tailed bandage. The limb was perfectly straight, and visible throughout its whole length.

Considerable discussion has taken place as to the applications best adapted to keep within proper limits the inflammation following gunshot wounds. M. Blandin, when stating his preference of cold poultices and cold water to ice, observed, that ice should not be applied to a limb affected with that sort of local stupor which all surgeons are acquainted with, and for this reason considers it contra-indicated. M. Roux delivered his opinion to the Academy as follows:—"I do not believe that great advantage is to be derived from the employment of irrigation, or from the application of cold water or ice. The inflammation cannot be prevented; and, even were it possible, care should be taken to avoid such a result. Our efforts should be solely directed to moderate it."

The prophylactic properties of cold, as an agent to ward off inflammation, have been highly lauded by many surgeons; the danger of gangrene from the use of ice, when inflammation and tension are present, has also been set forward. Amongst British surgeons, the superiority of cold water dressings, in the early treatment of gunshot wounds, has been long admitted; and, with few exceptions, cold, in one form or another, may in like manner be said to have been the favourite application here. M. Baudens relies exclusively on ice for limiting the local action, and esteems it the most powerful of all agents for combating traumatic lesions. He applied it, in every case of gunshot wound, as soon as the resulting shock had subsided; and, at this period, without consulting the feelings of the patient. In one or two rare instances only, its first application produced a disagreeable sensation; this, however, speedily subsided, and gave place to an ineffable feeling of ease and well-being, which could only be fully appreciated by being witnessed. This was almost the uniform condition of his patients; and even in those afflicted with wounds of the lungs and of the liver, the relief afforded by its local application, however questionable its utility, seemed to be con-

siderable.[1] It was employed in the following manner:—A piece of linen, spread with cerate, was first applied to the wound, over which a somewhat thick layer of loose *charpie* was placed; this was covered by a single fold of linen, the corners of which were knotted together, so as to maintain in position the two, three, four or more lumps of ice which were laid on it. The pieces of ice varied in size from that of a walnut to a small hen's egg; they were not applied directly to the surface; they melted very gradually, and, when exhausted, the patients renewed them themselves; the lint was thus kept continually moist. Applied in this manner, the result was invariably one of great comfort to the patients, and a look of suffering was, in exceptional cases only, visible throughout the wards. The absence of all disagreeable odour was equally striking—a circumstance in great measure attributable to the facility with which the change of the dressings was managed; the wards were equally crowded with those of the other hospitals, and any thing approaching "*pourriture d'hôpital* was rarely witnessed. The mode of application of the remedy would appear to modify the effects; at least in a case in the service of M. Velpeau, where a bladder, filled with pounded ice, was suspended so as to come in direct contact with the wound, the result was far from being equally satisfactory. It was a wound of the thigh, and the patient seemed to suffer considerably.

So long as the local temperature continued exaggerated, so long did M. Baudens continue the ice; on applying the hand to the part after its use for some time, the surface generally conveyed a sensation of coolness, rapidly replaced, however, by one of deep-seated and pungent heat, where the persistance in its employment continued to be indicated; no sooner was it attended with a feeling of discomfort, than it was exchanged for a moist compress, which was also discontinued after one or two days. The remaining treatment presented no features of interest.

Irrigation with cold water, after the manner of M. A. Bérard, has been partially adopted in nearly all the hospitals. The wounded limb was laid on oil-cloth, so disposed as to convey the unevaporated water into a bucket at the bed-side; the wound was covered, as in the latter instance, with a piece of linen spread with cerate, and over this a considerable quantity of loose charpie. The irrigation was kept up by means of a flexible syphon; one extremity of the syphon was placed in a can of water, suspended at the head of the bed, while the other was placed in contact with the charpie covering the injured part, a small piece of linen only intervening, which was attached to this extremity; a cork, fixed near this end of the tube,

[1] In the service of M. Jobert more than one case, where the lung was wounded and where formidable inflammation was lighted up, was successfully treated without local applications, by copious blood-lettings according to the formula "*coup sur coup.*"

regulated the dropping of the water. Its use was restricted to wounds which, in the inferior extremity, did not extend above the knee, and, in the superior, above the elbow. In every case its employment was marked by distinct amelioration of the suffering. M. Jobert had recourse to cold applications, but, in the place of ice or water, preferred cold linseed meal poultices, enveloped in linen; these were changed from six to eight times in the twenty-four hours. M. Malgaigne pursued the same practice, and also M. Velpeau; but with the difference that he applied them to the surface without the intervention of a fold of linen, believing that they thus adapted themselves better to the parts, and that the somewhat oily nature of the poultice was not unproductive of benefit. M. Laugier also resorted to cold applications previous to the establishment of the suppuration; in one or two instances he made use of the continuous irrigation; more frequently he merely placed a thick compress, wrung out of cold water, between the superior layer of charpie and the many-tailed bandage; the ordinary dressings, already described, were then proceeded with as usual, without the interposition of any substance to retain the moisture, and the wound left till the period of performing the next dressing arrived—certainly a very inadequate attempt to fulfil the indication sought to be realized.

Further multiplication of these details would serve no purpose; sufficient have been adduced to show that the weight of authority is in favour of cold applications. The objection of M. Roux has no just foundation: the action necessary for the reparation of the injury is in no degree counteracted by ice or cold irrigations, the effusion of plastic lymph is in no way impeded, the pus formed is of laudable quality, and the granulations present the most healthy characters under the use of either. With regard to the employment of the cold irrigation, the testimony of M. Nélaton may not inappropriately be cited. He states—"In the service of MM. Breschet and Sanson, I have seen these irrigations continued during ten, fifteen, twenty days, a month even, for the most grave wounds of the articulations, for the most complicated fractures. In many cases I have seen them succeed perfectly; and I have never seen any serious accidents arise which could be imputed to them."[1] M. Blandin prefers cold poultices and cold water to ice, but it is to be observed that he has made no use of the last remedy; he dreads the effects of its application during the period of local stupor, but this period is one of very short duration; at all events, reasoning must give place to facts, and the opponents of this practice of M. Baudens have not been able to adduce a single unfavourable occurrence in support of their strictures.

Ice applied in moderate quantity, according to the method of M.

[1] Pathologie Chirurgicale, tom. i. p. 189; Paris, 1844. He adds:—"MM. Breschet, A. Bérard, Pinel Grandchamp, have substituted tepid water for cold in these irrigations; the results obtained have been generally satisfactory."

Baudens, and not loading the part, as when a bladder filled with it is used, appears to possess some advantages over cold irrigation; for instance, it is applicable to parts, such as the thigh and humerus, where cold irrigation is considered inadmissible even by its originator, and a painful sensation, lasting occasionally twenty-four hours, which generally attends the commencement of the use of the latter, is much less frequent and less durable when ice is employed. The danger of gangrene from its employment when inflammation is established, is in great measure illusory—it has been successfully used under such circumstances; with it, as with irrigation, the heat, pain, and engorgement diminish rapidly, and finally disappear. All the objections as yet urged against its use vanish on examination; and, when the benefit which has been derived from it is considered, more particularly the relief from suffering which it has conferred, a perseverance in its employment appears not only justifiable, but may be held to offer great advantages. An important phenomenon, resulting from the local application of ice in the hands of M. Baudens, was a marked sedative influence on the circulation: from being very rapid, the pulsations in a short time were frequently observed to descend to seventy and sixty in the minute. In one instance, when the symptomatic fever had run high, after the use of the ice for some days the pulse was only forty-eight, soft and regular, all suffering was allayed, the tongue was moist, and the general surface of a comfortable and agreeable temperature.

In estimating the value of the remedy, it should perhaps be borne in mind, that the last week of June and the month of July was the period when its action was observed, and also that the experience of the surgeon who more particularly extols its effects has been chiefly acquired in Algeria.

The *Erysipelas*, which showed itself in conjunction with these wounds, was combated by various means; by M. Baudens, by the exercise of superficial scarifications and the application of ice; by M. Velpeau, chiefly by smearing the affected parts with mercurial ointment; by M. Nélaton, by washing the parts with a solution of sulphate of iron (about an ounce to a quart of water); by others, by strong lotions of nitrate of silver or by opiate fomentations. With all these diverse remedies, the success was pretty equal; no decided superiority could be said to mark the action of the one over that of the others.

Hospital Gangrene gave rise to the usual varieties of treatment, but without presenting any particular points of interest. Lemon juice, which Dupuytren was in the habit of resorting to, was often employed to modify the character of the parts attacked. In the early stage, lint saturated with lemon juice was the mode of application with some, as MM. Nélaton and Giraldès. M. Roux and others completely covered the surface of the wound with slices of lemon as a dressing. In both ways it appeared to exert generally a beneficial

action. To destroy the sloughs, M. Jobert employed the acid nitrate of mercury.

Purulent Infection.—In two cases which presented the general symptoms of purulent infection, repeated rigors, &c., without any assignable local cause, M. Nélaton administered the tincture of aconite, and the patients recovered; others similarly attacked died under its employment.

The remedy was continued during several days, more than a week, commencing with one gramme the first day, and augmenting the dose gradually to three grammes.[1] M. Nélaton attributed the recoveries to the aconite; none of the theories, however, which prevail respecting the etiology and nature of the malady, suffice to explain its *modus operandi* in these cases. A tincture possessing the activity of that directed by Dr Fleming in his admirable monograph on aconite,[2] could not be given in the manner stated without incurring serious danger; none of the physiological effects there described followed its administration, and further experience is required to confirm its possession of any influence in combating the tendency to a fatal termination in purulent infection.—(*To be concluded in next Number.*)

Part Second.

REVIEWS.

1. *An Act for Promoting the Public Health.* 31st August 1848.
2. *An Act to renew and amend an Act of the Tenth Year of her present Majesty for the more speedy Removal of certain Nuisances, and the Prevention of Contagious and Epidemic Diseases.* 4th September 1848.
3. *First and Second Reports of the Health of Towns Commission.* London, 1844-5.
4. *First, Second, and Third Reports of the Metropolitan Sanitary Commission, with Evidence.* London, 1848.

The important movement for improving the health and physical condition of the poor, which has now for several years engaged the sympathies of the British public, and which has just achieved its first great victory in the enactment of the measure for the improvement of the public health, of which we elsewhere insert an abstract,

[1] From twenty to thirty drops of a tincture, according to its specific gravity, are equivalent to one gramme.

[2] On the Physiological and Medicinal Properties of Aconite, by Alexander Fleming, M.D. 8vo. London, 1846.

is to so great an extent the offspring of medical philanthropy, and is so largely indebted for its ultimate success to the patient and self-denying exertions of members of the medical profession, that we offer no apology to our readers for occupying their attention with a few additional remarks upon this interesting subject. We are, moreover, strongly impressed with the opinion, that this public recognition of the science of preventive medicine is an event fraught with the most auspicious consequences to the practitioners of the medical art, and one which will, at no distant period, lead to that social elevation of the profession and improvement of medical science, which every right-minded man has so long desiderated, but of the attainment of which many had begun to despair.

Before entering upon an examination of the provisions of the Sanitary Acts passed during the last session of Parliament, it may be useful to present a very brief sketch of the sanitary movement as it has proceeded in this country.

On the appearance of any disease of extraordinary malignity, it has, in this as in other countries, been usual for those inhabitants distinguished for their wealth, influence, or knowledge, to meet together and consult for the common safety. And the temporary committees thus spontaneously formed have, with a few trifling exceptions, hitherto constituted the sole guardians of the public health.

With such an utter absence of sanitary supervision, it will not appear extraordinary that the metropolis and provincial towns, to which the agricultural poor have for many years been attracted in large numbers by the high wages offered in manufactories, should exhibit the most fearful scenes of misery, profligacy, and disease. For the details of these horrors we must refer to the evidence appended to the reports of the Health of Towns Commission, and to other similar sources of information; suffice it here to state, that from the neglect of the authorities, and the short-sighted selfishness of the proprietors of tenemented houses, filth of the most disgusting and noxious nature has been allowed to accumulate in enormous masses, in and near the dwellings of the poor; that no provisions have been made for the removal of refuse, or for the supply of water; that from the excessive crowding of adults of both sexes in small unwholesome lodging rooms, the preservation of decency has been rendered as impracticable as indulgence in habits of cleanliness; and that shamelessness and demoralization have advanced with giant strides among the unfortunate beings thus exposed from their very childhood to the most deadening and polluting influences. Whilst typhus has in these regions of wretchedness become an indigenous and constant scourge, all other diseases of the zymotic class have found there a hot-bed for their propagation; and, from the operation of these various sources of moral and physical disorder upon the industrious poor inhabiting our large towns, the duration of life has been gradually shortened, until at Liverpool the

average age at death of the labouring class did not exceed fifteen years, while the gentry and professional persons attained on an average thirty-five years.

Now the connexion between excessive mortality and the neglect of certain natural essentials to health, such as pure air, water, &c., is no new discovery. To say nothing of the ample treatises on these subjects in the old medical classics, we find in the works of our own countrymen frequent reference made to the importance of diminishing the mortality among the poor by greater attention to their physical comforts. And some time after the benevolent exertions of Howard had been crowned with success, and Cook and Lind had demonstrated the practicability of preventing scurvy, and other diseases of seamen, by ventilation and a judicious choice of food, a society for improving the condition of the poor was formed in London, under the auspices of Wilberforce, Davy, and other persons equally eminent for philanthropy and science. These isolated efforts were not, however, productive of much permanent benefit, and it was not until the advent of cholera in 1831, that any general steps were taken for removing the Augean accumulations from the courts and bye-streets of our large towns. The instinct of self-preservation operating in the minds of the wealthier classes, based upon the fear that the terrible epidemic, once attracted by the fetid squalor of crowded lanes, might subsequently extend its visitation to more spacious and refined residences, produced at that time an unwonted zeal for purifying the habitations of the poor. But this zeal was from its very nature destined to expire with the occasion which induced its manifestation; and typhus, and other diseases which had for the moment been threatened with extinction, rapidly resumed their sway over their favourite localities. With the disappearance of the cholera, boards of health vanished, the regular inspection of the houses of the poor was discontinued; the government ceased to interest itself in the health of the community, local authorities followed the example of the central administration; poor-rates, often the consequence of unnecessary widowhood and orphanage, annually increased, and the tax upon the benevolent public for the support of medical and other charities, already excessive, promised soon to be wholly inadequate to the increasing demand upon it for the maintenance and relief of the diseased and destitute.

For the interests of the poor, and, as time will ultimately show, for the interests of the wealthy and rate-paying classes themselves, it is fortunate that the revelations made during the existence of the last cholera panic were not wholly unproductive. The office of chief secretary to the English Poor-law Commission happened to be filled by a gentleman, who, to great natural ability, and an extensive official acquaintance with the actual condition of the poor, added a considerable knowledge of the principles of medical science, and an earnest conviction of the importance of

applying those principles to the prevention of disease, on a grander scale than had hitherto been attempted. Having obtained the requisite authority from government, and being ably seconded by some distinguished members of the medical profession, among whom may be mentioned Drs Southwood Smith, Arnott, and Kay, Mr Chadwick issued a series of questions to medical practitioners and other competent persons, resident in various parts of Great Britain, and the answers to these enquiries were subsequently embodied in the first "Report of an Inquiry into the Sanitary Condition of the Labouring Population of Great Britain," published by the Poor-law Commissioners in 1842. Almost simultaneously with the preparation of this report, Mr Chadwick instituted "a special inquiry into the practice of burials in towns," which in the following year appeared as a supplementary report, and which, both as regards the demonstration of the evils suspected to exist, and the suggestion of practicable measures for their prevention, completely exhausts the subject. These reports, and the startling facts which they revealed, led to the issuing of a commission, composed of various noblemen and gentlemen, selected without reference to party, who in 1844–5 presented reports comprising a mass of evidence on the actual state of the dwellings of the poor in the large towns, and embodying the results of the commissioners' personal enquiries and observations in a series of recommendations for the improvement of the law in various particulars affecting the health of populous districts. And it is upon these recommendations of the Health of Towns Commission that all the subsequent efforts of various parliamentary leaders to obtain the enactment of a general sanitary measure have been founded. The bill which has recently received the sanction of the legislature, and which is but a very imperfect realization of the wishes of sanitary reformers, is the *fifth* project that has been submitted in recognition of the necessity for accomplishing this long neglected act of national duty. The Marquis of Normanby twice induced the House of Lords to assent to the provisions of a bill on this subject, but it was each time rejected by the Commons. The Earl of Lincoln endeavoured to meet the difficulty by originating the measure in the Lower House; but, in consequence of Sir Robert Peel's resignation of office, was prevented from conducting his bill to a successful issue. And Lord Morpeth, to whom, under the Whig administration, this branch of legislation was committed, after carrying a bill through its second reading in the session of 1846–7, was prevented by the dissolution of Parliament from seeing it passed into law.

Having thus roughly sketched the chief incidents in the progress of sanitary reform, we shall now endeavour to indicate those features of the new sanitary laws which appear to us most worthy of approbation; while we shall, at the same time, not shrink from pointing out the omissions and defects which will, it is to be feared, render the more important of these acts in many places almost wholly

inoperative. And, in the execution of this task, we shall adopt as a standard the recommendations contained in the Second Report of the Health of Towns Commission. The first and most important provision of the Public Health Act 1848, is the creation of a General Board of Health, at the head of which is placed a cabinet minister, for the superintendence of all matters affecting the sanitary condition of the people. This appointment is not only an immediate advantage to the community at a season like the present, when a fearful epidemic is daily expected, but it also constitutes, in the heart of the kingdom, a nucleus from which sanitary regulations, and the organization of a medical police, may ultimately extend throughout the whole country. And in so far carrying out one of the chief recommendations of the Health of Towns Commission, the government is entitled to the warm commendation of the friends of the sanitary cause. But here the defects of the measure become at once obvious. In the first place, the Act contains no provision for the appointment of a medical member of the General Board; and although this slight upon the profession, and fundamental error in the construction of the central administrative body, has since been partially and indirectly remedied by a clause subsequently inserted, *sub silentio*, in the Nuisances Removal Act 1848, under which Dr Southwood Smith has been temporarily appointed as medical member of the General Board, the omission is not the less reprehensible. Then, with regard to the most essential duty of a central board, viz., the superintendence of the local administrative bodies in the execution of the powers conferred upon the latter by this Act, the measure before us is singularly barren; for the only clause which imparts to the General Board of Health a virtual check upon any reckless or ill-judged expenditure of the local bodies is the 119th, which forbids "the Local Board of Health to borrow or take up at interest any sum or sums of money upon the credit of any rates authorized to be made or collected under this Act, without the previous consent of the General Board of Health." Now, we are no supporters of the principle of centralization; we are, on the contrary, among the number of those who attach great importance to the preservation of municipal and other local privileges, and to the administration of the laws, as far as practicable, by persons who have by long residence become intimately acquainted with the wants of each district, and the wishes, the peculiarities, and it may be the prejudices, of its inhabitants. But if a mass of social evils, admitted by all parties to exist, be by different administrations deemed worthy of the attention of Parliament; if the Queen, in successive speeches from the throne, recommend measures for the removal of those evils to the serious attention of the Legislature; and the latter, after examining witnesses by a select committee of its members, and receiving the reports and recommendations of eminent men specially deputed to enquire into this subject, manifest their

wish to carry into effect the suggestions of her Majesty, supporting her ministers by large majorities in every division that has recently taken place on the question,—then it certainly does not appear too much to require, that the persons entrusted with the local administration of the act should faithfully assist in carrying out the provisions of a law deemed essential to the general interests of the state; and that, in the event of their neglecting the duty which they have undertaken, some arrangement be made for its performance by more efficient parties. For these reasons we shall ever consider our sanitary legislation imperfect, until the Central Board of Health be invested with a discretionary power to compel the local bodies to exercise, for the benefit of the public, the authority which the law has deputed to them for that purpose. Under the present Public Health Act, it is evident that the Local Boards, when constituted, may do almost as much or as little as they please. They are compelled to meet once in each month; they are to "cause the sewers, vested in them by this Act, to be constructed, covered, and kept so as to be not a nuisance or injurious to health, and to be properly cleared, cleansed, and emptied;" lodging-houses and slaughter-houses are to be registered; no new houses are to be built without drains and other conveniences; no new streets shall be laid out without the levels having been previously fixed by the Local Board. Offensive trades, newly established, shall be subject to the regulations of the Local Board; offensive drains, &c., shall be cleansed or covered.

These constitute the chief compulsory clauses of the Public Health Act; the permissive powers are much more extensive; but before noticing them, we may perhaps be allowed to say a word on *permissive* legislation generally. The respect and obedience with which the inhabitants of this country are wont to regard the laws have a twofold origin; they arise in part from a belief that no laws are made but such as are required for the public good, and in part also, and in no slight degree, from a conviction of the impossibility of individuals resisting, evading, or treating with indifference the enactments of the legislature. Of late years, however, Acts of Parliament have lost much of their ancient *prestige;* instead of clear, concise, positive *laws*, the parliamentary printers have annually issued a series of bulky documents, with copious instructions for the use of some hypothetical public body, which may hereafter be called into existence, and the members of which "may, if they think fit," adopt the suggestions of the legislative Mentor. And perhaps no cause has received more injury from this sham legislation, or been more effectually retarded by it, than that of sanitary reform. For proof of this, it might suffice to refer to the various Town Improvement Acts, most of which contain numerous sanitary provisions capable of being enforced so as to prevent many of the evils above alluded to. But an act passed in the session of 1845-6, entitled, "The

Towns Improvement Clauses Act," and which we believe is still in force, constitutes even a more striking illustration of the evil to which we refer. In it the most ample provisions are made for the sanitary regulations of any town within which it may be applied. It is in many respects more complete than the Public Health Act of 1848; but as its introduction into any town requires another Act of Parliament, it is scarcely necessary to add, that it has not yet, so far as we know, been in a single instance rendered available for the public use. Our experience of the inefficiency of "permissive" legislation, therefore, renders us very doubtful whether the majority of the powers vested in the Local Boards of Health will, in the present state of the law, be exercised by them. And as the limited application of the Public Heath Act will compel us to enumerate many of those provisions among the other *desiderata* of a satisfactory sanitary measure, we beg to refer to the summary of the Act at p. 338, for a full account of the benefits which the Local Boards may confer upon the inhabitants of the towns which may be brought within the operation of this law.

Notwithstanding the defects which we have attempted to point out in the preceding remarks, we consider this measure as a most important instalment of the great boon of sanitary reform. As such it would command our deepest gratitude, were it not that we in Scotland, as well as our neighbours in Ireland, to say nothing of the English metropolis itself, are excluded from its provisions. We have expressed, in a previous number of this Journal, our regret that so limited a measure should have been introduced, more especially as we can neither suppose that any special sanitary condition of this portion of the country, nor any special obstruction on the part of its local authorities, presented adequate reasons for withholding a measure of so much importance. So great a defect would be felt as a peculiar hardship at present, when a disastrous epidemic has begun to select its victims from among us, were it not that, in the supplementary act, to which we shall presently refer, we have a remedy against this temporary evil. We sincerely hope that by this act, together with the improved police regulations of our two principal cities, we may be, while the scourge lasts, placed in a more favourable position than we had expected up to a recent period; and we have every confidence that our local authorities will show by their acts, that apathy on their part will not stand in the way of any future legislative provition for the sanitary condition of Scotland.

We have endeavoured to indicate the points in which the Sanitary Act of 1848 is defective in principle, and likely to be inoperative in practice. This duty we feel called upon to discharge in consideration of the interests of the public, but certainly in no spirit of unfriendly criticism, being sensible that in this country no legislative measure can be carried, amidst such variety of interests as exists in the case of this reform, without a weight of concur-

rent popular opinion, which, we regret to say, has not been manifested in the present instance. It is to the public opinion, and especially to the enlightened *medical* opinion, of this country that we appeal to place this question in a proper point of view before the Legislature, and, by the suppression of local interests and prejudices which are at variance with the public weal, to pave the way for a more complete measure of sanitary reform. In Scotland, in particular, we have no time to lose; the temporary act should be put in operation to its utmost limits; and every exertion should be employed, when the urgent necessity for its application is over, to induce the Legislature to replace it by a permanent measure of reform of equal or greater efficiency.

The Nuisances Removal and Diseases Prevention Act of 1848, which received the royal assent a few days after the Public Health Act, though allowed to pass without criticism as a mere renewal of a former act which would otherwise have expired, is a much more efficient measure than that which we have just been considering. As before observed, it corrects one important omission in the Public Health Act, by authorizing the appointment of a medical member of the General Board of Health during the continuance of the Order in Council authorizing the operation of this act. As the latter applies to the whole of Great Britain and to Ireland, and the order of the Privy Council putting in force its provisions has already been issued, it may be useful to present a brief outline of its more important provisions. The order of the Privy Council, applying the act to the whole or any part of the united kingdom, continues in force for six months only, but may be renewed. From time to time after the issuing of such order, and whilst the same shall continue in force, the General Board of Health in Great Britain, and the Commissioners of Health in Ireland, may issue such directions and regulations as they may think fit for the prevention or mitigation of epidemic, endemic, or contagious diseases; and may by such directions and regulations provide for the frequent and effectual cleansing of streets and public ways and places by the surveyors, district or assistant surveyors of highways, trustees, county surveyors, and others by law entrusted with the care and management thereof, or by the owners and occupiers of houses and tenements adjoining thereto; and for the cleansing, purifying, ventilating, and disinfecting of houses, dwellings, churches, buildings, and places of assembly, by the owners or occupiers and persons having the care and ordering thereof, for the removal of nuisances, for the speedy interment of the dead, and generally for preventing or mitigating such epidemic, endemic, or contagious diseases, in such manner as to the said Board or Commissioners may seem expedient; and the said Board or Commissioners may authorize and require the guardians of the poor in England and Ireland, and the parochial boards for the management of the poor in Scotland, by themselves, or their officers, or any per-

sons employed by them in the administration of the laws for the relief of the poor, or by officers specially appointed in their behalf, to superintend and see to the execution of any such directions and regulations, and, where necessary, to provide for the dispensing of medicines and medical relief of persons afflicted by or threatened with such diseases. The Poor-law Commissioners in England and Ireland, and the Board of Supervision established under the act for the amendment and better administration of the laws relating to the relief of the poor in Scotland, may require the officers and persons acting under them to execute the directions of the General Board of Health (and Commissioners of Health in Ireland), and shall have the same powers for enforcing the execution of such directions as they now or may hereafter have in relation to any matter concerning the administration of the law for the relief of the poor. *The guardians and parochial boards may appoint or employ, for the superintendence and execution of such directions, officers or persons in aid of the officers or persons employed in the administration of the laws for the relief of the poor;* and such guardians and parochial boards respectively, shall defray the expenses incurred by them respectively in the superintendence and execution of such directions and regulations, out of the funds of their respective unions, parishes, or combinations. Persons obstructing the execution of this act, or wilfully violating the directions or regulations of the General Board of Health, are liable to penalties. The act is not to apply to districts or places where the Public Health Act is in force.

The above are the chief general provisions of this act, and a very brief comparison of them with those of the Public Health Act will probably induce the reader to agree with us, in pronouncing the smaller the better act of the two. This short and simple Nuisances Removal Act, in fact, invests the Board of Guardians and Parochial Boards with most of the powers conferred on the Local Boards of Health by the Public Health Act, while it possesses the very great advantage over the latter of compelling the local administrative bodies to carry its chief provisions into effect. How far the town-councils, whose power as conservators of the public health is virtually superseded by this conversion of the Boards of Guardians into Local Boards of Health, will deem it necessary to exert themselves for the recovery of the public confidence, remains to be seen. We are not without some hope that the steps now being taken by the General Board of Health, through the machinery of the poor-law, may stimulate the municipal bodies to avail themselves without further loss of time of the powers held out for their acceptance in the Public Health Act. In addition to its more general enactments for the prevention of epidemic and other diseases, the Nuisances Removal Act enables any two inhabitant householders to cause the removal of any accumulation of filth, or other nuisance, by addressing a notice in writing, according to a

form prescribed, to the town-council, or trustees, or commissioners for drainage, &c., of any city, town, burgh, or place, or to any other body of a like nature, or to any commissioners of sewers or guardians of the poor, or in Ireland to the officers of health of any parish. And these public bodies must either compel the owner or occupier to remove the nuisance, or do it themselves at the public expense. In Scotland, the notice must be addressed and delivered to the procurator-fiscal of any county, or the procurator-fiscal or the dean of guild of any royal burgh, or the procurator-fiscal of the justices of the peace of any county, or the commissioners of police, or the trustees for paving, lighting, or cleansing any city, town, burgh, parish or place, or the inspector of the poor of any parish.

Having thus cursorily reviewed the acts for the improvement of the public health, passed during the last session of Parliament, we shall now indicate in very few words the chief points to which the attention of sanitary reformers should in our opinion be directed. They are—

1. The establishment of a local board of health in every town containing more than a certain number of inhabitants (say 10,000.)

2. The sanitary supervision of the small towns and rural districts by some existing machinery, such as that constituted by the boards of guardians and parochial boards, aided by medical and other superintending inspectors.

3. The extension of the Public Health Act in a more perfect form to the whole of Scotland and Ireland, and to the metropolitan districts.

4. The substitution of compulsory for permissive powers, in cases where the latter shall prove to be ineffective.

5. The abolition of the tax on light, and the diminution of the smoke nuisance.

6. The investment of the Central Board of Health with a general power of superintending the administration of the act by the local boards, and of compelling the latter to discharge actively and faithfully the duties which they have undertaken.

7. The appointment to every sanitary district of a medical officer of health, and the establishment throughout the country of a system of medical police.

This latter subject is one of such great importance, that we shall, in conclusion, submit to our readers, and, through them, to the public, some of the reasons which render those appointments one of the most urgent requirements of the age. And though we have said nothing on this point when commenting on the deficiencies of the Public Health Act, it cannot in justice to the profession be wholly passed over in silence.

The commissioners for inquiring into the health of towns state (Second Report, vol. i. p. 122), that—

"The most eminent medical witnesses concur in declaring, that it is by the careful observation of the causes of disease and mortality operating upon large classes of the community, that the mode and extent of their operation may be ascertained, and the power of diminishing and preventing them be acquired; that for this purpose the appointment of an officer, whose duty it would be to direct his undivided attention to such causes would, in our opinion, be a public benefit, more espepecially to the poorer classes, and might be advantageously employed in making investigations into matters affecting the sanitary condition of the district under his charge;" and recommend "that the local administrative body have power to appoint, subject to the approval of the Crown, a medical officer, properly qualified to inspect and report periodically upon the sanitary condition of the town or district, to ascertain the true causes of disease and death, more especially of epidemics increasing the rates of mortality, and the circumstances which originate and maintain such diseases, and injuriously affect the public health of such town or populous district."

In accordance with this recommendation, the bill introduced by Lord Lincoln not only proposed the appointment of a medical officer of health for the discharge of the above-mentioned duties, but also placed the services of this officer at the disposal of the coroner of the district in all judicial inquiries into the cause of death. In the Towns' Improvement Clauses Act, to which we have previously referred, a similar clause is inserted; and in the first Public Health Bill, introduced by Lord Morpeth, the same recommendation was acted upon. But the Public Health Bill of 1848, as first introduced into parliament, did not contain the slightest reference to this subject, or, in fact, to the existence of a medical profession. The duties of a medical officer of health, as laid down by the writer of a report of the Committee of the London Health of Towns' Association on Lord Lincoln's bill (generally understood to be Dr Southwood Smith), are sufficiently numerous and important.

"His primary or fundamental duties are the verification of the fact as well as of the cause of death, the correct registration of both, and the personal examination on the spot of the sanitary circumstances under which death takes place."—"There are," says the writer, "purposes to be answered by making it compulsory on the officer of health to verify in every case, by inquiries made on the spot, the fact of death, of far greater importance than are apparent at first view. In the performance of this duty, the medical officer, chosen, let us suppose, from the highest rank of his profession, and selected on account of his practical knowledge and his eminence in science, must necessarily visit whenever a death takes place the hovels of the lowest persons in the realm. Such a visit would be attended with inestimable benefit both to the individual families in particular, and to the community in general. The effect of such an appointment would be to place the most ignorant, forsaken, and helpless being in the community, in the moment of his utmost bewilderment and desolation, under the direction of an instructed mind; it would be to secure universally and invariably the presence of knowledge and science in the abode into which death follows the footsteps of disease, and in which are present the conditions that are necessary to give instant birth to pestilence, and to arm it with appalling power. With the visits of the policeman, the rent-collector, and the tax-gatherer, the poor are familiar; they see also the union surgeon, and the officer for administering parochial relief; occasionally the agent of some charitable society, and sometimes the clergyman; but there is no responsible officer, whose proper duty it is to visit

them in the season of distress and sorrow, for the express purpose of affording them protection, counsel, and help. The medical officer of health, with the qualifications here supposed, would be such an officer; his mission would be one purely of benevolence, and most healing and beneficent would be the influence of the appointment by the state of such a public servant for such a purpose. It may be necessary to arm him with certain summary powers—with the power, for example, to place the dead body, during the interval between death and interment, under circumstances which will secure it from injuring the living, to enforce ventilation when it is obviously necessary to the safety of survivors, and summarily to remove nuisances which originate and maintain disease."—"The station and education of the officer of health would be a sufficient security that his professional visit could not afford annoyance to the higher classes, while his visit must often be serviceable to them in various ways." And, "on the other hand, the experience of the local registrars affords direct evidence that the visit of the officer of health would not be unacceptable to the humbler classes."

The officer of health would thus in every case ascertain the true cause of death, and the very large number of cases in which the cause of death is now uncertified by qualified medical practitioners, would then be made to form part of the elements of statistical information; while men thus trained to a close observation of the causes of disease, and accustomed to pathological generalization, would, it is to be presumed, speedily enlarge, and impart additional certainty to the science of preventive medicine.

"Further, the correct registration of the fact and cause of death would put an end to false registration, at present not unfrequently made for fraudulent purposes; it would present more trustworthy materials for constructing and correcting tables of assurance; it would facilitate the recovery of the sums of money assured; and, as the present local registrars might act as auxiliaries to the officers of health, it would most materially improve the machinery for local registration in general."—"There is another service of great public importance which the medical officer of health might perform, namely, a supervision of the established medical parochial relief."—"Finally, there is one more service which the officer of health might render that deserves particular notice. In the very performance of his duties as a responsible officer, employed to verify the fact and the cause of death, he would necessarily exercise a powerful additional check upon crime."

The late enormous increase of the crime of poisoning adds considerably to the weight of this last argument; and when it is remarked that, in addition to these uses, the medical officer of health will act as confidential adviser to the local boards, and as a connecting link between the latter and the general board, it must, we think, be obvious that no sanitary measure can work efficiently without the appointment of such officers. Liverpool and the city of London have already spontaneously demanded, and immediately exercised, the power of making such an appointment; and we are satisfied that the most active opponents of the Public Health Act would not have objected to an amendment rendering the appointment compulsory upon the local boards of health.

We have now completed this necessarily imperfect review of a subject which is as yet but in the process of development. For the attainment of the enactments required to complete this, the first chapter of sanitary legislation, the public must mainly rely

upon the exertions of the local associations already formed in many of our large towns, and upon the establishment of similar philanthropic unions in those districts where they have not hitherto been organized. In this work the members of our profession will in most cases be expected to take the initiative, and sure we are that they will every where prove themselves fully equal to the demand made upon them in the name of humanity and religion; and that in future ages no fairer spectacle will present itself to the Christian philosopher and historian, than that of the disinterested patriotism and untiring benevolence of the present generation of medical practitioners. G. R.

The Treasury of Natural History; or, a Popular Dictionary of Animated Nature, &c. With Nine Hundred Woodcuts. By SAMUEL MAUNDER.

THE works of Mr Maunder are miracles of laborious compilation, and, although not pretending to any of the higher literary or scientific qualifications, have always been so copious and generally so correct in their information, as to have secured for them justly a large share of public approbation. We can state confidently, from an examination of many of its principal articles, that this work is equal, if not superior, to any of its predecessors.

The volume consists of a treatise on the natural history of the animal kingdom, the English names of the animals being arranged in alphabetical order. The work is concluded by an excellent synopsis of practical taxidermy; including directions for the preparing and also for the procuring of specimens of natural history.

The Diagnosis and Treatment of the Eruptive Diseases of the Scalp. By J. MOORE NELIGAN, M.D., M.R.S.A., Physician to Jervis Street Hospital, &c. Dublin: 1848. 12mo, pp. 55.

THIS little work, to which we have already been indebted in the pages of our *Retrospect*, has been judiciously reprinted from the *Dublin Quarterly Journal*. It is admirably calculated for the use both of students and practitioners, by the clear and simple style of its descriptions. We are glad to see that the author has forsworn entirely the obsolete pedantry of classification, by which diseases of the scalp were erected into a separate class, and then subdivided without end or meaning. He has undoubtedly taken the true view of favus, in regarding it as a parasitic disease, requiring a peculiar soil for its existence, and connected with a constitutional taint; although, for the treatment, we would rather trust to generous diet and warm clothing, along with the proper local manage-

ment, than to the iodide of arsenic, recommended by Dr Neligan; and we suspect that, without attention to these hygienic influences, little save temporary good will be gained by the exhibition of remedies. We do not understand why the author has placed pityriasis among the inflammatory affections: in a work so decidedly practical in its character, this is surely an erroneous classification. We mention these points as the principal ones in regard to which we differ from Dr Neligan. To most of his opinions we can give a hearty assent, and have no hesitation in recommending this work as an excellent and practical guide, free from all unnecessary and cumbrous book-learning, to the diagnosis and treatment of diseases of the scalp.

Part Third.

MEDICAL NEWS.

EDINBURGH OBSTETRIC SOCIETY.

SESSION XXVII.

MEETING VII.—*Wednesday, 12th July* 1848.—Dr SIMPSON, President, in the Chair.

REPORT OF THE EDINBURGH ROYAL MATERNITY HOSPITAL, ST JOHN'S STREET. BY PROFESSOR SIMPSON.

THE Edinburgh Maternity Hospital was opened in St John's Street in May 1844, and continued in this locality till May 1846, when the Charity was removed to its present premises in Milton House, Canongate. The following Report comprehends an analysis of the obstetric practice of the Institution during the two years in which it was located in St John Street. The Report was drawn up and communicated to the Medico-Chirurgical Society twelve or eighteen months ago; and its publication was for a time postponed, under the hope that I might find leisure to render it still more minute and extensive. As, however, I have little prospect of fulfilling that wish, I now publish it, with the permission of the Obstetric Society, in the printed account of their transactions. And, I trust, that the continuation of the Reports of the Institution will, betimes, be drawn up and communicated to the Society by some of the younger and more active medical officers attached to the hospital.

At the time the hospital was opened, the directors appointed to it the following medical staff:—Drs Campbell and Beilby, consulting physicians; Dr Pagan, consulting surgeon; Drs Moir and Simpson, ordinary physicians; Dr Ziegler, ordinary surgeon; and Drs Bell, Thomson, Niven, and Carmichael, assistant medical officers. One or two advanced pupils were appointed to live in the house as resident house-surgeons.

I. *The Number of Cases.*—Of women delivered in hospital, there were 374. Of women delivered at their own homes in various parts of the town, there were 1101; making a total of 1475 women delivered under the superintendence of the Institution. Among these are included 58 cases of miscarriage or premature labour.

II. *The Ages of the Women.*—In 1457 of the cases, the ages of the women were noted. In 18 cases the ascertaining of this point was omitted. The following table exhibits, in detail, the number of patients delivered, with their respective ages:—

Ages of the Women.	Number of Women.	Ages of the Women.	Number of Women.
16	4	33	47
17	10	34	40
18	30	35	49
19	57	36	46
20	64	37	23
21	75	38	33
22	101	39	18
23	90	40	37
24	84	41	8
25	77	42	11
26	107	43	9
27	69	44	6
28	94	45	2
29	69	46	2
30	107	47	1
31	34	48	1
32	52		
Total	... ...		1457

III. *The Quickening.*—The average period of quickening, as calculated from the intern cases where it was noted, was 183 days after the disappearance of the last catamenia.

IV. *The Duration of Pregnancy.*—In the intern cases where the data are afforded, the average duration of pregnancy, calculating from the end of the last catamenia, was 273 days. But on the preceding computation little confidence can be placed, considering the class of patients from which the data are drawn.

V. *The Number of the Pregnancy.*—In 1459 cases, the number of the pregnancy is stated, namely, whether it is the first, second, or third time, &c., the woman has been pregnant; in 16 cases it is omitted. 398 women were delivered of their first children; and thus the frequency of first deliveries was to that of all subsequent deliveries in the proportion of about 1 to 4. The following table exhibits at length the number of cases, with the corresponding number of the pregnancy in each.

Number of the Pregnancy.	Number of Women.	Number of the Pregnancy.	Number of Women.
1	398	10	38
2	294	11	24
3	161	12	11
4	158	13	7
5	116	14	2
6	88	15	1
7	65	16	1
8	57	17	1
9	36	20	1
Total	... ...		1459

VI. *The Number of Children.*—The children resulting from the 1417 labours, at or near the full time, were 1436 in number. Of these there were born

In single births	1399	children
... twin births	34	...
... triplet birth	3	...
Total	1436	

VII. *The Sex and State of the Children.*—The children are further to be distributed as follows :—

Sex of the Child.	Number of Children.	Number Born Alive.	Number Born Dead.	Proportion of Still-births.
Male	739	682	57 or	1 in every 13
Female	693	650	43 or	1 in every 16
Sex not stated	4	3	1	... ...
Total	1436	1335	101	1 in every 14

Excluding from the above list of still-born children 37 which were putrid, there remain 64 whose death was recent. Of these 64, there were born

Under cranial presentations	40
... breech presentations	8
... footling presentations	4
... arm presentation	1
... shoulder presentation	1
... presentation or prolapse of the cord	8
... placentar presentation	1
Presentation not stated	1
Total	64

In one of the cases of still-birth under cranial presentation, the mother had convulsions during labour ; and, in another, the child was a male twin ; in a third, the cord was twisted twice around the neck ; in a fourth, the child exhibited marks of abdominal disease ; and in a fifth, the delivery was completed by perforation. Six of the dead children were born in labours complicated with hemorrhage during or before labour.

Of the 37 children born putrid, 18 were male and 19 female infants ; and the presentations were as follows :—

The head presented in	26 cases.
... breech ...	6 ...
... feet ...	1 ...
... shoulder ...	1 ...
Presentation not stated in	3 ...
Total	37 ...

During pregnancy two of the mothers whose children were born putrid, laboured under syphilitic complaints ; and a third was affected with ascites and anasarca. In several of the still-born children which I took the opportunity of examining along with the pupils, effusion of coagulable lymph (the consequence of extensive inflammation) was found covering various parts of the peritoneal membrane.

VIII. *The Length of the Children.*—The extreme length or height of the child at birth is stated in 330 cases of single birth completed in the hospital. 169 of these children were male and 161 female.

Of the male children there measured in length—

Under 15 inches,	1 child	Under 20 inches,	53 children
... 16 ...	1 ...	... 21 ...	28 ...
... 17 ...	3 children	... 22 ...	21 ...
... 18 ...	14 ...	... 23 ...	3 ...
... 19 ...	45 ...		
Total . .		. .	169

Of the female children there measured in length—

Under 15 inches,	1 child	Under 20 inches,	40 children
... 16 ...	1 ...	... 21 ...	32 ...
... 17 ...	1 ...	... 22 ...	13 ...
... 18 ...	10 children	... 23 ...	2 ...
... 19 ...	60 ...	... 24 ...	1 child
Total . .		. .	161

Average length of male child 19½ inches
... female child 18¾ ...
... both sexes 19⅓ ...

IX. *The Weight of the Children*—The following table gives in detail the respective weights of 337 children delivered within the hospital, including 171 males and 166 females. The children born in plural births are excluded from the table.

Of the male children there were :—

Under 4 lbs.,	4 children.	Under 8 lbs.,	60 children.
... 5 ...	7 ...	... 9 ...	31 ...
... 6 ...	17 ...	... 10 ...	3 ...
... 7 ...	45 ...	... 11 ...	4 ...
Total . .		. .	171 ...

Of the female children there were :—

Under 4 lbs.,	2 children.	Under 8 lbs.,	50 children.
... 5 ...	4 ...	... 9 ...	27 ...
... 6 ...	17 ...	... 10 ...	7 ...
... 7 ...	59 ...		
Total . .		. .	166 ...

The lightest child (premature?), a male, 3 lbs. 4 oz. in weight.
The heaviest child, a male, 10 lbs. 8 oz. ditto.
The heaviest female child, 9 lbs. 7 oz. ditto.
The average male child, 7 lbs. 3 drs. ditto.
The average female child, 6 lbs. 10 oz. 11 drs. ditto.
The average of both sexes, 6 lbs. 13 oz. 7 drs. ditto.

X. *The Length of the Umbilical Cord.*—In 327 cases of children delivered within the hospital, the length of the cord was noted. Among these—

The shortest cord measured 12 inches in length.
The longest ditto 43 ditto.
The average ditto 23½ ditto.

In a note attached to the details of an extern case the cord is stated to have been 50 inches long.

XI. *Weight of the Placenta.*—In 325 of the cases delivered within the hospital, the weight of the placenta is noted.
The lightest placenta weighed 7 oz. Premature child?
The heaviest ditto, 2 lbs. 15 oz. The single placenta of twin children.
The average ditto, 1 lb. 4 oz. 14 drs.

XII. *The Duration of Labour.*—The duration of labour from the first com-

mencement of pains was ascertained in 311 cases, all delivered within the hospital. The following table exhibits the average length of the whole labour, and of the first, second, and third stages separately in these 311 cases :—

Average duration of first stage of labour,				11	hours	50	minutes.
Ditto	ditto	second	ditto	1	„	37	„
Ditto	ditto	third	ditto	0	„	38	„
Ditto	ditto	whole labour		12	„	57	„

The following table exhibits in detail the duration of the whole labour, and of each stage of labour respectively, in all the cases. It reads thus :—The whole labour was completed in 1 hour in 4 cases ; in 2 hours in 4 cases ; and so on. The first stage of labour was completed in 1 hour in 3 cases ; in 70 hours in 1 case, &c.

Duration in Hours.	Whole Labour.	First Stage.	Second Stage.
1	4	3	161
2	4	9	65
3	7	11	20
4	16	20	16
5	17	26	10
6	16	17	2
7	28	18	4
8	21	18	1
9	17	16	2
10	20	16	1
11	20	15	4
12	12	16	0
13	23	9	0
14	14	9	1
15	8	5	0
16	6	6	0
17	6	3	0
18	8	5	0
19	10	6	0
20	3	7	0
25	22	14	1
30	12	8	0
35	5	5	0
Above 36	14	9	0
Duration not stated.....	61	85	103

The third stage was completed in	5	minutes in	. .	40	cases.
Ditto	10	...	. .	33	...
Ditto	15	...	. .	80	...
Ditto	30	...	. .	104	...
Ditto	1	hour	. .	16	...
Third stage completed within	1	hour in	. . .	328	...
Ditto	1½	...	. . .	7	...
Ditto	2	...	. . .	1	...
Duration not stated in			. . . . .	38	...

XIII. *The Modes in which the Children presented.*—In 1421 of the births the presentation of the child is given. The following table exhibits in detail the various presentations, and the number of each respectively in 1421 cases :—

Presentation.	Number of Cases.
Head	1333
A hand descending with the head	31
Both hands descending with the head	1
Face	6
Shoulder	3
Arm	3
Breech	27
Breech and feet	2
One or both feet	15
	1421

The cord presented along with some part of the fœtus in 5 cases.

The cord became prolapsed in 13 cases.

The placenta presented partially along with some part of the infant in 3 cases.

In the preceding table of presentations I have entirely omitted to classify those of the cranium, according to the different positions in which the head may present; because I do not place much reliance upon the perfect accuracy of the reports of positions entered in the hospital records by the younger pupils. In fact, one of their principal objects in hospital and dispensary instruction is to acquire, among other things, a practical knowledge of the somewhat difficult subject of positions. But I have great pleasure and confidence in citing the notes on this subject of Dr Martin Barry, who acted for sixteen months as house-surgeon to the hospital, more especially as the data which he has obtained entirely coincide and agree with the results of my own observations. Dr Barry himself carefully observed and noted the position of the head in 335 cases of cranial presentation, among the patients of the Institution. I shall classify these 335 cases according to the four positions and numerical nomenclature used by many of the German schools :—

I. Occipito-anterior Positions.

1. Position ; or occiput directed to left foramen ovale, in 256 cases.
2. Position ; or occiput directed to right foramen ovale, ... 1 ...

II. Occipito-posterior Positions.

3. Position ; or occiput directed to right sacro-iliac synchondrosis ... 76 ...
4. Position ; or occiput directed to left sacro-iliac synchondrosis ... 2 ...

Total . . . . . 335 ...

In the cases of occipito-anterior position the head was invariably found to descend and emerge, without any change or movement from the same oblique diameter of the pelvis as it was originally placed in, with one exception. In this solitary case the head rotated from the first into the fourth position, in which it was born.

Out of 78 cases of occipito-posterior position carefully watched, in 75 the occiput turned forwards, and emerged first from under the arch of the pubis. In 2 cases of the third position the occiput continued directed backwards, and the child was expelled without the usual rotation into the first position. Thus the exceptions to the rotation did not occur oftener than about once in thirty cases.

Of the two cases which presented in the fourth position, in one the rotation was performed into the first position. In the other, the head continued to maintain the same position till it was born.

XIV. *The Plural Births.*—Among the 1417 women delivered, 17 or 1 in every 83 gave birth to twin children, and in one case triplets were born.

In the *twin cases* the presentations and state of the children were as shown in the following table :—

Presentation.	Number of Cases.	Number of Children Born Alive.	Number of Children Born Dead.
Double cranial presentation .	3	13	3
1st child, head ; 2d child, breech	6	11	1
1st child, feet ; 2d child, head .	1	2	
1 child, arm ; other not stated .	1	2	
1 child, head ; other not stated	1	2	
Total . .	17 cases or 34 children	30	4

The sexes of the twin children were as follows :—

In 3 cases, both children male . .		6
... 5 cases, ... female . .		10
... 9 cases 1 child male and 1 female .		18
Total	17	34

The average weight of the twin child was 6 lbs. 1 oz.

In the *triplet* case, two of the children presented the head, the third presented the feet. Two of the children were male, one female. All were born alive and vigorous, but, before leaving the hospital, one of the children was accidentally overlaid by its mother. The male children weighed respectively, 5 lbs. 11 oz. and 5 lbs. 2 oz. ; the female child weighed only 3 lbs. 12 oz. The labour was 8½ hours in duration.

XV. *The Mode of Delivery.*—Deducting the abortions and miscarriages, which amounted in number to 58, there remain 1417 cases of delivery about the full time. The following table shows the modes of delivery in these 1417 cases, and their relative frequency to each other. Among those naturally delivered is included a case of spontaneous cephalic evolution or expulsion :—

Mode of Delivery.	Number of Cases.	Proportion.
Naturally	1404	1404 in 1417
Artificially	13	1 ... 107
Mode of Artificial Delivery.		
Traction of presenting feet . . .	1	1 in 1417
Bringing down a foot (breech presentation)	1	1 ... 1417
Turning	6	1 ... 236
Forceps	3	1 ... 472
Crotchet	1	1 ... 1417
Induction of premature labour . .	1	1 ... 1417

Turning was adopted in five cases for preternatural presentations, and in one case for the purpose of hastening delivery under an attack of apoplectic convulsions. The breech presentation, in which a foot was brought down, was a case in which the presenting part did not pass the brim, notwithstanding strong and powerful pains of upwards of a day's duration.

XVI. *The Complicated Labours.*—The following table contains a list of the chief complications observed in the 1417 labours, with the respective numbers and proportion of each :—

Hemorrhage from partial placenta prævia, in	3	cases, or	1	in every	472	cases
——— before labour,	12	...	1	...	118	...
——— during labour,	4	...	1	...	354	...
——— between the expulsion of the child and of the placenta,	16	...	1	...	87	...
——— post partum,	24	...	1	...	59	...
——— (indefinitely stated),	8	...	1	...	177	...
Total of cases of hemorrhage,[1]	67	...	1	...	21	...
Convulsions during labour,	4	...	1	...	354	...
Prolapsus of the entire uterus to the outlet,	2	...	1	...	708	...
Rigidity of the os uteri,	11	...	1	...	128	...
Carcinoma of the os uteri,	1	...	1	...	1417	...
Prolapsus of the anterior lip of the uterus,	6	...	1	...	236	...
Expulsion of the entire ovum (membranes unbroken),	1	...	1	...	1417	...
Prolapsus of the cord,	18	...	1	...	79	...
Cord once or twice around the child's neck, in	158	...	1	...	9	...
—— three times around the child's neck,	4	...	1	...	354	...
—— four times do. do. do.	2	...	1	...	708	...
—— encircling the neck and one hand,	1	...	1	...	1417	...
—— around the neck and arm,	4	...	1	...	354	...
—— around the neck and body,	2	...	1	...	708	...
—— around the shoulder,	2	...	1	...	708	...
—— around the body,	2	...	1	...	708	...
—— around the arm,	1	...	1	...	1417	
—— around the leg,	2	...	1	...	708	...
—— snapped across before delivery (12 inches long),	1	...	1	...	1417	...
Amaurosis and delirium before delivery,	1	...	1	...	1417	...
Gangrenous slough from the interior of the rectum,	2	...	1	...	708	...

XVII. *The Instrumental Deliveries.*—Instruments were had recourse to in very few cases. The following table exhibits the number and proportion of the deliveries in the 1417 labours :—

Instrumental delivery,	in 4	cases, or	1	in every	354	cases
Viz. Delivery by the forceps,	3	...	1	...	472	...
Delivery by the crotchet,	1	...	1	...	1417	...

Of the four mothers, one died eight days after delivery from puerperal fever. She had been in labour upwards of two days, and was ultimately delivered of a dead child by the short forceps. Of the children extracted by the forceps, two were still-born. In both cases the head had descended into the pelvis before the instruments were applied. In one of these two cases the hospital record states, that, from the state of the cord, the child appeared to have been dead for some time before delivery. It was born after above two days' labour. In the second case the woman had been upwards of twenty-four hours in labour. Convulsions came on an hour and a half before delivery, and returned several times. The child was of very large size, weighing 10 lbs. 2½ oz.

Delivery by the crotchet was resorted to, in consequence of the head of the child being arrested at the brim of a distorted pelvis.

The proportion of cases in which instrumental delivery was had recourse to in the Edinburgh Hospital, was smaller than in most other Obstetric Institu-

[1] In the Hospital books "hemorrhage" is frequently noted when there was a more than usual loss of blood, although it may have been in comparatively small quantity.

tions. The following table will perhaps illustrate this point more fully than any more lengthened remarks. The first column of figures shows the absolute number of labour cases reported by the different practitioners named; the second column shows the proportion of instances in each of the returns in which the practitioner resorted to delivery by instruments; and the third and fourth columns respectively show the comparative proportion in which the two modes of instrumental delivery (the forceps and crotchet) were employed by each practitioner. It will be seen how frequently the forceps were employed by some in comparison with craniotomy; how frequently craniotomy was employed by others in comparison with the forceps; and how very different the proportion of cases supposed to require instrumental delivery at all, is in the practices of different medical schools and institutions.

Table showing the Proportion of Instrumental Deliveries, and of Deliveries by the Forceps and by Craniotomy in different Obstetric Institutions.

Name of Reporter.	Total Number of Labours Reported.	Proportion of Instrumental Deliveries.	Proportion of Deliveries by Forceps.	Proportion of Deliveries by Craniotomy.
Siebold—Berlin . .	2093	1 in 7	1 in 7	1 in 2093
Busch—Berlin . .	2056	1 ... 11	1 ... 12	1 ... 342
Carus—Dresden . .	2549	1 ... 13	1 ... 14	1 ... 283
Nægele—Heidelberg .	1711	1 ... 31	1 ... 31	1 ... 1711
Bland—Westminster .	1897	1 ... 95	1 ... 158	1 ... 237
Beatty—Dublin . .	1182	1 ... 98	1 ... 131	1 ... 394
Collins—Dublin .	16,654	1 ... 115	1 ... 617	1 ... 141
Churchill—Dublin .	1640	1 ... 117	1 ... 546	1 ... 149
Lever—London . .	4666	1 ... 137	1 ... 518	1 ... 186
Boer—Vienna . .	9589	1 ... 199	1 ... 274	1 ... 737
Lachapelle—Paris .	22,243	1 ... 252	1 ... 293	1 ... 1854
Ramsbotham—London .	48,682	1 ... 322	1 ... 553	1 ... 773
Simpson—Edinburgh .	1417	1 ... 354	1 ... 472	1 ... 1417

XVIII. *The Maternal Deaths.*—Among the 1475 women delivered under the superintendence of the Institution, eleven deaths occurred, or one in 134. Of these, seven occurred among the cases delivered in the hospital, and four among the out cases.

Four of these eleven patients were in their first labours; three in their second; and the remaining severally in their third, sixth, ninth, and twelfth.

The cause of death was,—

Puerperal fever and inflammation in . . .	7 cases
Puerperal convulsions in	2 ...
Chest disease in	1 ...
Fatal sinking after incision of a carcinomatous os uteri in	1 ...
Total, .	11 deaths.

The delivery was effected by the natural efforts in nine of the cases; by the forceps in one of the cases dying of puerperal fever; by turning in one of the cases dying of convulsions,—the body, on dissection, presenting a large clot of effused blood in the brain, and a collection of pus in one of the kidneys. In the case where the os uteri was incised for carcinoma, the mother had been in labour for two or three days, and the pulse had become very rapid, &c., before the operation was adopted. The child was subsequently born in the course of four or five pains. But the pulse never rallied after delivery, and she died on the third day.

I have merely further to add, that the convalescence of the hospital patients was often interrupted by febrile and inflammatory attacks. The house, No. 3, St John Street, used as an hospital, was, in addition to a sunk kitchen flat, three storeys in height, with two rooms in the first and second flats, and three in the third; and it was much too small for the accommodation of the patients, and of the resident house-surgeons, matron, &c. The rooms themselves were low-roofed, and very imperfectly ventilated. Various plans were attempted to improve the ventilation, but not with much success. Whenever the hospital became in any degree crowded, fevers (or weeds), with more or less abdominal tenderness, appeared among the patients; and frequently the supervention of such attacks during convalescence became so common, as to constitute the rule rather than the exception to it. Every one acquainted with hospital practice, whether obstetric, surgical, or medical, is well aware of the great liability among the patients to febrile and inflammatory attacks whenever the wards are overcrowded; and in no practice is this more visible than in midwifery. Indeed, I believe there are few or no circumstances which would contribute more to save surgical and obstetric patients from phlebitic and other analogous disorders, than a total change in the present system of hospital practice. I have often stated and taught, that if our present medical, surgical, and obstetric hospitals were changed from being crowded palaces, with a layer of sick in each flat, into villages or cottages, with one, or at most two patients in each room, a great saving of human life would be effected. And if the village were constructed of iron (as is now sometimes done for other purposes), instead of brick or stone, it could be taken down and rebuilt every few years; a matter apparently of much moment in hospital hygiene. Besides, the value of the material would not greatly deteriorate from use; the principal outlay would be in the first cost of it. It could be erected in any vacant space or spaces of ground, within or around a city, that chanced to be unoccupied; and, in cases of epidemics, the accommodation could always be at once and readily increased.

PUBLIC HEALTH ACT.

The following pages contain an account of the most important sanitary provisions of this bill. For a more extended view of its different clauses, we refer our readers to the bill itself, or to the abstract in the *Journal of Public Health* for September 1848, to which we are in great part indebted for the following condensed account:—

General Board of Health.—The act is of limited application, including only England and Wales, and excluding London and the metropolitan sewerage districts. Its first and most important object is to provide a *General Board of Health*, to consist of the first Commissioner of Woods and Forests as president, and two other persons to be appointed by her Majesty, and removeable at pleasure. One of these is to be a salaried officer. The Board is to appoint a secretary, superintending inspectors, and such other officers as they see fit, subject to the approval of the Treasury. The superintending inspectors to have salaries of not more than three guineas a day, besides travelling expenses.

Petition for Inquiry.—The general board may, on the petition of one-tenth of the poor-rate payers ("not being less than thirty in the whole") of any city, town, borough, or place, having a known or defined boundary, direct a superintending inspector to make public inquiry as to the sewerage, drainage, water supply, burial-grounds, number and sanitary condition of inhabitants, existing local Acts of Parliament for sanitary purposes, natural drainage area, existing municipal or other local boundaries, best boundaries for the purposes of this act, and such other matters as the general board may require to enable it to report, or make a provisional order. The general board may also institute an inquiry, and take the right steps thereon, in all cases where the annual mortality of any city, town, borough, parish, or place, shall be found, on an average

of seven years, as shown by the registrar-general's last returns, to exceed twenty-three to a thousand of the population.

Notice of Inquiry and Report.—Before proceeding with the inquiry, the superintending inspector shall give fourteen days' notice of his intention to make the same, and that he will be prepared to hear all persons desirous of giving evidence. Public notice to be by advertisement, and by fixing notices to the doors of the principal churches and chapels, and also on the public buildings. The inspector shall then report to the general board; and if it shall appear that the best boundaries for the purposes of this act are not the same as those of the city, town, borough, or place reported on, n superintending inspector shall visit and examine the parts "within the boundaries proposed to be adopted," in the same manner and after similar public notice. And after he has reported to the general board, the report shall be published in the parts to which it refers, and copies shall be deposited with the town clerk of any corporate borough, with the clerk to the justices of any petty sessional division, and with the clerk of the board of guardians of any union or parish affected thereby: and with the clerk to any board administering any local act for sanitary purposes.

Application of the Act.—After the inquiry and report above mentioned, her Majesty may, by an order in council, or a provisional order, direct the application of the provisions of the Act to the district reported on; the consent of the local authorities being unnecessary, except where a change of boundaries is proposed.

Local Boards of Health.—In corporate boroughs, these are to consist of the mayor, aldermen, and burgesses; in non-corporate boroughs and extra-municipal districts, the board is to consist of members elected by the ratepayers, to a number fixed by order in council or provisional order. The members must be resident within the district, and must possess certain other qualifications mentioned in the Act. The local boards are to hold meetings at least once a month.

Officer of Health.—The local board may appoint "a legally qualified medical practitioner, or a member of the medical profession," to be their officer of health, removeably by them, and who shall perform such duties as the *General Board of Health* shall direct. He may be officer of health for two or more districts, and such salary or other remuneration shall be paid to him out of the general district rates, as the local board or boards may direct, and where there are more than one district under his charge, in such proportion from each as the general board may appoint. The appointment and removal of the officer of health to be subject to the approval of the general board.

Sewers.—The local Board of Health shall repair and maintain all sewers vested in them, and shall cause to be made such main sewers and drains as are necessary for effectual drainage of their district. After notice in writing, such sewers may be carried through any property. Existing sewers may be discontinued, provided no private right be interfered with, and in such case an equivalent sewer must be provided. The local board shall cleanse all sewers and drains belonging to them, and construct such reservoirs, sluices, engines, &c., as may be needful. It may cause sewers to be emptied into needful places, and the manure sold; but not to create nuisance.

House-drains, Privies, &c.—No house shall be built or rebuilt without sufficient drainage, on report of the surveyor, and if the sea, or any sewer or drain which the local board can make use of, be within 100 feet of any part of the site of the house, the drain from the house must communicate with either, as the local board shall direct. Where there is no main drainage, then a cesspool must be constructed to receive the house-drain, at such a distance from the house as the local board shall direct. Neglect of these provisions shall incur a penalty not exceeding L.50. All existing houses shall be similarly drained, and if the requisite drainage be not completed after due notice, then the local board may execute the works, and recover the expense from the owner in a summary manner, or as private improvement expenses.

No house shall be built or rebuilt without a sufficient water-closet or privy, and an ash-pit furnished with proper doors and coverings, under a penalty not exceeding L.20, for neglect of the law. In existing houses, where there are not sufficient conveniences, the local board shall also give notice to the owner or occupier, and if within reasonable time the proper conveniences be not constructed, the local board may execute the same, and charge the cost.

All places of manufacture, trade, or business, where above twenty persons of both sexes are employed at the same time, must be provided with separate water-closets or privies for the use of each sex. And if, after due notice from the local board, the proper conveniences be not constructed, a penalty not exceeding L.20 may be inflicted, and a further penalty of L.2 a day during the time of default.

The local board shall provide that all drains, privies, &c., within their district do not occasion nuisance.

If any person make a written application to the local board (but not otherwise), complaining of any nuisance arising from any drain, water-closet, privy, cesspool, or ash-pit, the surveyor, by written authority of the local board, and after twenty-four hours' notice, or, in case of emergency, without notice, may enter the premises, with or without assistants, and make examination. If every thing be found in proper order, the damage must be repaired at the expense of the local board; but, if alteration or amendment be required, the ground shall be closed, and the local board shall send notice to the owner or occupier, giving reasonable time for the purpose; and if the notice be not complied with, a penalty of 10s. a day may be levied, and the local board may execute the necessary works, and recover expenses from the owner.

Street Cleansing.—The local board shall at all convenient and proper times cause a thorough surface cleansing and watering of all streets, &c., and it may make bye-laws for the removal of refuse from, or about houses, stables, cow-houses, &c., and for preventing accumulations of such matters, and also for cleansing and emptying of water-closets, cesspools, &c. Bye-laws regulating removal of manure, &c., by occupiers, may also be made.

Places of Deposit.—The local board may provide convenient boxes and places for deposit of refuse matters, sewage, &c., which shall be vested in the board and sold, and the proceeds carried to the district fund account. All persons removing refuse without consent of the local board, shall be liable to a penalty not exceeding L.2 for every offence.

Public Necessaries.—The local board shall provide public water-closets, privies, and other conveniences, in proper situations, and charge the cost to the district rates.

Offensive Drains to be cleansed.—The local board shall cause to be drained, cleansed, covered, or filled up, all ponds, pools, open ditches, drains, and places containing or used for the collection of any drainage, filth, water, matter, or thing of an offensive nature, as likely to be injurious to health. Written notice is so be sent to the person causing nuisance, or to any owner or occupier on whose premises it exists, and in default of compliance the local board shall execute the works, and charge the cost. But the local board may order the whole or a portion of the expenses to be paid out of the special or general district rate.

Penalties for keeping Swine, &c.—The local board may serve notice of nuisance on any person keeping swine in a dwelling-house, or in any other way so as to occasion nuisance, or who permits water to accumulate in any cellar or place within a house, or who permits any soakage or overflow from any water-closet or privy; and if in twenty-four hours after such notice the nuisance be not abated, a penalty not exceeding L.2 may be inflicted, along with 5s. for every day during which the nuisance is continued, and the local board shall abate the nuisance and charge costs on the occcupier. The inspector of nuisances shall also give notice of all collections of refuse causing nuisance, and if they are not removed in twenty-four houses, they shall be removed, and be-

come the property of the local board, the proceeds being carried to the district fund account.

Purification of Houses.—Any house, or part of a house, which is certified to the local board by the officer of health (if any), or by any two medical practitioners, to be in a filthy or unwholesome condition, and tending to produce disease, or if disease may be checked by white-washing or purifying the same, it may be ordered by the local board to be so purified, on written notice to the owner or occupier; and in default, after the specified time, the offender shall be liable to a penalty not exceeding 10s. a day, and the local board may cause the necessary purification to be made, and the cost recovered from the owner or occupier.

Slaughter-Houses, &c.—All slaughter-houses and knackers'-yards shall be registered by the owner or occupier in a book kept by the local board, within three months after the application of this Act, or within three months of the time of use, under a penalty not exceeding L.5, and 10s. for every day after notice from the local board.

Local boards may provide public slaughter-houses, and make bye-laws for their regulation, and also for the regulation of all slaughter-houses and knackers'-yards, for the cleansing of them; but the rights of all companies incorporated by local acts to be respected.

Inspector of Meat, &c.—The inspector of nuisances may at all reasonable times enter, with or without assistants, and inspect any place where butcher's meat is slaughtered or sold, or where game, poultry, or fish is sold, and may examine their contents, and seize all meat, poultry, game, flesh, or fish unfit for food, and any justice may cause the same to be destroyed, or kept from being sold as food, if he think fit, after receiving evidence from some competent person; and the individual in whose custody unsound meat, poultry, game, or fish is found, shall be liable to a penalty not exceeding L.10 for every separate piece so found, to be recovered before two justices.

Offensive Trades.—No offensive trade shall be newly established in any building or place, after the application of this Act, without the consent of the local Board of Health, unless the general board shall otherwise direct, under a penalty of L.50, and L.2 a day; and all such works shall be under the regulation of the local board.

This Act does not affect the present law as to nuisances.

Lodging-Houses to be Registered.—Every common lodging-house must be entered on a register kept by the local board, which shall make regulations for fixing the number of lodgers, and for proper cleanliness and ventilation. Officers appointed by the local board shall have access, at all times, to such houses, for inspecting and disinfecting the same, at the expense of the occupier; and if admittance be refused in the daytime, between eleven and four o'clock, the offender shall be liable to a penalty not exceeding L.2. Neglect of registration to be visited by a similar penalty.

Regulation of Cellars.—No cellar, to be built or rebuilt after the passing of this Act, can be let separately as a dwelling, or no cellar which has not been so let before the passing of this Act. And *no existing* cellar shall be let separately as a dwelling, except it be seven feet high, and have three feet of its height above the level of the street; provided also that there be an area in front two feet six inches in width, and the whole height from six inches below the level of the floor of the cellar to the level of the street; and in any case there must be a sufficient drain laid at least one foot below the floor of the cellar, along with a proper fire-place, water-closet or privy, and ash-pit. The window must be nine feet clear of the frame, and made to open; and if there be a back cellar, the window must be four feet clear of the frame. The churchwardens and overseers to give public notice of the law, and the local board to give written notice to proprietors, and in default of compliance a fine of 20s. a day may be inflicted. Steps for access may be constructed, so as not to obstruct the window. Any cellar or vault in which a person passes the night, is a dwelling

under this Act. The Act not to be in force as to cellars till one year after its passing, nor till six months after its application to any district.

Management of Streets.—All present or future streets and highways, with their materials, are vested in the local board, whose duty it shall be to see that they are properly levelled, paved, flagged, channelled, and repaired, and proper fences kept for the safety of foot passengers. Wilful injury, without consent of the board, to be punished by a fine not exceeding L.5, and 5s. for every square foot injured.

Any present or future street (not being a highway) which is not properly sewered, paved, &c., must be put in a proper state, on notice being served on owners or occupiers of premises fronting or abutting on the defective parts of the streets; and if the notice be not complied with, the local board may execute the works, and charge costs upon owners, according to their frontage, and under advice of the surveyor, or by arbitration.

Any street (not a highway) which may have been paved, &c., to the satisfaction of the local board, may be adopted by the board on public notice, and repaired afterwards out of the rates; but the proprietor of a private street may object within a month after public notice.

The local board may compel any alteration in the position of gas and water pipes, paying the cost out of the general district rate, unless there be legal liabilities on companies; but the use of the pipes is to be preserved unimpaired.

Before laying out any new street, a month's notice must be given to the local board, with the level and width of the street, which shall be fixed by the local board. All persons contravening this clause, are liable to a fine of L.20 a day, and to have alterations made in their property, to bring it into conformity with the Act, and the cost charged upon them. But if the notice be not replied to in one month, parties may proceed to lay out the street; and, should they feel themselves aggrieved by the proceedings of the local board, they may appeal to the general board.

The local board may purchase such premises as may be needful for improving lines of street; but any portion more than necessary must be re-sold, for the benefit of the district-fund account.

Pleasure Grounds.—The local board may purchase and keep in order grounds for public recreation, or pay towards the support of such grounds, provided by any person whomsoever, under approval of the general board.

Water Supply.—It shall be lawful for the local board to provide a constant supply, at pressure, of pure water, for the purposes of this Act. It may erect new works, contract with companies either for supply or lease of works, or it may purchase water-works, &c., with the approval of the general board. But before laying down any new works, the local board must give notice in writing to all water companies within whose limits it requires a supply, stating the amount required, &c.; and so long as the companies can supply enough, and on such terms as the general board shall deem reasonable, or as may be agreed on by arbitration, the local board cannot lay down new works.

If, on report to the surveyor, it appear to the local board that any house is inadequately supplied with water, and that it is possible to obtain a supply on terms not exceeding twopence a-week, notice to supply shall be given to the occupier; and, if this be not complied with, the local board may execute the necessary works, and levy a water-rate not exceeding twopence a-week, and charge the cost of works as improvement expenses.

The local board may supply water for trading or manufacturing purposes, or for public baths or wash-houses.

The local board may maintain and supply with water all cisterns, &c., for gratuitous use, and construct such public works; and may also supply all *public* baths and wash-houses gratuitously.

Injuring water-works, or unlawfully diverting and appropriating water, shall be punished by fine; but existing rights to streams to be respected.

Any person bathing, washing, or throwing impurities of any kind, into any

water-works belonging to the local board, shall be punished by fine; or any gas or other manufacturer allowing refuse matter to injure water, to be subject to heavy penalties.

Interments and Burial-Grounds.—The local board may provide reception-houses for the dead, and make bye-laws for their regulation; and may also, on application, inter decently and economically any corpse placed in such reception-houses.

If, on representation from the local board, the General Board of Health issue and publish a certificate, to the effect that any burial-ground or vaults under a church are overcrowded with dead, so as to be dangerous to health, no future interment shall be permitted in them, except in specific cases mentioned in such certificate, under a penalty of L.20. The general board, before issuing the certificate, must direct an inquiry, notified to the bishop of the diocese, and publicly advertised, to be made by a superintending inspector or otherwise.

No interment shall take place in or under any place of public worship, built after the passing of this Act; and no burial-ground shall be made in any district after the passing of this Act, or shall be used for interment, unless on land purchased for the purpose before the passing of this Act, without consent of the general board, and under a penalty of L.50 and costs for each offence.

Rates.—The rates for carrying out the provisions of the bill are to be levied in the individual districts, under the authority of the local boards. All property liable to poor-rates is to be assessable. Unoccupied premises are not to be charged. Assessments may be levied on any portion of a district, for specific purposes. Whenever the local board has incurred expenses for the improvement of private property, it may levy a rate to repay principal and interest in thirty years, to be called a *private improvement rate.* It shall be levied on the tenant during his occupancy, and, when premises are vacant, on the owner. In all cases where the occupier is rack-rented, he may deduct three-fourths of the private improvement rate from the landlord, and in other cases in proportion. Rates may be reduced or omitted, in case of poverty. In cases of small tenements (not exceeding the annual value of L.10), or premises let to weekly or monthly tenants, the Local Board of Health may compound with the owners for the payment of rates at not less than two-thirds or more than four-fifths of the net annual value of the property as assessed; the occupier of the premises being also in this case made liable for the payment of the rates to the extent of the rent actually due by him.

General Superintendence.—The superintending inspector, during any inquiry for the purposes of this Act, may examine on oath any person whomsoever, and he may require public officers, poor-law guardians, or commissioners, to produce before him surveys, plans, sections, rate-books, and other documents, and he may take copies. A fine to be inflicted for resistance to his order. All persons attending an order of the inspector to be paid reasonable charges, and no person to be compelled to go further than ten miles from his house.

NUISANCES REMOVAL AND DISEASES PREVENTION ACT.

We have given in p. 323 of the present Number, an account of the principal provisions of this excellent enactment, which, we beg to remind our readers, has been already put in force in Great Britain and Ireland by a special Order in Council. We subjoin in full the form of notice, by which, under this act, any two householders can cause the authorities to make inquiry as to any nuisance, and, if necessary, ordain its removal.

"To the Town-Council of the Borough of
(*or* Guardians of the Poor of the
Union, *or* of the Parish of in the
County of *or as the case may be;*
or, in Scotland, to the Procurator-Fiscal of the County of

or to the Procurator-Fiscal or Dean of Guild of the Burgh of
or to the Procurator-Fiscal of the Justices of the County of
or to the Inspector of the Poor of the Parish of
or as the case may be.)

"We, the undersigned (Inhabitants (*or, in Scotland*, Householders) of (*insert the Parish or Place*), and residing at , in (*insert the Parish or Place before mentioned*) aforesaid, do hereby give you Notice, that to the best of our Knowledge and Belief a Dwelling-House (*or* Building) situate at No. in Street, in aforesaid (*or such other Description as may be sufficient to identify the Premises*), is in such a filthy and unwholesome Condition as to be a Nuisance to (*or* injurious to the Health of) *A. B., or as the case may be;* (*or* that upon certain Premises situate at (*inserting such a Description as may be sufficient to identify the Premises*) there is a foul and offensive Drain, Ditch, Gutter, Privy, Cesspool, *or* Ashpit, *or* a Drain, *&c.*, kept *or* constructed so as to be a Nuisance to us, *or* to the Occupiers of the Premises adjoining the Premises aforesaid, *or as the case may be; or* that upon certain Premises situate at, *&c.*, Swine, *or* an Accumulation of Dung, Manure, Offal, Filth, Refuse, *or* Matter, *or as the case may be*, are *or* is kept, so as to be injurious to our Health, *or* to the Health of *A. B., or* of the Occupiers of the Premises adjoining the Premises aforesaid, *or* of Persons living in the Neighbourhood, *or* of the Persons living in the Premises aforesaid, *as the case may be; or* that upon certain Premises, *&c.*, Swine, *&c.*, are kept so as to be a Nuisance to us, *&c., as the case may be*). And we hereby require that you will cause such Proceedings to be taken as are directed in this Behalf by the 'Nuisances Removal and Diseases Prevention Act, 1848.'

Dated this day of One thousand eight hundred and

"*A. B.*
"*C. D.*"

TRIAL FOR CHILD-MURDER BY POISONING WITH HEMLOCK—ALLEGED INNOCUOUSNESS OF HEMLOCK.

Hannah Bower, aged twenty-two, was tried at Ipswich, August 3, before Mr Baron Parke, on the charge of murdering her illegitimate child, about two years of age. She had never shown much maternal affection for the child. She had been heard frequently to express herself rancorously towards the child, and even threaten to kill her, as an obstacle to her living comfortably with a new paramour. A few days before the child's death, she predicted that she would not trouble her much longer, and even spoke of her intention to poison her with hemlock. On the day before the girl's death, she got an order for the Union doctor to visit her, though at that time, and up to two o'clock the next day, she was to all appearance well and happy. On the day of her death (May 1), the mother went into the fields to gather hemlock, with the help of a girl, who pointed it out to her, and to whom she said that the child's feet were sore. From her own confession, which, though rejected at first by the judge on account of some informality, was at length put in, it appeared that she was persuaded to kill the child by Glasscock, her paramour; that she showed him the hemlock she had gathered; that he said it was sheep's parsley, and would do as well as hemlock; that by his directions she boiled it, and gave half a cupful of the liquid to the child while it sat in the chair. At two o'clock on the day of its death, the child was seen playing about the cottage door, while the mother stood by, venting her usual feelings in regard to it. The child was then seen to run into the house; the mother followed, and gave her a crust of bread, and immediately she was heard to cry, and in about two minutes from the time of their entrance into the house, the mother came out with the child in her arms, and then it was evident that something serious had befallen her: "her eyes protruded, as if she was scared; her limbs hung

lifeless about her body, and all her muscular powers seemed to have been in one instant paralysed." The mother carried the girl to a doctor, saying that she had bowel complaint, for which he prescribed. She went through the form of giving the medicine prescribed; but the child continued to droop, and died in about an hour from the first occurrence of the symptoms. It does not appear from the report whether any examination of the stomach was made before the body was buried; it is stated, however, that the contents of the stomach were thrown away, so that no medical opinion was obtained in regard to these; the body, however, was exhumed, and the stomach and viscera sent for examination to Mr Taylor of London, and these appear to have been examined also by some medical gentlemen at Harwich. No evidence of the presence of poison was found by any of these parties. Mr Taylor, at the trial, said that the absence of vegetable matter both from the stomach and viscera was not conclusive against the administration of a poison decocted from plants; for it was possible, though not probable, that the fluid might have been carefully strained, or skimmed with a spoon, and that so none of the vegetable particles would pass into the body of the deceased. Mr Taylor further spoke to the symptoms which would follow upon the administration of hemlock as being analogous to those detailed by the witnesses who saw the child carried by the mother to the doctor, and at the time of her death. Mr Martin, however, one of the medical men who had examined the stomach at Harwich, expressed his doubt whether hemlock was of so dangerous a quality as was supposed by many chemists; and stated that he had administered a decoction, made from a quantity of that plant—about equal to that which the prisoner was supposed to have employed—to a rabbit, and that the animal had not suffered the slightest inconvenience from the dose. He also deposed to having been informed by a gentleman last night, that he had been in the daily habit of eating the leaves of the hemlock plant for a considerable time past, and that he had not experienced any bad effect from such treatment.

The jury found the prisoner not guilty, adding, "we entertain a doubt, and give her the benefit of it."—*Times*, August 7, 1848.

We cannot but feel surprised at the verdict in the above case. There does not appear to have been any legal difficulty in the evidence. The presiding judge was plainly satisfied that it was quite competent for the jury to bring in a verdict of murder against the prisoner. What the doubt was, the benefit of which the jury gave to the prisoner, is not stated. The report of the trial is not explicit enough to enable us to discover if the poison, of which the child died, was really hemlock. It seems certain that death took place in consequence of the administration of a poison, supposed by the mother to be hemlock, and that the drug was administered for the purpose of taking away the child's life. In trials for poisoning, the necessity for proving the species of poison employed is not absolute; this proof is often rendered imperative, only because in most cases without such proof the fact of the poisoning cannot be established. Here the proof of wilful poisoning is complete, whether the poison were hemlock, or some of the poisonous plants which resemble hemlock. There is no room for doubt, unless the indictment was so drawn as to exclude from the charge every other poison but hemlock; and, had this been the case, the judge would hardly have left it to the jury, of their own motion, to acquit the prisoner on this score. But most probably the doubt was raised in the minds of the jury by the evidence of Mr Martin, one of the medical witnesses. Mr Martin says he doubts if hemlock be of so dangerous a character as was supposed by many chemists. We know that inaccuracies are apt to creep into the reports of medical testimony in courts of justice, and we should be sorry, on the mere ground of a newspaper report of his evidence, to arraign Mr Martin for ignorance of what should be known by every medical witness. But Mr Martin owes it to himself, and to the profession, to publish what he

said, if the report be inaccurate. We cannot conceive that any medical man could have glanced over the article hemlock, in so accessible a book as Taylor on Poisons, without feeling distrust in his own single small experiment on a rabbit, and in the testimony of a friend, so eccentric as to prefer hemlock with his morning bread and butter to water cresses. The poisonous qualities of hemlock admit of no doubt; and, since the publication of Dr J. H. Bennett's well reported case, no room has remained to question the identity of the common hemlock of our hedges with the Athenian state poison by which Socrates suffered death.

TRIAL OF JAMES M'WHEELAN, OR M'QUEEN, FOR MURDER, AT AYR.

The interest of this case lies in the conclusiveness of purely circumstantial evidence.

The body of James Young, a young man of sixteen years of age, the son of a farmer, and farm-servant to a neighbouring farmer, was found on the road near his master's house, about four o'clock in the morning of May 27th, cold and stiff, with a wound in the neck, and two pools of blood close by, one near the head, another about a yard distant. The pockets were empty, and seemed to have been turned out; there was no watch, watch-chain, or guard on the body,—beside it there was a bundle containing a pair of new trousers. On the other side of the hedge, twelve yards from the body, was found a chisel clotted with blood, both on the blade and handle, and beside the body there was a stone, different from the stones around, about the size of a large potato; it was cleaner than the stones around, being free from dust, and had a sweaty appearance, as if it had been carried in the hand. On the same morning, about a mile nearer Kilmarnock than the place where the body was found, or nearly two and a half miles from that town, a handkerchief was picked up from the road-side, with marks on the road beside it as if a person had sate there. On the evening before the deceased had been with his father and elder brother, and had left his father's house some time before eleven o'clock, to go to his master's, a distance of about two and a half miles—and he then had his watch, watch-chain, and guard, and there was strong reason to believe he had some silver in his pockets, the remainder of his wages, which he had received not long before.

About eleven o'clock on the same morning on which the murdered body of the deceased was found, the accused applied to an acquaintance in Beith to pawn a watch for him—which he did, and brought to him the proceeds, namely, one pound. This watch was proved satisfactorily to be the watch of the murdered man—the watch which he had in his possession late in the evening before he was found dead on the road. It was proved by the evidence of a barber, that an hour before, on the same morning, the accused had come to be shaved, and had desired his whiskers, which were very large, and also the hair below his chin, to be removed—the same barber had shaved him several times before, and but a small part of his face around his mouth had then been shaved—the shaving on the morning of the 27th May, made a considerable change in his appearance—the barber remarked to the rest of his customers, as M'Wheelan went away, that he had a strange and startled look. The evidence of the barber was confirmed by that of a surgeon, who had seen him that morning both before and after his whiskers and superfluous hair had been removed. The accused was traced most distinctly from Beith backwards to Kilmarnock, a distance of about fourteen miles, in the course of the morning of 27th May—at ten o'clock he was seen entering Beith by the Kilmarnock road; at nine o'clock he was seen two miles from Beith on the same road—at six o'clock in the morning he was at Dunlop, five and a half miles from Beith—at three o'clock in the morning he was seen by a person who walked for some distance along with him from the toll-bar at the end of the town of Kilmarnock on the Beith road. Between twelve and one o'clock A.M. he overtook a person in Kilmarnock who was walking smartly home in the direction leading from the Ayr road, the

accused coming up behind at a quick pace; he invited this person, with whom he claimed a slight acquaintance, to join him in a glass of whisky—accordingly they adjourned to a spirit shop, and, while so engaged, his companion remarked that there was blood on the sleeve of his left arm, saying, "You have surely been murdering a man, or killing somebody's swine;" and when the accused turned round he saw a large stain of blood on his left knee, as if it had been dipped in blood—the accused said that he had had a quarrel about a lass, and, drinking off his glass, went quickly out; his companion waiting a little in expectation of his return—but he never came back. The handkerchief discovered on the road from Kilmarnock to Ayr on the morning of the murder, was not certainly identified as belonging to the accused; but several witnesses deposed to his having had in his possession, a few days before, a handkerchief exactly like it—and one who saw him with a handkerchief on the morning of the 26th of May, said, that if he had found that handkerchief after the prisoner left the place where he was, he would certainly have suspected that it belonged to him. It was, moreover, proved that the prisoner was carrying about with him, some days before, a chisel exactly like that which was found covered with blood near the place of the murder. He had at the same time, about his person, a pointed steel rod, termed a widener, which was found on him when apprehended—and when asked, some days before, as to the use he intended to make of these instruments, he used some mysterious expressions, intimating, that if the one did not suffice, the other would. On one occasion, some days before the murder, a miner took both the instruments into his own hand, examining them, and at the trial he identified the chisel found beside the dead body as that which he had taken from the prisoner's hands for examination on that occasion; he then remarked a spot of rust on it, which spot he afterwards observed on the chisel when first exhibited to him by the authorities—but at the time of the trial that spot of rust was no longer there. The prisoner left Beith in the afternoon of the 27th by railway for Johnston, but stopped at Lochwinnoch, and on the following day, Sunday the 28th, he was apprehended on a charge of robbing on that day the toll-house at Lochwinnoch—a sum of money being found on him corresponding to that missing at the toll-house. The judicial declarations made by the prisoner were altogether at variance with the distinct testimony of numerous witnesses on the trial; he denied that he had disposed of a watch, and declined to explain where he got the money which he acknowledged he had on the day the body of the deceased was found; he denied, also, any knowledge of his clothes having been stained with blood, and that he had attempted to wash stains of blood from them. Besides the testimony of the witnesses who had seen the accused before his apprehension, and of Dr Haldane and a washerwoman who had inspected them after his apprehension, a minute report as to the presence of blood-stains was presented from Professor Rainy of Glasgow. The chief points brought out in exculpation were, that the watch was pawned in his own name; that the guard-chain known to have been attached to it was nowhere discovered; the assertion by two females, that there were blood-stains on the prisoner's clothes three weeks before the murder, and that they had assisted in removing them with whiting; the testimony of a boy, that he had, in the beginning of May, given a cast in his cart at the time it contained slaughtered calves, to a man he could almost swear was the prisoner; and the testimony of a boy and a man, that they had seen a person who was not the prisoner, at nine o'clock on the morning of the murder, about three miles from the place where the body was found, with blood upon his vest—the man did not see the blood, but said that the boy spoke to him of it at the time—they did not then know of the murder; they saw him twice within twenty minutes, first going towards the place of the murder, and then in the direction from that place. The jury unanimously found a verdict of guilty, and, after sentence of death had been pronounced, the prisoner said, "I got the watch, though I never said it before, from a man named Hall, in Kilmarnock."

If any thing were wanting to prove how perfectly circumstantial evidence, when distinctly brought out and well supported, may demonstrate guilt, this case supplies the deficiency. The common objection to circumstantial evidence as the sole proof of guilt, applies only to the manifest hazard of injustice from the effect of mere *prima facie* circumstances of guilt on the minds of an uneducated, and perhaps prejudiced jury. But where, as in the case before us, every circumstance is well attested, and all the circumstances are distinctly connected together into one whole, we confess we should prefer such proof to any ordinary amount of direct testimony unsupported by decisive circumstances. In well-supported circumstantial evidence, there is even much less hazard of being led astray by conspiracy, popular excitement, or error of identity, than in ordinary direct testimony. Direct testimony, under improbable circumstances, must be rejected ; the same, unsupported by circumstances, is to be suspected; but let the circumstances be clear, connected, and sufficiently numerous, then the proof is as convincing as the proof of loaded dice from the recurrence of the same throw many times without interruption. Chance can do much, but there is a limit beyond which it cannot produce a uniform effect. In the case before us, the possession of the murdered man's watch within a few hours of the murder, and these hours the hours of the night, was a most suspicious circumstance, especially when coupled with the prisoner's solemn denial of ever having had such a watch in his possession. But he might have got the watch in the night from a person whose character he knew to be bad, and might have hoped to avoid getting into trouble by denying all knowledge of the watch; or the murder might have been the work of another from revenge without robbery, and the prisoner might have stumbled on the body in his wanderings, and taken the watch and money, and been afraid to avow what he had done. But the chisel which fitted the wound, found near the spot of the murder, was proved to have been in the prisoner's possession; it was identified by the spot of rust; but chisels being made of many sizes, even if no more than its close resemblance to that before seen in his possession, had been proved, what more could have been wanted ? A man is found possessed of a murdered man's watch a few hours after the crime ; he is proved to have been travelling about with an instrument, not belonging to his trade, exactly resembling the instrument of the murder, along with another weapon; when he is apprehended, the other weapon is discovered on him, the chisel is not in his possession. Can accident produce such effects as these ? Can direct testimony make a case stronger than this, even when the numerous corroborating points established in this trial are left out of view ? The presiding judge remarked on the zeal, industry, penetration, and efficiency displayed in procuring evidence in the case of M'Wheelan. This, doubtless, is particularly requisite in a case, in which the expectation of a conviction is to rest solely on circumstantial evidence. Here the mode of procedure in criminal matters practised in Scotland, namely, by private investigation under the authority of the Lord-Advocate, as chief public prosecutor, has peculiar advantages over the more cumbrous proceedings in England and Ireland. And it may be confidently predicted that, under the present system, convictions for crimes will become more and more difficult in these parts of the United Kingdom, simply because, as criminals become more knowing in the actual proceedings of courts of justice, the evidence of crimes will become by degrees reduced to a more and more refined evidence of circumstances. We are indebted for the particulars of M'Wheelan's trial to the *Ayr Advertiser* of October 12th, which contains a report very different from the common meagre accounts of justiciary trials, so disgraceful to our Scotch newspapers in general.

THE CHOLERA.

Since our last publication, the cholera has appeared in this country. The first cases occurred in Hull, and were said to have been brought by a vessel from Hamburgh. On the 1st October a case was reported in Calton Street, Edinburgh;

on the 4th other cases occurred, and the disease afterwards continued to make steady, though not rapid progress. In Woolwich, two cases occurred on the 7th, and three more on the 8th instant; almost immediately afterwards the disease was announced in various parts of London, chiefly in the neighbourhood of the Thames. Uxbridge was affected on the 13th; Gravesend on the 15th; Birmingham on the 16th; and new cases continued to appear in the metropolis and in Woolwich. We must leave it to the newspapers to detail the number of cases in each of these localities. The last information which we have received indicates a renewed outbreak at Hull, and the occurrence of cases at Sunderland, York, Plymouth, Ware, the Isle of Portland, Bedford, and other places in England. The authority for several of these, however, is not stated. In London there has been for some days a diminished number of cases; in Edinburgh, also, there was for a few days a lull, but for two days past increased numbers have been again reported. The total number ascertained is already above 200. The fatality of the disease has been every where very great.

These circumstances have somewhat eclipsed the interest attaching to the news of the progress of the epidemic in other countries. We learn, however, that its virulence has declined at Alexandria, Cairo, and St Petersburg. On the other hand, it has broken out in a severe form at Smyrna, and other places in Asia Minor. In Europe it has attacked Lubeck, Amsterdam, and Königsberg, and continues to rage at Hamburg and Berlin. A case was declared in Paris, but this was officially contradicted.

We may now, therefore, consider this disease as certainly settled among us in an epidemic form; and it becomes the medical profession, as the real guardians of the public health in this country, to consider by what resources, and in what frame of mind it is to be met, and, as far as possible, averted. The weekly medical and daily non-medical journals teem with the most opposite opinions, in reading which it is impossible to avoid the humiliating conclusion, that, in regard to many most important practical points, we are in a state of ignorance quite as great as during the former epidemic. We think it would be well if medical men, instead of submitting to the public their hasty theories and crude experience on this subject, *as individuals*, would, at this juncture, try to discover those principles which result from the united experience of all, as regards the former epidemic.

In some points of view, we think we may fairly say that there is an improved condition of the public mind since 1832. The vague but intense alarm which preceded the former visitation of the epidemic, has been exchanged for a more calm and rational view of the real amount of danger. We are far from coinciding with the spirit of one of the daily journals, which, in a series of leading articles, of a characteristically impulsive and reckless character, peculiarly calculated *ad captandum vulgus*, has attempted to over-ride at once facts and public opinion, and to get up an impression that the cholera is really a disease of very slight danger, as compared with other epidemic visitations. We think, however, that we are well rid of that species of alarm, which, during the last epidemic was partly, perhaps, fostered by the absolute ignorance, even of medical men, as to the phenomena of the disease, and which frequently led to the mistake, in imagination, of the slightest and most trivial disorders for the dreaded malady.

There is no doubt that one of the causes of this improved state of public feeling, is the extent to which the opinion has been gaining ground, that the cholera is only slightly, if at all contagious. That this opinion is scientifically well founded, we have some months since expressed our strong conviction (No. for March, p. 675); nay, we have not scrupled to maintain that the weight of evidence is in favour of the view, that cholera is not under any circumstances contagious. We return to the subject of contagion now, not with the view of re-asserting dogmatically our own opinions, still less with the intention of receding from them; but in order to consider the subject in relation to the practical duty of the medical man, when called upon to give advice in matters of

action. That the public should be guided by the common sense and practical judgment of the reasonable majority of the profession, instead of by the discrepant views and clamorous assertions of extreme parties, will, we think, be generally admitted to be highly desirable. As journalists and interpreters of the public voice, we are sincerely desirous to aid in the establishment of such general principles of action as may, without the sacrifice of individual opinions, become serviceable to the cause of union.

A scientific principle or theory is a good thing, and we like to see it strongly and unreservedly stated as the result of profound conviction. But in the face of an actual emergency, and with probabilities only, not certainties, to deal with, our actions must be modified by considerations of expediency and practicability, as well as by the possible consequences of our being in the wrong. Hence we hold that the non-contagionist and the unqualified contagionist are both grievously in error, if, in spite of the obscurities of the subject, and the manifest dictates of expediency, they are willing to push their opinions in action to the utmost limits of possibility. We should consider it inexcusable and cruel in the latter to confine his patients and their attendants, and surround them with an inviolable quarantine; but, in like manner, it would be monstrous in the former, however strong his convictions of the non-contagious nature of cholera, to allow the healthy and the diseased to sleep together, or to be placed in any other circumstances unusually fitted for the development of contagion. Both must act with a due regard to circumstances; and must not attempt to enforce unbearable restrictions, or make perilous experiments, simply to carry out an individual and a disputed opinion.

We cannot deny that, in relation to cholera, the errors which flow from the exclusive and extreme contagionist theory seem to be more pernicious than those of the opposite. The former have a tendency to generate excessive alarm, to repress the charities of private life, and to destroy the comfort both of the healthy and the sick; and by thus impairing confidence and arousing fear, to create more disease than they avert. But whether this be the case or not, it is certain that the opinions of the extreme contagionist have now ceased to be held by any influential portion of the medical public. The difference in this respect between 1848 and 1832 is very marked. At the period of the former epidemic, Dr Christison had reason to complain, on the part of the Board of Health of Edinburgh, that the members of the London Board seemed to have been selected as being "unconditional contagionists;" and the acts of that Board were such as to impress the public with a most immoderate fear of contagion. At present, on the contrary, the London and Dublin boards are composed of non-contagionists; and the instructions of the London Board are evidently pervaded by the same spirit, as that which dictated the report of the Metropolitan Sanitary Commission on this subject.

Apart from all exclusive theories, we conceive that the practical part of the question as to cholera and contagion must be settled in a very similar manner, upon the data now before the public, by all reasonable men. Few will deny that the contagiousness of cholera must be very slight as compared with other epidemic diseases, seeing that, as an ordinary rule, the attendants of cholera patients are not attacked in any considerable proportion, unless placed in circumstances which, independently of contagion, are such as to pre-dispose strongly to the disease. On the other hand, it may be readily admitted, that there have been adduced, in individual cases, evidences of contagion so apparently strong as to make the non-contagionist pause long, before acting upon his theory to such an extent as to involve seriously the chances of human life. Indeed, so strongly do these circumstances operate along with the prejudice in favour of an error on the safe side, that neither the London nor the Dublin Board of Health, although evidently under the influence of the idea of non-contagion in their instructions to the public, has ventured to enunciate this principle without a reservation. Accordingly, the documents issued by both these Boards, but especially the former, present inconsistencies

calculated to detract very much from their usefulness. The public are told in the London document, that the fact of cholera spreading from person to person in close unventilated apartments, "is not to be considered as affecting the general principle of its non-contagious nature;" thus admitting in reality the whole principle of contagion in the very act of enunciating its opposite. The Dublin instructions, though framed entirely upon the non-contagionist view, admit that the disease may be "rarely" contagious.

The document containing the resolutions of the Edinburgh College of Physicians, has avoided the error of inconsistency by the simple method of maintaining a neutral position on the abstract question of contagion. It is indeed evident that, practically, the difference is insignificant between a non-contagionist *with a reservation*, and a rational and moderate contagionist; and that, therefore, no practical end would have been gained by engaging in the dispute on the abstract principle. If it be true, as we think almost every one must now admit, that the contagion of cholera is so limited as not to form an argument for encountering the great risk incurred by the removal of patients from their homes, unless under peculiar circumstances of poverty or destitution, then we cannot see how any real difference would be effected by the fullest assertion of the non-contagionist theory, unless its reckless use were to throw the public or medical attendants off their guard as to those minor hygienic regulations which, whether contagion exist or not, are of the utmost importance to the safety of the community.

We think, therefore, that those of our body who are really anxious that their advice should be useful to the public, and whose position is such as to cause their expressions to be carefully scanned, should in the present juncture avoid the dogmatic and unnecessary assertion of extreme theoretical opinions, and content themselves with allaying exaggerated fears, and avoiding rash and dangerous neglect of those precautions which are equally requisite on either theory.

We subjoin the portions of the Edinburgh document which relate to the subject of contagion. The College declare it as their opinion:—

"That it is not advisable to remove cholera patients from their own houses, if it be possible to command advantageously the means of treating them there; but that, according to experience in the former epidemic, a large proportion of cases must occur in Edinburgh among the lowest population, inhabiting apartments in which it is impossible to treat the sick for want of proper beds, bedding, fires, and other means of heating the body, besides other necessary medical resources."

"The College of Physicians also passed an unanimous resolution, strongly approving of the establishment, as in 1832, of houses of refuge, for the removal of the healthy from localities threatened with a severe visitation of the epidemic. This measure was carried through energetically by the Edinburgh Board of Health during the first epidemic; so that, at one period, upwards of 700 persons, from infected districts, were lodged under observation in houses of refuge, but allowed to go to their usual places of work. There is no doubt, according to the opinion of the best judges, that the ravages of the disease were in that way greatly circumscribed in various localities in Edinburgh; and that no other measure contributed so much to keep the epidemic within moderate bounds. The utility of such houses of refuge must be obvious to all, whether the disease be viewed as originating in infection, or, as seems now far more probable, in a peculiar miasma, prevailing with intensity only in limited localities."

SANITARY PRECAUTIONS AGAINST CHOLERA.

Immediately after the passing of the Nuisances' Removal Act on the 4th September, it was put in force for the period of six months by an Order in Council. Some delay, however, took place in the formation of a Board of Health, which was not finally constituted till the 22d September. The General Board of Health for London is composed of the Earl of Carlisle (late Lord Morpeth),

chairman, with Lord Ashley and Mr Edwin Chadwick, to whom has been added, under the temporary act, Dr Southwood Smith as a medical member. This constitution of the Board of Health, by which a medical member is included as it were by accident, and only in the time of an epidemic, has been severely and most justly criticised. We are by no means disposed to claim for the medical profession too much power or influence as regards sanitary reform; but where a central Board of Health for the kingdom is constituted, without the admission of some of the profession as permanent members of that board, we think that the public interest must inevitably suffer, not only by the mistakes of men in office, but by the indirect action of such a board on the temper of the profession. Medical men, so far from being in general over-anxious to increase their influence by occupying a prominent position among public bodies in the work of sanitary reform, have in our opinion been neglectful to an undue extent of their legitimate influence, and have exposed themselves, not unjustly, to the charge of apathy and criminal neglect. While we admit and lament their deficiency in this respect, we must say in palliation, that when a central hygienic body can be constituted without being necessarily guided by medical opinion, or recognizing sanitary matters as subjects on which medical opinion is indispensable, it is too much to expect that practitioners shall withdraw themselves from the line of their more strictly professional labours, to study a subject in which they can have no interest but that of an enlightened patriotism.

We are happy to find that, notwithstanding these defects of the central legislation, the civic authorities of the city of London have shown a disposition actively to assist in the good work; and that a medical officer for the city has been appointed, in the person of Mr John Simon.

So soon as the existence of cholera in this country was communicated to London, the Board of Health sent Dr Sutherland and Mr Grainger to Hull to investigate the cases which had arisen there. On the breaking out of the disease in Edinburgh, Dr Sutherland was commissioned to proceed hither, while Mr Grainger went to Hamburg to report upon the epidemic on the Continent. A Board of Health was soon nominated in Edinburgh, which proceeded immediately to act, and would have continued to do so, had it not been discovered that, by the provisions of the act, the General Board had no authority to delegate its powers except to the parochial boards, and other already existing bodies. It is to be regretted that, in this way, much valuable time was lost, so far as the public interest was concerned. Instructions, however, have recently been transmitted to the parochial boards, of which it is to be hoped they will energetically avail themselves. In the mean time, we regret to learn that the excellent proposition, originating with the Sheriff and Lord Provost of Edinburgh, to combine the different parochial boards into a single working body for the benefit of the city at large, has been frustrated in the present case, as in that of the fever last year, by the determination of the West Church parochial authorities to stand aloof. Considering that the West Church parish embraces so large a portion of Edinburgh, we cannot too much regret the danger which thus arises, that some of its poorer inhabitants may, when afflicted with cholera, and requiring immediate relief, be at a loss to what quarter to proceed. We must also regard this unfortunate occurrence as a strong argument in favour of a consolidated system of sanitary administration for our large towns, instead of the present mode of parochial supervision.

We are happy to learn, however, that under the police authorities, both of Edinburgh and Glasgow, measures have been organized under the new police act, which, though late of being put in operation, may yet have the effect of resisting the progress, and diminishing the fatality, of the epidemic.

A quarantine, which was for a time imposed on the shipping in the port of London, has been abolished since the 17th inst. by an order of the Privy Council.

BOOKS RECEIVED—next Month.

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*Observations on Gunshot Wounds, made in Paris during the Summer of* 1848. By Edward Waters, M.D., F.R.C.P., Edinburgh, formerly Senior President of the Royal Medical Society of Edinburgh.—(Continued from p. 316.)

TREATMENT OF FRACTURES OF THE EXTREMITIES IN CONNEXION WITH GUNSHOT WOUNDS.

c.—Cases demanding Amputation.

In nearly all times, civil and military surgeons have been at variance as to the necessity of amputation in certain cases of gunshot wounds, the former maintaining that many limbs could be saved which were ordinarily sacrificed; the latter, on the contrary, holding that such attempts could only be made at a great sacrifice of life. The vast experience afforded by the wars of the Empire; the writings of Larrey, Percy, Guthrie, and a host of military and naval surgeons of acknowledged merit; the prominent positions many of these surgeons have occupied as teachers during the long peace that has succeeded, rendered their opinions dominant in the schools; theoretical conclusions were obliged to give place to facts. Dupuytren, whose opportunities for observing these wounds, after the attack of Paris by the allied armies, after the days of July 1830 and June 1832, were very extensive, ended by inculcating in his lectures the same rules of action.

Since the revolution of February, and the combat of June 1848, the question has been re-opened; surgeons advocating the attempt to preserve limbs which, in accordance with the rules alluded to, should be condemned, have again come forward. On the other

hand, the disciples of what may be termed the military school, have had their confidence in their practice confirmed by the results of their recent treatment. The facts on which the former ground their arguments are not yet fairly available; the results of their attempts, except in as far as regards the mortality which has hitherto attended them, cannot now be ascertained. How far the limbs preserved may be useful, or a source of annoyance and torment, it is impossible at present to decide.

An appreciation of the merits of the two modes of practice, founded on an observation of the wounded in the late events, while the results are thus incomplete, is necessarily open to the charge of being premature and imperfect; still not to have formed some estimate of their relative value, after an attention of so long a period given to their study, would have been impossible. The development of the conclusions arrived at will now be entered on, and a few cases cited in illustration; a detail of the numerous facts observed, which bear upon the subject, would extend beyond the limits assigned to this communication. It is premised that it is not intended to establish any absolute rules of proceeding; circumstances, which the tact of the surgeon can alone estimate, may justify an exceptional course in particular cases.

M. Jobert insists on the attempt to preserve wounded limbs in all cases of fracture, where, in addition to the injury to the bone, the disorganization of the soft parts is not such as to render it hopeless in cases even where articulations have been penetrated. M. Malgaigne inculcates the same practice, with the exception that he considers amputation indispensable where the hip or knee joint is opened. The practice of MM. Ph. Boyer, Gosselin, and Laugier, has been governed by the same conservative principles. M. Velpeau, by his powerful authority, encourages to a certain extent the like practice. He stated before the Academy: "The more I see the less I amputate"—(*Plus je vois, moins j'ampute*). In February, he amputated less than he had previously done, in June less than in February; and he believes that, should similar cases again present themselves, he would amputate still less than in June.

On the other side, the long experience of M. Roux has convinced him that the safety of the patient requires, in regard to amputation, the adoption of the practice of the military school, and many eminent surgeons agree in entertaining the same opinion. Their conclusions possess greater weight from their having witnessed and compared the results of the two methods of practice. MM. Roux, Nélaton, and Baudens essayed the preservation of the fractured limb in several cases, while the exclusive supporters of the conservative practice pursued only the one method.

In considering the cases which demand amputation, the necessity of separating the lesions of the upper and inferior extremities is too obvious to need comment, and the examination will be in a still

greater degree facilitated by treating the lesions according to the parts implicated.

Wounds of the Hip-joint.—M. Malgaigne, whose disapprobation of primary operations has been already noticed, considers penetrating wounds of the hip-joint a case for disarticulation. M. Baudens has stated, that if a lesion of the head or neck of the femur were such as to permit him to choose between excision and disarticulation, he would decide for the former. No such opportunity has presented itself, the operation does not appear to have been ever tried in connexion with gunshot wounds; and, so long as its advantages are thus unproved, disarticulation must be laid down as the general rule in all cases of comminuted fracture of the femur at too high a point to admit of amputation. The high mortality of an operation is no valid objection to its performance when it forms the only resource of the surgeon.

Four cases of disarticulation at the hip-joints have resulted from the events of June; one of the patients, an insurgent, aged twenty-nine, operated by M. Nélaton, has survived. Of the other cases, M. Baudens performed one on the person of a young soldier; he never completely recovered the shock caused by it, and died the second day;[1]—another occurred in the service of M. Richet at the Tuileries; the patient survived ten hours. The remaining case presented itself at the Hôpital Beaujon, in the service of M. Robert; the patient died during the operation. The particulars of the observation were communicated to the Academy as bearing upon the question of death under the influence of chloroform. This agent was administered in all the cases.

It might be argued that, in at least one of these instances, death was not attributable to the operation, but, however high the mortality, it is more than equalled by that which followed the attempts at conservation; one case died in the service of M. Nélaton whilst he was performing the operation of disarticulation recorded, and all the cases of comminuted fracture of the femur, involving its neck or the situation of the trochanters, which have fallen under the writer's observation, have sooner or later had the same termination.

The following remarkable case is not considered as belonging to the category absolutely demanding disarticulation:—

Joyerot, aged thirty-seven, pavier, married, admitted into the *Ambulance de la Salle des Concerts*, under M. Blanchet, was transferred, July 11, to the service of M. Malgaigne. A musket-ball had penetrated the thigh posteriorly at the fold of the nates, and issued anteriorly an inch and a half below Poupart's ligament, fracturing the neck of the femur in its course. No direct examination of the fracture was made; but it appears to have been simple. It was treated by the double inclined plane; no splinters had been discharged nor

[1] The application of ice after capital amputations seems to be contra-indicated. Its remarkable sedative effect on the pulse has been already mentioned; and it is easy to conceive that, when added to the shock which follows these operations, it may in many instances exert a very prejudicial influence.

removed, and on admission into the wards of M. Malgaigne, now twenty days after the infliction of the wound, both orifices were healed, and the fracture united by provisional callus. As shortening to the extent of nearly an inch existed, the long external splint of Desault was applied to exert extension. No action supervened, and he has been allowed to leave the hospital.

Severe lesions of the femur, in the immediate neighbourhood of the articulation, if not attended with solution of continuity, do not call for the immediate sacrifice of the limb. The following cases support this conclusion :—

Housset, *garde mobile*, aged nineteen, of good constitution, was struck by a musket-ball on the 23d June, immediately below the great trochanter, on the external surface of the femur; the ball passed into the substance of the bone to a considerable depth in a somewhat backward direction, causing a fissure of some inches in length, extending from the great trochanter obliquely downwards, inwards and forwards, and nearly, but not completely, causing actual solution of continuity. A military surgeon attempted the extraction of the ball in an *ambulance* without success. The following day the patient was admitted into the Hôpital de la Clinique under M. Giraldès ; inflammation had then set in, and the position of the ball could not be ascertained. Thinking it might be lodged in the soft parts, where its presence would disclose itself at a later period, the exploration was not persisted in ; warm poultices were applied, and the limb laid on a double inclined plane. *October* 3. Progress to cure unsatisfactory; callus has formed to some extent, but the external wound remains open, and suppuration continues ; some small portions of necrosed bone have been latterly removed. M. Giraldès placed the patient under the influence of chloroform, and effected the extraction of the ball with the lever after dilating the external wound, breaking up a portion of the callus, and removing several necrosed splinters. The operation was a tedious and difficult one, and strict injunctions were given to watch the patient, and combat actively any symptoms of inflammation which might show themselves in the neighbouring joint. None such arose; the removal of the foreign bodies was a source of immediate relief to the patient, and the next morning he was reading tranquilly, and expressed himself free from suffering.

These particulars show the benefit conferred by the removal of extraneous substances, even when necessitating a severe operation. The following case presented itself in the service of M. Baudens :—

Meusy, officer in the army, wounded in the fight of June, had the great trochanter shattered by a musket-ball; all the splinters were immediately removed, and the case has progressed uninterruptedly towards cure.

Guthrie considers that amputation may be delayed with advantage in fracture of the lower part of the thigh-bone, and states that recovery not unfrequently follows. Schmucker, so favourably referred to by the late Professor Thomson of Edinburgh and Mr S. Cooper, recommended the same course. The injury produced by a musket-ball is here, owing to the less compact structure of the bone, small, compared with that which is generally produced when the central part of the shaft is implicated; and the difficulties opposed to the co-aptation of the fragments are far less than when the fracture exists in the neighbourhood of the trochanters, where the bone presents an analogous structure. M. Baudens, however, maintains the necessity of primary amputation in this as in all other

cases of fracture of the femur. It may reasonably be concluded that neither course should be exclusively followed; that the surgeon should be guided by the amount of injury, by the number and size of the splinters, and consequent loss of substance. When these are not too considerable, success may crown the effort to save the limb; in the contrary case amputation is demanded.

A printer, aged nineteen, of good constitution, admitted into the service of M. Nélaton, June 24. A musket-ball had traversed the thigh at its inferior fourth, and fractured the femur in its passage. The splinters of bone were immediately extracted, the loss of substance was not great, and slight dilatation of the orifice of entry was alone required. The apparatus of Desault, modified according to Boyer, was applied, without the too frequent accompaniments of compresses, many-tailed bandage, and cushions; the splints were slightly padded, and the limb was left visible throughout its whole length. Slight effusion occurred within the knee-joint, which was speedily absorbed. *September* 25. Consolidation of the fracture complete, a large amount of callus formed, the orifices of the wound healed, and the patient free from all uneasy feeling.

This case affords an example of the benefit of the immediate removal of all splinters; the dilatation so much dreaded by M. Jobert caused no ill consequences.

Leforestier, aged thirty-three, *chef de bureau*, of good constitution, unmarried, wounded the 24th of June, and admitted the same day into the service of M. Giraldès. The projectile, fired from a musket, had entered the thigh anteriorly between four and five inches above the knee-joint, and fractured the femur in this situation; the posterior tegumentary surface presented an opening by which a rolled mass of lead, about an inch and a half long, was removed; this orifice was supposed to have been made by the exit of a separated portion of the projectile, or by a splinter of bone. The primary splinters were extracted during the first days, and for a fortnight the limb was put up in the ordinary Parisian apparatus of three splints, cushions, many-tailed bandage, and compresses; the dressings were removed every day, and for some time twice a day. M. Giraldès then applied the gutta percha moulded to the limb, a valvular opening being practised, corresponding to the posterior orifice for the evacuation of the discharges.[1] *October* 2. Orifice of entry healed, that of exit still open, and giving issue to a slight amount of suppuration. Union of the fracture is effected by the deposition of a large amount of callus. M. Giraldès anticipates a perfect cure in this case, after the elimination of some splinters which still exist. There is shortening of the limb to the extent of fully $1\frac{1}{4}$ inch. The patient is free from fever, eats and sleeps well. He states that, when five years old, his leg was fractured at the same place, but that the cure was completed without the slightest deformity.

The conduct to be pursued in comminuted fracture by gunshot wound of that portion of the femur where the medullary canal is fully developed, and where the bony substance is remarkable for its exceedingly dense structure, is a question of the gravest interest, and continues, at the present day, to be matter of controversy. The solution of this question must depend mainly on whether, after such an accident, the realization of a useful limb be possible, and, if possible, on the rate of mortality by which such cases are pur-

[1] After a trial of some days of the ordinary apparatus, M. Giraldès adopted very generally the employment of the gutta percha.

chased. It is necessary that the limbs saved should be at least as useful as artificial ones; but it is not to be required that the amount of life sacrificed to their preservation should be as low as that which follows amputation. Where a patient has to choose between a dangerous operation, involving the positive loss of a limb, and a somewhat greater danger, with the hope of retaining it, there can be little doubt that he would prefer the latter. In computing the comparative success of the conservative and amputating practice, it may therefore be justly urged, that greater weight should be assigned to cases where, in addition to life, a useful limb is preserved, than to those where life alone is saved, and the limb amputated. If, however, it should appear either that the ratio of deaths, which follows the attempt to cure this kind of fracture, is disproportionately greater than that which follows the sacrifice of the limb; or if, in the cases where life is not lost in the attempt, the preserved limb is very rarely found to be other than a cause of embarrassment and uneasiness, amputations must then be held to be as a general rule imperative.

Gunshot fracture of the portion of the femur under consideration, is almost always accompanied by large splinters, and destruction of a considerable portion of the bone; in many cases, fissures extending into one or other of the articulations have been observed in connexion with them. Under these circumstances, consolidation cannot be otherwise than an uncommon event, and a considerable proportion of the cases in which it is sought for, are found to terminate fatally. These facts are not denied by those who endeavour to obtain it; M. Malgaigne freely admits them, and his defence of the treatment is in great measure, if not entirely, founded on the still greater mortality of the opposite method. He himself once professed this method, and put it in practice on a vast field of battle; but the result was far from answering his expectations, and, in the campaign of Poland, he lost all his cases of amputation for fracture of the thigh by gunshot wound.[1] This astonishing want of success is unparalelled in the annals of surgery, and, in consequence, is not entitled to the weight he would attribute to it; the cases should be merged in the statistics collected in reference to the same subject, and their ominous character would then be in some degree neutralized. These statistics however, as hitherto studied, do not possess an encouraging character for the operating surgeon; the examination made by M. Malgaigne of the mortality of amputation of the thigh for traumatic lesions in the Parisian hospitals, during a period of ten years, extending from 1836 to 1846, gives 24 deaths for 34 operations, somewhat less than three-fourths,[2] or 1 in 1·4.

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[1] Meeting of the Acad. de Méd. le 8 Août 1848.

[2] At the same meeting, M. Malgaigne erroneously set down this mortality at more than 3·4ths.

Dr Fenwick, in his memoir on surgical operations,[1] containing incomparably the best statistics hitherto published on amputations, found the average mortality on a total of 869 amputations of the thigh for various causes, to be 1 in 2·31.

In order to throw light on the long controverted point, whether the result of amputations on account of traumatic lesions or of disease be the more favourable, he extended his researches, and succeeded in ascertaining that the mortality is much greater in the former than in the latter class of cases. In 487 amputations of the thigh, he showed that 313 performed for pathological causes gave 124 deaths, or 1 in 2·52, while 174 for traumatic lesions gave 112 deaths, or 1 in 1·55; a mortality somewhat less than that of M. Malgaigne, but still sufficiently high to shake the courage of the boldest surgeon.

M. Jobert, in his work on gunshot wounds,[2] after the detail of some cases of fracture of the femur, where the limb was preserved, states,—"It results from these facts, sufficiently numerous, that in every case of comminuted fracture of the bone by a ball, in an adult of great muscular power, I should be induced, in conscience, to amputate, the rather because the wounded almost always sink during the acute period, or under the abundance of the suppuration. Besides, if they resist these formidable complications, it is to retain an unserviceable member, a source of suffering without end." The views of the author have undergone a complete revolution since the publication of the above work; his only exposition of the causes of this change is that laid before the Academy of Medicine.[3] From this it would appear that the mortality of the amputations, and an opinion that the sad results which attended the conservative practice were due to a mode of treatment at least as dangerous as the wounds themselves, decided him to attempt the union of all fractures; the latter reason is probably the secret of his innovations in the treatment of gunshot wounds.

Eight cases of comminuted fracture of the femur from gunshot wounds, received during the days of June, have been treated in the service of M. Jobert. At the meeting of the Academy of Medicine, on the 26th of September, he stated that six of the patients were dead; and that in the remaining two the union of the fractures was accomplished by the definitive callus. The following is a summary detail of these cases:—

Descan, aged fifteen, carter, admitted June 27, 1848; ball entered on the outer surface of the thigh, about its middle, fractured the femur, and presented itself beneath the skin at the inner side, on a plane slightly higher than

[1] Report of the Surgical Operations performed at the Newcastle-upon-Tyne Infirmary, during a period of seventeen years and a half.—Monthly Journal of Medical Science, Nos. from October 1847 to February 1848, inclusive. Translated in the Arch. Gén. le Méd. 1848.

[2] Des Plaies d'Armes à Feu. 8vo. Paris, 1833.

[3] Meeting of September 26, 1848.

that of the orifice of entry, whence it was extracted by incision. No dilatation of the wound nor extraction of splinters was performed; cold poultices were applied during nineteen days. July 29.—Two days since, patient was removed from the ward *Napoleon* to the ward *St Augustin;* his appetite and sleep have been slightly troubled in consequence; appetite good previously. Considerable swelling of the thigh, heat natural, suppuration moderate, no fever. Sept. 25. (Period when the case was reported to the Academy.)—Internal orifice healed, external still open; wounded thigh fully twice as large as the other, no tension, suppuration diminished; large deposition of provisional callus, irregular in form; shortening of the limb to the extent of two centimetres. The patient is completely free from suffering. Oct. 16.—Swelling of fractured thigh somewhat diminished; suppuration by external wound continues, but is slight in quantity. Extension by pelvic and foot bands is still maintained; the pelvic band is passed beneath the groin on the unaffected side. Oct. 24.—Suppuration still continues.

Philippe Hubert, aged twenty-six, blacksmith, unmarried, admitted June 23, 1848; wounded by a musket-ball, which traversed the thigh, fracturing the femur somewhat below its superior third. No dilatation of the wound, nor examination of the fractured bone, made; cold poultices applied. Swelling and tension great, to relieve which M. Jobert practised two longitudinal incisions, unconnected with the tract of the ball. An abscess subsequently formed, and was opened about the third week. The cold poultices were continued for about three weeks. Sept. 23. (Period when the case was reported to the Academy.)—Free from suffering, general health good; position of the limb good, extension kept up by pelvic and foot bands, as in the last case; another band extended across the bed, and, attached to its sites, prevented elevation and displacement of the knee; no splint in either case from the commencement. Oct. 16.—Limb straight, shortening to the extent of two centimetres (about three-quarters of an inch); suppuration continues, pain experienced in neighbourhood of fracture; general health not so satisfactory, some irritability, and diarrhœa. Oct. 24.—Abscess at the inner side of the thigh healed; general health good; fistulous opening still exists. Warm baths.

M. Malgaigne, in the report of his wounded before the Academy (August 8), stated that he had received five fractures of the thigh, and that of these two were cured. One of the successful cases was treated during the first three weeks by M. Blanchet, and was only admitted into the service of M. Malgaigne, July 11. This case cannot, therefore, be correctly included in the statistics of the latter.[1] Of the other cases treated by him, the following alone survives.

Guillaumet, *garde mobile,* aged twenty-two, wounded June 24; the ball entered the thigh anteriorly, about the middle of the rectus muscle, passed backwards and slightly internally, fracturing the femur in its course. The bone would appear to have been struck obliquely on its inner side. No examination into the state of the fracture was made. Sept. 24.—The patient states that, from the time of admission, he has been exempt from suffering; that, at the beginning of August, the posterior orifice was closed, and that the anterior one likewise healed towards the end of the same month, without a single splinter having passed out or been extracted. Consolidation of the fracture is effected with riding of the fractured extremities, the thigh being shortened to the extent of more than an inch, and presenting a curve with the convexity outwards. Oct. 24.—Has continued perfectly free from pain. At

[1] This case is already recorded in connexion with disarticulation at the hip-joint.

the visit of this morning, M. Malgaigne declared this to have been a case of simple fracture of the femur.

The following is the only case of fracture of the shaft of the femur under treatment in the wards of M. Velpeau :—

Leroy, aged twenty-three, soldier, admitted the 25th of June, with fracture of the femur, somewhat above its middle, by a musket-ball. No extraction of splinters was practised. Sept. 26.—Fractured thigh considerably swollen, but without tension; suppuration continues; provisional callus in large quantity, and irregular form. Oct. 31.—There appears but little prospect of the limb being a useful one, if even it be saved; there is considerable swelling, with discharge of thin dark-coloured pus, together with marked deformity, the thigh being curved outwards.

Three cases of comminuted fracture were treated by M. Laugier. One of these cases, which has terminated fatally, is excluded, as it would have necessitated disarticulation at the hip-joint. Of the two remaining, one is also dead, the other is still under treatment :—

This patient, a washerwoman, aged thirty-six, married, of excellent health, active and steady habits, was wounded accidentally, June 25, by a musket-ball, which traversed the thigh, shattering the femur about its centre. No dilatation nor extraction of splinters was performed; but the presence of numerous fragments was distinctly perceptible during the pressure exerted for the discharge of the pus, at the time of changing the dressings. The ordinary apparatus of compresses, many-tailed bandage, junks, splints, and "*porte-attelles*," was applied, but no extension kept up. Nothing but the excellence of her constitution seems to have supported this woman through the course of prolonged suppuration, fever, and occasional attacks of diarrhœa, which she has undergone. She is now much emaciated; suppuration continues; there is considerable deformity of the limb, and consolidation is incomplete, splinters in all probability still remaining in connexion with the fracture.

In the service of M. Richet, at the Tuileries, which is considered to have presented the best possible hygienic conditions for the patients, three, with comminuted fractures of the femur, resisted amputation, and died respectively on the 10th, 17th, and 29th days.[1]

To avoid further details, the following Tables have been drawn up. They comprise all the cases connected with the present inquiry amongst the wounded of June, of which the writer has been able to complete the history up to the present time :—

[1] M. Huguier attributed the success of his amputations at the Tuileries to the favourable hygienic conditions; the fractures in which conservation was attempted were not, on that account, the less fatal.

TABLE I.

Cases of Fracture of the Femur where Amputation in the continuity of the Bone was practicable, but in which Preservation of the Limb was attempted.

Surgeons.	Cases treated.	Deaths.	Remaining under treatment.
M. Jobert . .	7	5	2
... Malgaigne .	4	3	1
... Boyer . .	5	5	0
... Baudens . .	2	2	0
... Richet . .	3	3	0
... Giraldès .	2	1	1
... Nélaton . .	1	0	1
... Laugier . .	2	1	1
Total .	26	20	6

TABLE II.

Cases of Fracture of the Femur treated by Amputation.

Surgeons.	Cases.	Deaths.	Cures.
M. Richet (Tuileries) . .	1	0	1
... Larrey	1	0	1
... Baudens	1	1	0
... Roux	1	1	0
... Blandin	1	0	1
... Monod (Maison de Santé)	1	0	1
... Huguier (Tuileries) .	3	0	3
... Gerdy	2	2	0
Total	11	4	7

These Tables do not include all the cases afforded by the events of June; but having been collected generally, and not selected, they may be held to offer a fair criterion of the success of the two methods of treatment.

The first Table has been framed so as to give the most favourable character the materials permit to the conservative practice; it includes a case of simple fracture, the only one remaining in the service of M. Malgaigne, and the case of fracture in the lower fourth of the femur treated by M. Nélaton, where the splintered fragments were small in number and size, and removed immediately; the particulars of both these cases have been already given. It has been impossible to include the cases of fracture of the femur admitted into the wards of M. Velpeau, from the writer having failed to reconcile the facts there observed with the account of them given to the Academy; the details of the case still under treatment have

been given in illustration of what so high an authority considered, as far back as the 12th of September, a case in progress of cure.[1]

What is the result of the conservative practice? Twenty-six cases have been adduced in which it was adopted; of these 20, or 77 per cent., have already succumbed. Six only survive, all of them with shortened limbs, and of these one was a case of simple fracture; in this and the case of M. Nélaton, of fracture at the lower fourth, the external wounds have healed; in all the others fistulæ exist, and suppuration continues. Further, in one of the latter cases, that of M. Giraldès, the fracture also occupied the lower fourth of the femur, the situation which Larrey, Schmucker, Guthrie, and S. Cooper conceived to justify an attempt to save the limb; even in this case the progress to cure is still complicated by the presence of a fistula and portions of necrosed bone. The case of M. Laugier presents little prospect of a favourable termination. The two remaining cases are those of M. Jobert, in which he reported the consolidation as completed by the formation of the definitive callus (2 *ont gueri par cal secondaire*) on the 26th of September, a time at which, it appears from the report given of the cases, that swelling and suppuration still existed, and that extension was still maintained. The re-union of the fractured extremities by definitive callus is a process which is known not to be completed under a twelvemonth, and, once accomplished, extension can no longer exert any influence. It cannot be sustained that, in these cases, union had proceeded thus far; on the contrary, the fistulæ and the suppuration clearly denoted the presence of fragments of dead bone interfering with the progress of the cure. It is not improbable that these cases may turn out to be such as M. Bégin alluded to when he stated:[2]—"I have frequently met persons, to whom it was believed that a great service had been rendered by the preservation of their limbs, who, after many years of suffering, presented themselves to me with fistulæ, inflammatory swellings, inexhaustible suppuration, rendering amputation necessary, when it was thought that their limbs had been for ever saved. Amongst others, I remember the case of an officer wounded at Lutzen, in whom splinters of the tibia were enveloped by a bony production of new formation. Abscesses formed every year, intolerable pain supervened. I was obliged to amputate the thigh twenty-five years after

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[1] In collecting the observations of a great number of cases in different hospitals, it is possible that some may have escaped notice. At the sitting of the Acad. of Med., Sept. 12th, all that M. Velpeau stated regarding his fractures of the femur was:—"Sur 5 fractures de cuisse, je n'ai fait qu'une amputation; des 4 autres, 1 est sorti; les 3 autres sont en voie de guérison."—(Gaz. Méd. le 16 Sept., 1848). The amputation was performed during the period of reaction, and was fatal; the case discharged could not be considered cured, its termination could not be predicted at so early a period.

[2] Acad. de Médecine, 26th Sept. 1848.

the infliction of the wound." M. Baudens referred to similar cases, and most military authors have adduced examples of them.

M. Jobert believes that splinters of dead bone may contribute to the permanent consolidation of the fracture; a belief opposed to the principles of surgery as taught in every age and country. Rare cases have existed, where balls have been lodged in bones without creating disturbance; but no example has hitherto been recorded of necrosed bone occupying the same position with impunity.

Twenty of those whose limbs were thus attempted to be saved, have already died; six out of twenty-six survive, and, in some of these, there is every probability that the limb must yet be sacrificed. On the other hand, Table II. gives four deaths for eleven amputations, a mortality less than one half of what has already occurred in the other series,—a result decidedly in favour of amputation in these cases. It is to be observed that the amputation of the thigh, performed by M. Baudens, was not fatal till the 34th day, when the stump was completely healed, with the exception of a small spot. The case was that of General Damesne, who suffered much anxiety as to his prospects, and the dread of losing his command. In some of the other fatal cases the thigh was amputated after the period of inflammatory action had set in, which experience has proved to be the most unfavourable period for operation.

It may be urged that the statistics here brought forward are too limited to influence the decision of the question; they, however, place beyond doubt the delusive nature of the hopes which are generally entertained during the early period of the conservative practice, the unsatisfactory nature of the results in the great majority of the successful cases, and the frightful mortality at which they are purchased. In this they only bear out the conclusions of previous observers. When Gualtiér de Claubry, formerly surgeon to the imperial guard, joined the army of Italy, he entertained sanguine hopes of success from the conservative treatment; the mortality which attended his trial of it obliged him to abandon it. With regard to the army in Spain, he states that almost all the military, who had the thigh fractured, died when immediate amputation was not performed. Paillard joined the army at Antwerp with the same conviction, and underwent the same conversion. Ravaton, Lombard, Ribes, have held the same language. According to Percy, scarcely 1 in 5 recovers of those who have suffered gunshot fracture of the femur; according to Guthrie, 1 in 6; and Schmucker, 1 in 7.

M. Malgaigne supported his views by a reference to the results of Dupuytren's practice in 1830, thus drawing an opposite conclusion from the same cases. In a clinical lecture delivered after the *emeute* of June 1832, Dupuytren stated,—"I have repeated it frequently, and I repeat it for the last time, after the facts which I have witnessed, principally in 1814, 1815, and 1830, my opinion on this point is not to be shaken. In compound fractures, in those

especially by gunshot wounds, in rejecting amputation more individuals are lost than limbs saved."[1]

The most extensive scale on which the merits of the conservative practice have ever been tested, was in the Prussian army under the authority of Bilguer, who announced at the conclusion of the war, as quoted by Sir G. Ballingall, that, amidst 6000 wounded, the Prussian surgeons had not amputated a single limb. Out of 300 cases of fracture of the extremities, he cites 11 as instances of surprising cures, and even these are considered destitute of the accuracy and minuteness of detail which would render them useful. He excludes from his calculations gunshot fracture of the femur as being perfectly hopeless.

To deny the mortality which attends the preservative practice in these cases after this evidence, would be in vain; neither is it so much by denying a high mortality, as by adopting the general conclusion, that the wounded do not run a greater chance of death by seeking to preserve their fractured limbs than by amputating them, that the advocates of the attempt defend their practice. In proof of the high mortality which follows primary amputation, M. Malgaigne adduces the statistics which have been already quoted, and which give a mortality of 70 per cent. in amputation of the thigh. The statistics of Dr Fenwick, which are more extensive, and include those of M. Malgaigne, reduce the mortality to 64 per cent. Deciding the question by these numbers, the balance in favour of amputation is considerable; for, assuming the limbs preserved to be 1 in 6, and a more favourable calculation does not seem justifiable, the mortality is more than double that yielded by the statistics of Dr Fenwick.

As has been stated, the important fact, that the amputations for traumatic lesions, almost exclusively primary, performed in civil hospitals, are fatal in a much greater proportion than those performed for pathological lesions, such as diseases of the joints, in which the condition of the patient is assimilated to that existing in secondary amputations, appears established.

Is, however, the practice with respect to gunshot fractures to be governed by these statistics? The writer believes not; whereas it is by the ratio of deaths, thus afforded, that M. Malgaigne seeks to settle the mortality of amputation in these cases. He is well acquainted with certain facts, which seem to militate against such a course. Thus he cited the success of Fercoq in 1794, who performed 60 primary amputations on beaten sailors, all of which, except 2 followed by tetanus, were successful, being a mortality of 1 in 30; of Percy, who, after the affair of Neubourg, out of 92 amputations of the leg, thigh, and arm, lost 6, or 1 in 15; of Guthrie at New Orleans, who, out of 45 primary amputations, had 7 deaths,

[1] Dupuytren. Traité des Blessures par Armes de Guerre; Leçons Recueillies et Publiés par Brierre de Boismont et Marx. 2 vols. Paris, 1839. 2e. éd.

or 1 in 7; after the battle of Toulouse, 47 amputations, 9 deaths, or 1 in 5; of the English army in Spain, 291 amputations, 24 deaths, or 1 in 8. "Del Signore at Navarino; here it is superb; 31 primary amputations, 1 death, 1 in 30!" "The English surgeons at the battle of Aboukir, and of Camfordomer, 30 primary amputations, 30 cures!!!" Larrey during the days of the 27th and 29th *brumaire*, 1 in 6. To these he might have added, that Masselet, after the battle of Aboukir, performed 11 primary amputations, all of which were successful.

M. Malgaigne endeavours to discredit these results, not by evidence or reasoning, but by irony; his statistics do not correspond with them; the greater the success, the greater the number of marks of admiration with which the paragraph announcing it is honoured. It is not thus, nor yet by the application of the epithet "*superbe*," that facts are to be overthrown, and science advanced.

These facts exist, and, instead of denying, it should be sought to explain them; they are not more marvellous than the success of M. Huguier, who performed 19 capital operations for fractured extremities during the days of June, including several amputations of the thigh, humero-scapular disarticulations, &c., without a single death.

By the statistics collected by M. Malgaigne, of amputations performed in the Parisian hospitals for traumatic lesions from 1836 to 1842, a period free from any popular struggle, and therefore not productive of gunshot wounds, it is shown that 182 operations were followed by 117 deaths, being a mortality of 1 in 1·55, or 64 per cent.; the statistics of the Newcastle Infirmary, published by Dr Fenwick, give in 81 traumatic amputations 26 deaths, an average mortality of 1 in 3·11, or 32 per cent; the statistics collected by him from the returns of different hospitals, in 597 similar operations, 304 deaths, a mortality of 1 in 1·96, or 51 per cent.

When these results are compared with those above cited, the discrepancy is so great, as naturally to suggest the inference, that they form two distinct classes of cases—a conclusion which derives confirmation from the military statistics of Mr Alcock, which give on a total of 664 primary operations an average mortality of 1 in 8·3, or only 12 per cent.

How is this discrepancy to be explained? What is the distinction between the traumatic lesions which present themselves in civil hospitals, and the lesions met with in gunshot wounds? This distinction is believed to lie in the more serious nature of the injury, and consequent grave character of the nervous commotion, which exists in the former class of cases. The traumatic lesions necessitating amputation, admitted into civil hospitals, are ordinarily caused by railway accidents, by machinery, crushing of some of the extremities by the fall of stones, weights, &c., the passage of carriages over an extremity, and so forth, producing frequently a frightful amount of laceration and destruction of parts, and the

nervous commotion attending these accidents is proportionably severe. The effect of this is evident in the statistics of Dr Fenwick during the four first days; in other words, during the period when nervous commotion would exert its most prejudicial influence, the number of deaths is 10 times greater in amputation of the thigh, and 8 times greater in amputation of the leg, for traumatic, than in the same operation for pathological causes; while 74 pathological amputations did not occasion a single death from nervous commotion, 106 traumatic amputations gave 12 deaths from this cause alone, or 1 in 8·83; the same with gangrene of the stump, which is attributed to nervous disturbance (1 in 17·66), and which generally terminated fatally in the four first days after traumatic amputations, while it did not cause one death in those for disease.

If the frightful mortality from these causes, which traumatic amputations produce, be compared with the almost insignificant mortality of amputations practised for disease of the joints, one cannot avoid being struck with astonishment; for in the latter case, during the period of nervous commotion, there are not 2 deaths in 100 operations. These facts are placed beyond doubt by the tables of Dr Fenwick; and they show that the greater mortality of these over pathological operations, is, in chief measure, due to the shock to the system caused by the accident.

Authors have commonly insisted on the shock to the system which is caused by gunshot wounds, and on the necessity of deferring amputation till its effects are passed away. The observation of modern military surgeons goes to show that these effects have been greatly exaggerated. In the numerous cases which fell under the writer's notice, he did not witness in any of the cases of fracture of the extremities, any such amount of shock as would counter-indicate immediate operation, no marked effect on the pulse, no coldness nor clamminess of the surface, no stupor; in several cases of abdominal wound, the signs of commotion were strongly pronounced, and the contrast was most striking. Sir G. Ballingall maintains, that the patient supports an examination into the condition of the injured parts, immediately after the infliction of the injury, better than at any subsequent period. M. Baudens has stated to the writer, that in his experience it has been exceedingly rare to witness general commotion from fracture by musket-balls, unless perhaps where the bones have been shattered in the neighbourhood of the hip-joint, but that the case is different as regards the effect of cannon-balls or howitzers.

The more extensively the method of amputating on the field of battle has been tried, the more clearly have its advantages been demonstrated; M. Bégin insists that amputation should be immediately performed, in order to give the patient every advantage. The success attending it is incompatible with the supposition of the existence of shock. The well-known conversion of the French surgeons, by witnessing the success obtained from immediate opera-

tions, performed by the American surgeons during the war of independence; the excellent influence on the *morale* of the troops, which the flying *ambulances*, first organized by Larrey under Napoleon in Italy, and which were adopted for the immediate performance of amputations, exerted; the fact of 664 cases of operations performed on the field of battle, collected by Alcock, only yielding a mortality of 12 per cent, combined with the examples of success which have been quoted after M. Malgaigne; and the still more astonishing success of Dr Burke, recorded by Sir G. Ballingall, of 80 cases of amputation with the army in Upper Scinde, not one of which had terminated fatally during the first 14 days—are so many additional proofs that nervous commotion, that powerful cause of mortality in the traumatic operations in civil hospitals, is in great measure, if not entirely, wanting in cases of gunshot fractures, and explains the superiority of the success which follows amputation in these cases, over that which is observed under other circumstances. The excess of mortality in the traumatic operations in civil hospitals over that in the pathological cases, is not to be attributed to the fact of the patient being in the one instance in his ordinary state of health, in the other prepared for the operation by confinement and suffering—a conclusion to which the compilers of statistics seemed to be fast arriving; but to the mode of infliction and severity of the traumatic lesions which render the operation necessary.

Should this view be confirmed, the bearing of the statistics of amputation in gunshot wounds will be entirely changed, and surgeons will no longer be seen, appalled by their formidable character, forgetting their doctrine and theory, to temporise. Amputation will be freed from the terrors which now surround it, and its adoption, in the true interest of the wounded, be resorted to in those cases of fracture of the femur in which it has been held to be indicated.—(*To be continued.*)

ARTICLE II.—*Notes on the Epidemic Fever of* 1847-48. By WILLIAM ROBERTSON, M.D., F.R.C.P., Physician to the Edinburgh Royal Infirmary.

THE following observations upon the fever epidemic of 1847-48, are founded solely upon data furnished by my own wards in the Royal Infirmary, from 1st March 1847 to 1st March 1848, and exhibit a faithful record of the proportionate mortality in the two forms of fever lately prevalent. I say *two* forms, because, after excluding cases of Ephemera and of Ague, I met with none which could not be included under the heads of Typhus and Relapsing fever. Another head I would gladly have introduced for the classification of the cases of Dothinenterite, of which, in the early part of the epidemic, a considerable number was observed. From what

I saw, however, in the dissecting theatre, I soon became convinced that the diagnosis of this disease is uncertain; that although it is easily recognised during life in certain exquisite cases, accompanied with its most characteristic symptoms, yet in others it may totally elude observation; and, above all, that cases of our ordinary Edinburgh typhus may go on to a fatal termination, deceiving the physician into the belief that Dothinenteritis exists, and yet, on post-mortem examination, revealing only those equivocal appearances so frequent after death from fever. Besides, I am inclined to consider the so-called *dothinenterite* as a mere variety of typhus, and have accordingly arranged under that head all the fatal cases which occurred to me.

For the explanation of the following table it is necessary further to state, that in a few instances convalescents from relapsing fever became affected with typhus, while convalescents from typhus contracted relapsing fever, before being dismissed from the wards. Such cases are invariably twice entered in the table; and when death ensued it is always attributed to the form of disease under which the patient last laboured. A large number of the deaths were immediately caused by dysentery, pneumonia, and similar affections, occurring after convalescence from fever seemed fairly established. I have, however, judged it proper to retain these fatal cases under the heads to which upon admission they were properly assigned, persuaded that by any other course I should present a very unfair view of the average mortality. A considerable number of miscellaneous cases find their way into fever wards—no reference is made to such cases in the table.

From the 1st March 1847 to the 1st November, the cases in Fever Hospital A (a building in Surgeon Square) were carefully collected; during the winter months my charge only extended to Shed E (a wooden building erected against the south wall of the Infirmary), and the two fever wards, 9 and 15, in the top story of the old building. The gentlemen who assisted me as resident clerks—Drs Begbie, Baikie, Morris, England, and others who acted for shorter periods—all kindly took their share in the labours necessary to make these statistics accurate, and no one who has not been engaged in similar pursuits can estimate the trouble required for such a purpose. The dry task of daily entering, dismissing, and classifying a large number of cases, is no pleasant addition to the daily and nightly duty of superintending a hundred fever patients. To these gentlemen my best thanks are due for the zeal and efficiency with which all their duties were performed.

In a communication read to the Edinburgh Medico-Chirurgical Society, in July 1847, Dr Bennett, at that time pathologist to the Royal Infirmary, stated that from 1st November 1846 to 30th June 1847, 2071 cases of fever had been treated in the hospital,

and that of this number 278 had proved fatal—*i. e.*, there was a mortality of 13·42 per cent. Now, it is notorious that considerable errors find their way into the general statistical record of the hospital from which Dr Bennett must have drawn the above statement; yet it is singular that this rate of mortality coincides very accurately with that deducible from my own table, if the calculation be restricted to equal numbers of *male* and *female* cases.

TABLE.

Admitted in	Males.				Females.			
	Typhus Fever.	Died.	Relapsing F.	Died.	Typhus Fever.	Died.	Relapsing F.	Died.
March.	38	7	17	0	20	2	13	0
April.	38	10	20	0	29	10	22	0
May.	32	2	20	1	21	2	9	0
June.	31	9	23	1	24	1	16	1
July.	23	10	24	0	13	2	35	1
August.	25	8	43	2	18	3	28	1
September.	17	2	39	8	9	2	20	0
October.	18	3	30	0	7	2	25	0
November.	29	9	60	5	13	7	23	0
December.	22	6	35	0	15	6	13	0
January.	37	15	32	1	17	3	14	1
February.	20	6	15	1	22	6	13	0
Total	330	87	358	19	208	46	231	4

Fever Hospital, A. — March. April. May. June. July. August. September. October.

E. 9 & 15. — November. December. January. February.

Mortality of Males.
From Typhus . 26·36 per 100.
From Relapsing Fever 5·3 per 100.

Mortality of Females.
From Typhus . 22·11 per 100.
From Relapsing Fever 1·72 per 100.

Gross mortality from Typhus . 24·72 per cent. or 1 in 4 nearly.
Gross mortality from Relapsing Fever 3·9 per cent. or 1 in 25·6 nearly.

Total of Fever cases 1127; of which died 156.
Average mortality 13·842 per cent. or 1 in 7·224.

But the above calculations are founded upon 688 male cases, and only 439 female.

The average mortality in males was 15·407 per cent.
The average mortality in females was 11·389 per cent.

The mortality in equal numbers of *male* and *female* cases, may consequently be stated at 13·398 per cent. or 1 in 7·46—numbers very closely approximating to those given by Dr Bennett.

In a paper published in the October number of the Edinburgh Medical and Surgical Journal, Dr Robert Paterson exhibits an elaborate statistical account of the cases treated by him in the sheds and tents erected last year for the accommodation of fever patients within the grounds of the Royal Infirmary. He considers

that the small mortality in the sheds and tents proves that such erections are better adapted for the treatment of fever than the ordinary fever wards of an hospital. From this conclusion I must dissent for various reasons. *Firstly*, I am sure that the female fever cases treated in the tents and sheds, were for considerable periods milder than those received into the main building of the Infirmary, and into the fever hospitals in Surgeon Square. During the months on which the duties of the admission-room devolved upon myself and my clerks, we uniformly acted upon what we deemed the *humane* principle, of not sending bad cases to the tents or sheds when there was room for them elsewhere. Dr Paterson is possibly correct in his statement, that the different forms of fever were observed in equal proportion in the tents, sheds, and ordinary wards of the hospital; but certainly he had not many patients sent to him *moribund*, nor do I think that the records of the tents can embrace many instances of death occurring within 48 hours after admission. The ordinary wards of the institution received far more than a fair proportion of unpromising and desperate cases, and to this circumstance I am inclined to attribute the large mortality observed in the house, as contrasted with that of the sheds and tents. *Secondly*, My own experience of the treatment of fever in sheds was not satisfactory. The shed E was under my charge all winter, and I had an opportunity of contrasting the progress of patients there, with that of similar or more severe cases treated at the same time in ward 9 in the wing of the main building. I well recollect the ravages which dysentery, pneumonia, and acute inflammations of the serous membranes caused among my convalescents in the shed towards the middle of winter, while the patients in No. 9 enjoyed a comparative immunity from these affections. *Thirdly*, Granting, as I most cheerfully do, that the experiment of treating fever cases in tents, or such "temples of the winds" as the Infirmary sheds, proved eminently successful during the mild dry summer of 1847, I do not think it probable that we should be equally fortunate on future occasions, unless we could calculate upon the prevalence of the epidemic during equally genial weather. I can see no solid reason for preferring a tent or shed to a well kept fever ward; and must accordingly, on public grounds, enter this *caveat* against the conclusions of Dr Paterson. At the same time, I must bear testimony to the energy and success with which he laboured at the Infirmary during the prevalence of the epidemic, and to the pleasure which I myself had in co-operating with so able and estimable a colleague.

From March to July 1847, while scurvy prevailed to a great extent among the railway labourers in the neighbourhood of Edinburgh, and among the poor of the city itself, a considerable number of fever cases, complicated with Scorbutus, were admitted into the hospital. On theoretical grounds, it seemed likely that the fever would in these cases prove of an unusually malignant type, and such certainly was its character in the two first cases of the kind which

fell under my observation. My notes do not, however, show that the gross mortality was materially affected by the *scorbutic* complication. Of 26 cases which I find recorded, 21 occurred in males, 5 in females; 2 of each died,—a mortality of 15·4 per cent. It must be recollected that the complication in almost all these cases co-existed with typhus. One died apparently from the exhaustion caused by repeated epistaxis, another in consequence of hæmorrhage from the stomach and mucous membrane of the lungs.

Numerous examples of *dothinenteritis* were observed in the earlier period of the epidemic, chiefly among patients brought from the country, and especially among the railway labourers on the Hawick line, to the south of Dalkeith. For reasons already assigned, I am unable to state the proportion of deaths occurring among patients who laboured under this form of disease. There is, I think, reason to fear that it must have been very large, probably (as in the experience of some Parisian physicians) amounting to nearly a third of those affected. Dr John Reid, while pathologist in the Royal Infirmary, used to remark the extreme rarity of dothinenteritis among the inhabitants of Edinburgh; and, in confirmation of the accuracy of his observation, I can state that no case of dothinenteritis, authenticated by post-mortem examination, and occurring in an inhabitant of Edinburgh, has for the last three years been seen in my wards.

Physicians have been long familiar with the enlarged and softened state of the spleen, so commonly found after fatal attacks of typhus. During the late epidemic, another morbid state of the viscus was very frequently observed, consisting of deposits in its substance of a brownish grey colour, and of a cheesy or creamy consistence.* These deposits were of various sizes, from that of a walnut to that of a pin-head—there were often several in the same spleen—and they were most usually situated immediately beneath the peritonæal tissue of the organ. They were most frequently noticed after death from typhus, but were not uniformly, and I think not even commonly, associated with dothinenteritis—in at least one case they were observed after death from relapsing fever. Although I made many examinations of these deposits with the microscope, I never could convince myself that they were histologically identical with the so-called *typhus deposits* found in the glands of the ileum and mesentery. For several months past, few or none of these exudations have been seen in the bodies of fever patients examined in the Infirmary. In the summer of 1847, I lost two patients from *peritonitis*, consequent upon the rupture of softened splenic deposits into the sac of the peritonæum.

Observations on the Relapsing Fever.

The relapse on the 14th day seems to be the most regular feature

[1] Monthly Journal, vol. ii. p. 299.

of this disease. Various attempts were made to check its periodicity, by the administration of different remedies during the remission. Quinine, Bibeerine, Arsenic, and Digitalis, were all fairly tried, and all proved equally inefficacious. The only practice which seemed occasionally to postpone the relapse, or to lessen its violence, was the administration of a powerful emetic early on the 14th day of the disease. Several of my hospital patients who were subjected to this treatment left the house about the 17th day, believing themselves cured; others did not relapse till the 18th day, and I believe that relapses may have occurred among those who were dismissed at their own desire, persuaded that the disease would not return. One of my resident clerks (Mr Rennie), who had a severe attack of this form of fever, took the emetic on the 14th day, and did not relapse till the 18th.

Jaundice was less frequent than in the epidemic of 1843. When present, it was usually accompanied with tumefaction of the liver, which was sometimes by percussion ascertained to extend from 3 to 4 inches below the right costal cartilages. The increase in the dimensions of the liver was also frequently observed in patients who were not jaundiced. It was interesting to watch the rapidity with which this *portal* (?) engorgement subsided under the use of appropriate remedies. Saline purgatives frequently repeated, and the detraction of a few ounces of blood from the epigastrium by cupping or leeching, generally reduced the viscus to its normal dimensions in a few days. I seldom had recourse to blistering in these cases, and never gave mercurials, except calomel, as a *purgative*.

The spleen, if the indications of percussion may be trusted, became much more frequently engorged than the liver. In almost every case in which I examined the left hypochondrium during the height of the disease, the dimensions of the spleen considerably exceeded the normal,—the dulness in extreme cases extending over a space of fully six inches by five. It was remarked that the margin of the enlarged spleen could seldom be detected below the cartilages of the ribs,—its upper border was sometimes ascertained to correspond with the fifth intercostal space. There was very rarely pain or tenderness over the dull region. This affection of the spleen usually soon subsided after the sweating crisis; in some cases, however, it did not yield without treatment. The most efficient remedies seemed to be combinations of aloes with Sulph. Quinæ or Sulph. Ferri, and the Syrup. Ferri Iodureti.

In five or six cases, a remarkable form of delirium was observed immediately to precede the sweating crisis. Its peculiar characters were its suddenness, its violence, and its extremely transitory nature. In the first case of the kind which I witnessed, the patient was labouring under a *relapse* of average severity, and had conversed with me quite coherently when I passed his bed at the daily visit. Scarcely had I left the ward, when I was recalled to assist this patient, who had suddenly become most outrageous, screaming

aloud, raving, and abusing the attendants who were endeavouring to prevent him from jumping out of bed—stools and urine were at the same time passed involuntarily. Within fifteen minutes the same patient was lying calm and collected, forgetful of what had passed, and bathed in a most profuse and critical perspiration. After I had noticed similar phenomena in three or four cases, I was informed by my clerk, Dr Heslop, that these curious symptoms were of more frequent occurrence in the relapsing fever of Dublin, and that they had already engaged the attention of some of the physicians there. To the best of my knowledge, they have not been hitherto described in connexion with the Edinburgh epidemic.

ART. III.—*Case of Wound of the Chest, penetrating the Right Ventricle of the Heart.* By JOHN TRAILL, Surgeon, Arbroath.

(*Communicated by Professor Syme.*)

ON Wednesday evening (September 20th) I was hurriedly summoned to Kinblethmont House, about four miles distant from Arbroath, to visit Alexander Gray, stable-boy to W. F. L. Carnegie, Esq., who was reported to have been stabbed in the side, and to be in a dying state. On reaching Kinblethmont, I found the lad (nineteen years of age, and stout and healthy) in bed, to which he had just been removed; he was very pale, with a strong expression of anxiety in his countenance—the whole surface of his body cold and damp—and his pulse so feeble as to be scarcely perceptible, and very quick—his breathing was natural and easy, and he said he had not much pain. He was vomiting, and had previously vomited a large quantity of vegetable matter in a fermented state. On the left side of the chest I found a punctured wound, commencing at the lower edge of the third rib, about an inch and a half from the sternum, and penetrating obliquely downwards and forwards to the depth of two inches and a half—the probe touching the fourth rib, and then passing onwards to the sternum, so that it could be felt under the integuments nearly in the mesial line.

I now learned that the boy had been in Arbroath at a horticultural show, where he had ate a large quantity of apples, plums, and other fruit, and had also drunk several glasses of rum. On coming home, being somewhat excited, he had gone into the laundry where one of the housemaids was seated sewing, and, by way of teasing her, had gone behind the chair, and, kneeling or squatting on the floor, had suddenly seized her by the elbows and endeavoured to pin her back. The girl (a stout young woman) had at the time a boil on her arm, and the lad unfortunately seized the arm over the sore, so as to cause great pain; this made her start up violently from the chair, at the same time striking back with the disengaged arm (the left), in the endeavour to drive him away. At the moment she chanced to have in her hand a pair of sharp-pointed scissors, and, quite forgetting this, the scissors were forcibly struck against the chest so as to produce the wound above described.

The boy immediately got up from the floor and rushed into the servants' hall, where he sunk down upon a chair, bleeding profusely. About an hour had elapsed before my arrival, and the servants, becoming much alarmed by the bleeding, had kept pouring cold water in large quantities over the head and chest until the flow of blood ceased.

My first impression on seeing the state of the boy was, that the wound must have penetrated the chest, and that internal hemorrhage was going on; but finding, on examination with a probe, and afterwards with a round-pointed

director, that the wound was certainly exterior to the wall of the chest, and being also convinced, after percussion and auscultation, that no effusion had taken place into the cavities of the chest, I felt disposed to attribute this alarming state to the combined effects of loss of blood, fright, the overloaded state of the stomach, and, above all, the drenching with cold water, which had been persisted in for nearly an hour. I immediately gave him some ammonia, which was repeated at intervals, and afterwards warm tea and gruel; at the same time applying warmth in every possible way, and friction to the surface. After these means had been persevered in for two hours, his pulse improved; the natural heat began to return, and he felt more comfortable.

On visiting him next forenoon, I found that he had passed the night without much suffering, and at intervals had slept for a short time—his countenance was still rather pale, but not so anxious—his pulse 110, pretty full, but compressible and quite regular—his breathing somewhat oppressed, but not affected by change of position, although he seemed rather to prefer resting on his right side—the respiratory sounds on both sides were natural, and on percussion no dulness could be detected. He complained of some pain about the centre of the chest, which was aggravated by full inspiration; his breath still smelt strongly of sour fermented matter, and once or twice during the night he had vomited.

I opened a vein in the arm, from which blood flowed at first freely, but very soon caused faintness, with almost imperceptible pulse, so that only six or seven ounces were obtained. On the faintness going off, he expressed himself as much relieved, and shortly afterwards a purgative (which had been taken in the morning) acted freely, with still further relief. He had been several times out of bed during the morning, and had walked through the room.

In the evening I was informed that he was much worse, and on going out I found him in a most alarming state; he was delirious, extremely restless, and constantly tossing about in bed, calling on some one to remove the weight from his breast or he would die.

His breathing was quick and oppressed—his pulse weak, quick, and now, for the first time, irregular—his countenance alternately flushed and deadly pale, and expressive of great anxiety. An attempt was again made to bleed him, but only four or five ounces had flowed when deadly faintness was caused.

During the night these symptoms increased, and on Friday he gradually fell into a state of torpor approaching to coma, with irregular and very feeble pulse; but at times he could be roused, and endeavoured to speak, but with great difficulty in articulating.

He died on Friday evening, fifty-two hours after receiving the wound. Some hours before his death, an erythematous blush had come on around the wound, and also around the venesection wounds.

After his state had become so alarming, he was visited by my brother Mr W. Traill, and also by Dr Bruce of Arbroath, both of whom repeatedly examined the wound with a probe and with a blunt director, but without being able to ascertain that the chest was entered. We also repeatedly examined the chest by percussion and auscultation, and were satisfied that both lungs acted freely, and that there was no effusion into either pleural sac; but on the Friday it seemed to us that there was evidently effusion into the pericardium.

During the Thursday and Friday he was bled five times, always with some relief to the pain and feeling of weight over the chest. Antimony, so long as it could be borne, and afterwards ammonia, and twice a dose of calomel and morphia, were given.

On Saturday, fifteen hours after death, an examination of the body by order of the sheriff, was made by Dr Smith of Forfar and the medical men who had visited the young man previous to his death.

On *forcibly* passing the little finger into the wound, it at once touched the cartilage of the fourth rib, which was then found to be partially divided, and the finger could easily be passed into the cavity of the chest, and then came in contact with a sac evidently containing fluid.

On dissection, we found the wound to have commenced at the lower edge of the third rib, an inch and a half from the sternum, then to have passed obliquely downwards and inwards, entering the chest at the upper edge of the fourth rib, close to its sternal extremity; it then passed through the anterior mediastinum without opening the pleural sac, entering the pericardium on its anterior surface, and thence directly into the right ventricle of the heart.

The pericardium contained from eight to ten ounces of dark-coloured blood, partially coagulated, and constituted the sac, which was felt on passing the finger into the wound. There was no effusion into either pleural cavity. The heart was empty, and seemed of small size, pale coloured but otherwise healthy. Neither the heart, pleura, nor lungs presented any appearance of inflammatory action. On the inner surface of the ventricle, adhering to the orifice of the wound so as apparently to close it, there was a small clot of fibrine about the size of a pea, and of a regular spherical form.

The wound in the heart was situated about the lower third or middle of the ventricle, almost close to its junction with the wall of the left ventricle; on the external surface it was rather less than a fourth of an inch, and on the inner surface about an eighth of an inch in length; it divided the muscular fibres in nearly a transverse direction. The wound through the pericardium was also about a fourth of an inch in length.

The wound through the wall of the chest was much longer than that through the integuments, so that it easily admitted the finger through the opening in the intercostal muscles, without dilating that part which divided the cartilage of the rib. The cartilage was divided very obliquely, and evidently by a sharp cutting edge, so that the inner portion formed as it were a very thin slice.

The scissors with which the wound was inflicted were strong and coarse, with both blades alike and thick on their backs, but they were new and sharp-pointed.

It was some time before I could explain the existence of two wounds, one external to, and the other within the wall of the chest. But on considering the nature of the wound at its entry into the chest, its comparatively great length, and the clean incision into the cartilage, showing that at that point a sharp cutting edge had evidently come into action, and on comparing this with the scissors, which when closed formed an instrument, sharp-pointed no doubt, but with two blunt and thick sides; it then became clear, that on the point striking the sharp upper edge of the rib, a separation of the blades must have taken place—the one blade penetrating internally, and the other forming the wound over the sternum under the integuments.

ARTICLE IV.—*Cases of Rupture of the Urinary Bladder, with Remarks.* By EBEN WATSON, A.M., M.D.

(*Read before the Glasgow Medical Society, October* 17, 1848.)

CONSIDERING that rupture of the bladder is not an extremely rare occurrence, there are very few cases of it on record. The reason has been supposed to be an unwillingness on the part of surgeons to publish cases so frequently terminating in death, notwithstanding their best efforts to preserve the patients alive. The great fatality of this lesion ought, however, only to increase the interest

with which cases of it should be studied, while the opportunities thus afforded of obtaining, by inspection after death, exact information regarding the nature of the injury sustained during life, ought to be the more fully and anxiously improved.

Such cases are not, on the other hand, so frequent in their occurrence, that the experience of any one man can be sufficient to establish on a firm basis the pathology, prognosis, or treatment of this grave injury, either for his own guidance or that of other practitioners. Such a happy result can only accrue from a careful induction of numerous and well-observed cases. Nor do we know of any better source from which to derive the required data than are the records of the Glasgow Royal Infirmary, which, from its position in the midst of a populous mineral and manufacturing country, receives into its wards the subjects of more numerous and severe accidents than any other hospital in the kingdom.

The following cases occurred during my residence in the Infirmary as house-surgeon. They are given in nearly the same words in which they were originally inserted by myself in the hospital journals :—

Case I.—William M'Culloch, aged thirty-two, a carter, was admitted into No. 11 Ward of the Glasgow Royal Infirmary, on the evening of August 13, 1844.

Three days before his admission, when the patient was in the act of leaping from a cart, which was the foremost of a line of several, his foot slipped, and he fell to the ground upon his back. While in this position, the wheels of three waggons passed between his legs, and over his left groin and thigh. His chief complaint, after the accident, was of pain in the perineum and above the pubis, with a constant desire but complete inability to pass his urine. The treatment consisted of bleeding from the arm, and by leeches to the parts affected. His bowels were freely moved by medicine.

On his admission, he still complained of pain in the perineum; his countenance was anxious, and his respiration rapid; pulse 113, feeble.

There was livid ecchymosis of the skin on the inside of the left thigh and groin, as also of the scrotum and penis. The cellular tissue of the upper part of the thigh, scrotum, and perineum, especially its left side, were much swollen, and communicated such a sensation as if the parts mentioned were distended with air and fluid. It was not at this time judged prudent to make such an examination as could enable us to detect fracture of the pelvis; but this being strongly suspected, perfect repose in the easiest position was enjoined, fomentations were assiduously applied to the injured parts, a Dover's powder was given, and wine was cautiously administered. The catheter was introduced with ease, and apparently into the bladder. A considerable quantity of bloody fœtid urine flowed by it, and the patient felt much relieved.

Next day this state was much the same, but easier, on account of the frequent introduction of the catheter, by which a very little urine was on each occasion allowed to trickle away. A free incision was made by the late Dr Hannay, then surgeon to the hospital, on the left side of the raphe, and at the anterior part of the perineum. Thirty-two ounces of fluid, which had a strong urinous odour, were thus evacuated; and, when the finger was introduced into the wound, the ramus of the pubis could be felt fractured about its middle. The patient is reported to have experienced much relief on the evacuation of this urinous fluid. Pulse still continued quick and feeble, and his other

symptoms indicated much general prostration. It is also reported that the sensibility of the lower extremities remained unimpaired.

He was ordered 12 oz. of wine and 1 lb. of beef-tea in the day, and to have camphor and opium thrice in the twenty-four hours.

On the 15th the hospital report is as follows:—"Urine flows freely by the wound. Feels and looks relieved. Ends of bone (viz., of pubis, felt through the wound) at a distance from one another, and distinctly moveable. Tongue clean and moist. Pulse 90, improved in strength. Natural movement of bowels. Patient can raise lower part of body in bed. He relishes his wine, has some appetite, and does not vomit."

Next day he was ordered six ounces of whisky, instead of his wine; but there is no report until the 24th, when it it is said that "patient has had several rigors, followed by heat of skin. Pulse rapid, small, and weak. Complains of thirst and of weakness only. Bowels free; tongue moist, and slightly white. Urine flows freely by the wound, and a considerable extent of the ischium is exposed, by sloughing of the cellular tissue."

Dover's powder at night was substituted for the camphor and opium.

Next day (25th) it is reported that there is extensive sloughing of perineum and over sacrum; but that, in other respects, the patient is not worse.

27th. Debility and exhaustion increase. Respirations 40 per minute. Pulse scarcely to be felt, and tremulous.

He died next morning.

29th. *On inspection*, much effused blood was found beneath the pelvic fascia, and in the folds of the mesocolon and mesorectum. The bladder was found ruptured obliquely through the left side of its neck, and below the reflexion of the peritoneum, which constitutes its lateral false ligaments. The coats of the bladder were thickened and inflamed. The rupture was a clean rent through both coats, and admitted three fingers. It opened into what appeared a large abscess, stretching up behind the bladder and between it and the sacrum. The incision had been made into this cavity. The urethra was sound and uninjured throughout its entire length.

The left sacro-iliac synchondrosis was completely separated, and the left side of the body of the pubis was fractured close to the symphysis. The left ramus of the pubis and ischium was also fractured, and the fracture extended into the acetabulum. The tuberosity of the ischium was completely separated from the body of the bone. The cavity of the left acetabulum contained a quantity of purulent matter, and its lining membrane presented the usual appearances of high inflammation.

On the right side of the pelvis the only injury was an oblique fracture of the ramus of the pubis, without any displacement.

At the lower margin of the right lobe of the liver there was a small deposit of purulent matter. Other organs were quite healthy.

CASE II.—George Bell, aged twenty-seven, an engineer, was admitted into ward No. 11 of the Glasgow Royal Infirmary, on the 14th March 1846, at nine and a half P.M. (Dr A. Buchanan was at this time surgeon to the hospital.) Half an hour before his admission the patient was at work beside the steam-engine which he had charge of, when his left foot was caught by the fly-wheel, and he was drawn in among the machinery. The foot passed between two spokes, and the whole force of the revolving wheel came upon his leg, which was pressed against the belt, and the engine thus stopped. But before that was effected his right leg had been twisted upwards, and he had received a violent blow at the lower part of his back from the main shaft of the engine.

On examination it was found that the patient had suffered a severe compound fracture of both bones of the left leg; which was immediately attended to.

He complained much of pain at the right sacro-iliac synchondrosis, especially when the limb of that side was moved, and, with the exception of being able to draw up the leg a little, he had no power of moving it. The posterior part of

the right ilium was felt to project more than that of the other side, and it was more moveable on the sacrum, which seemed at a relatively greater depth from the surface of the back. In short, there appeared to be displacement of the sacrum forwards, and of the ilium backwards, on the right side.

Patient had an uneasy and ineffectual desire to micturate. Some bloody urine was drawn off by the catheter. His face and surface generally were pale and cold. Pulse quick and feeble.

He had been in perfect health up to the time of the accident.

Next day the patient was still very uneasy, and evidently feebler. He was unable to micturate by voluntary effort, but felt much relieved by the frequent introduction of the catheter; on which occasions small quantities of slightly bloody urine were usually evacuated.

The evident indications of supporting the failing strength, and allaying the sufferings of the patient, were attended to, but without avail, for next morning he was found sinking fast, and he expired at six P.M.

17th.—On inspection of the fractured leg, it was ascertained that the posterior tibial artery was sound, but that the peroneal was ruptured.

On examining the abdomen, the bladder was found ruptured at its fundus, so as to admit three or four fingers. The rupture was transverse, *i. e.*, nearly parallel to the transverse diameter of the pelvis, and its edges were clean. A very small quantity of urine was found in the peritoneum; of which that part which lines the pelvis was much injected. A large quantity of blood was effused into the interior of the pelvis, and into the substance of the muscles at the lower part of the abdomen, as well as between them and the peritoneum. The other abdominal organs were healthy.

The right ilium was quite separated from the sacrum at the synchondrosis, and the pubic ligaments were also ruptured, so that the os innominatum was thus completely detached from the rest of the pelvis.

The first point of interest in these cases is the mechanical cause of the rupture in the coats of the bladder.

Dr Harrison, in his excellent paper "On Rupture of the Bladder," contained in the 9th volume of the *Dublin Medical Journal*, (page 349), states two causes of this lesion, to which he refers all recorded cases of it. His words are, "every case on record has been the result, either of some force directly applied against the abdomen, such as a blow or fall against some resisting body, or of a fall from a height, causing a general concussion of the whole frame." We think it will not be difficult to show that the cases which have just been read, resulted from neither of these two causes; and it will be equally easy to point out a different, but a very obvious and sufficient, cause for the lesions sustained.

In the first of them the force was not applied directly against the abdomen, but across the groin; neither does it seem that the bladder was ruptured by the general concussion, which, no doubt, was caused by the fall from the cart; for in all recorded cases of this last-mentioned kind, viz., those in which rupture has resulted from "a general concussion of the whole frame," the seat of the rupture is at the base of the bladder. This happens because, first, the base is the least distensible part of the organ, it being covered with peritoneum: and second, because the urine, which in these cases is the efficient cause of the rupture, is driven, by the impulse communicated to it, against the neck of the bladder and pubic bones, is by them resist-

ed and made to recoil against the base, where it operates, if the force be sufficient, by rupturing first the peritoneal, and then the other coats of the viscus. But the situation of the rupture in the case under consideration, was very different. It was in the neck of the bladder, and at that part of it, too, which exactly corresponded in position, extent, and direction, to the loosened part of the pubic bone. It ought to be remembered that the body of the pubis was completely fractured a little to the left of the symphysis, and that the corresponding ramus of the pubis and ischium was also fractured. This piece of bone, then, which corresponded so exactly with the rent in the bladder, was completely severed from its bony connexions, and might easily be, as we think it was, driven in upon the distended bladder. From a consideration, moreover, of the manner in which the external force was applied, and from the directions of the other fractures of the pelvis, formerly detailed, it will be readily perceived that a continuation, or, as really happened to this poor man, a repetition of the original force was quite capable, nay, most likely, to bear this fractured piece of bone against the part in which the rupture of the bladder was found after death. In explanation of the important bearing of these observations on the case in point, we need only refer to the history of it previously given.

In the second case, that of George Bell, the force which caused the rupture acted from behind. It is somewhat difficult, however, to determine precisely how the blow operated, whether by producing a general concussion of the whole frame, or by causing the bones to impinge directly on the base of the bladder. We think, however, that the nature of the accident and its obvious effects, sufficiently prove the latter supposition to be correct. It will be remembered that the hour at which the accident happened was late in the evening, when the man was about to quit his work for the day, and very probably he had allowed his urine to accumulate in the bladder for some time previously. It is not too much then, we think, to assume that the bladder was fully distended, when the man received the blow from the main shaft of the engine. This blow immediately detached the ilium from the sacrum on the right side, and ruptured the pubic ligaments, thus separating the os innominatum from the rest of the pelvis. Nor was the force of the blow exhausted in producing this separation; for the right margin of the sacrum was at the same time pressed forwards—a position which, owing to the nature of its other connexions, it retained even after death; thus indicating, with perfect certainty, the minimum extent of the indentation. Even apart, therefore, from a consideration of the violence of the blow, and the natural elasticity of the parts, such a change in the bony pelvis as was found after death, was amply sufficient to prove that the sacrum and the margin of the ilium had been brought into direct and violent contact with the base of the distended bladder. There is, however,

reason to think, that had this blow been the immediately efficient cause of the rupture, the rent in the bladder would have corresponded with the direction of the synchondrosis, and would not have been transverse. There seems to have been another link then in the sequence of events; and the history of the accident probably was, that the blow of the main shaft drove in the bones at the right synchondrosis against the base of the distended bladder, and that thus the violent impulse was communicated to the urine, which, by its rebound, produced the lesion in question.

In both of these cases, the accident was of a very severe kind, and the nervous shock proportionately great; but the progress of the cases was by no means such as it would have been had the bladder remained entire. The prominent symptoms in both cases were those occasioned by the lesions of the bladder; and yet their character and course were materially different.

As regards the case of the man M'Culloch, we, unfortunately, cannot speak with precision of the state into which he was immediately thrown by the accident, seeing that he was not admitted into the hospital until three days thereafter. But, judging from the severe antiphlogistic treatment which he appears to have required, and to have borne, his state must have been one much more acutely inflammatory than that of George Bell. The latter, indeed, never recovered from the nervous prostration caused by the effusion of urine into the peritoneum, combined with the shock of the accident. The latter, although severe, would not, we think, have been adequate to the effect produced, had not the former concurred with it. The patient in this case lived forty-five hours, and expired without exhibiting the slightest symptom of reaction. Such a termination of the case will not astonish any one, however, who remembers the effect of a foreign fluid in the peritoneum, especially when of an irritating nature, and suddenly introduced, as was the urine in this instance.

M'Culloch, the subject of the other case, lived for eighteen days after the accident. The "shock" which he received at first, was not indeed so severe as that under which the other man sunk, but the injury to the pelvis was much greater. The true explanation of the great difference in this respect between these two cases, is to be found in the fact, that in the latter the urine was not, as in the former, discharged into the peritoneum, but into the muscles beneath it. Inflammation, suppuration, and sloughing ensued; the tendency being towards a natural cure by external opening of the abscess. This effort of nature was duly aided by surgical interference; and there is little doubt in my mind, that this patient would have ultimately recovered, had he not fallen a victim to that mysterious disease phlebitis, which was then prevalent in the hospital. The commencement of this disease in M'Culloch's case, is noted in the report of the 24th, up to which time he had gone on improving. Consequently, at the inspection, it was found that one of those

peculiar purulent deposits, characteristic of that disease, existed in the right lobe of the liver, and that a quantity of matter was contained in the left acetabulum. This case, then, is in many respects similar to that of the child Nicholas Keog, recorded by Dr Harrison in the paper formerly referred to. Regarding this case, Dr H. remarks, that he thinks it might have been saved, had he sufficiently early used free incisions, so as to admit of the escape of the urine, and thus to check the diffuse gangrenous inflammation of which the child ultimately died. In his Pathology and Practice of Surgery, Mr Syme has lately recorded an interesting case of rupture of the neck of the bladder, from direct violence applied above the pubis, and without fracture of the pelvis. The skin was freely incised in the place indicated, the patient's strength was supported, and after a critical illness he recovered in about six weeks. This, we believe, is one of the only two cases on record, of recovery from rupture of the bladder. The true principle of prognosis in these cases, has been laid down by the same high authority, that "if the rupture takes place above or within the reflexion of the peritoneum, there cannot be the slightest chance of escape. But if the rent is at the anterior part, so as to discharge the contents of the bladder by a sudden gush into the cellular substance, and condense it in such a way that only the portion in contact with the urine may be deprived of life, it appears that the patient may be saved by timely incisions." This latter observation is equally applicable to such cases as that of M'Culloch, formerly related;—the only difference being in the position of the rupture, viz.—in the side rather than in the front of the neck of the bladder. Mr Syme, in penning the remark, obviously referred to cases similar to his own, in which no fracture of the pelvis existed; and, in these circumstances, the only part of the bladder which could suffer rupture without involving the peritoneum, is the anterior part of its neck. But in cases of fracture of the pelvis, a large portion of the sides, and of the inferior part of the bladder, is liable to rupture, while the peritoneum may not be injured.

In regard to treatment as well as to prognosis, cases of rupture of the bladder are divisible into two classes, viz., those within and those without the peritoneal sac. The great feature of treatment in the former class of cases is, incision for the relief of the extravasation, which is the immediate consequence of such ruptures. In all cases of this kind there will be some external mark of the phenomenon in question—such mark will be afforded by swelling, as in Mr Syme's case, or of that combined with crackling, as in the first of the two cases related in this paper. It is extremely unlikely that any such mark should be present if the bladder were ruptured within the peritoneum; for in that case extravasation would be prevented by the peritoneum; and, owing to the nature of the causes which generally operate in the production of such injuries, no apparent phenomenon may indicate rupture of the bladder. A

blow on the vesical region of the abdomen may be sufficient to produce this lesion, and yet no mark of violence may be visible on the skin; a circumstance which is remarkable in all abdominal injuries. In general concussions of the whole frame, on the other hand, the external marks of injury may be at a considerable distance from the seat of the most important lesion. It is fortunate, however, that in such cases, viz., those in which there is no external mark to direct the surgeon in making his incisions, the situation of the rupture being most likely within the peritoneum, there is no hope of relief from such practice.

The question now naturally occurs, What is the treatment proper for intra-peritoneal rupture of the bladder? or, Is there none? Dr Harrison recommends tapping of the peritoneum from the rectum; but the operation has not been tried. Certainly, if such an operation is to be at all successful, that must be the situation of the opening into the peritoneum. There could be no expectation of benefit from paracentesis in the usual place, where it was performed in two fatal cases of this kind, mentioned by Dr Harrison. This practice seems to me well worth trying in all such cases, in which the bladder is reported to have been much distended at the time of the accident. It can add nothing to the danger of the patient, and seems to give him the only chance of life. This opinion is strongly corroborated by an interesting provision of nature which has been found to occur in some of these cases, such as Gibson's case, related by Dr Harrison, and another mentioned by Dupuytren in the *Archives Générales* for June 1834. In both of these cases the lymph thrown out by the peritoneum had formed a pouch or secondary reservoir for the urine, by agglutinating the intestines and the fundus of the bladder itself to the walls of the abdomen. thus shutting out the pelvic cavity from the rest of the abdomen, Now, in these cases, if there had been some outlet for the urine, the inflammation would, in all probability, have subsided, and the men would have recovered. In other instances, however, a small quantity of urine having been at first effused, it seems to be rapidly absorbed in the early stage of the inflammation which it occasions. In these cases, then, no good could be done by operative interference of the kind alluded to. The *secretion* of the urine seems in them to be all but suspended, and therefore the passage of that fluid from its natural reservoir would not be sufficient to call for such procedure.

The difficulty of diagnosing between these two sets of cases, is often extreme: but perhaps the absence of every symptom except those of rapid failure of the vital energies, contrasts sufficiently with the phenomena, more or less apparent, of acute peritonitis, to enable the observer to distinguish, in most instances, those in which there is much from those in which there is little urine effused into the peritoneum. In the latter class of cases, the ordinary constitutional treatment of acute peritonitis—venesection, if the patient is seen

in time, the free administration of calomel, opiates, and warm water enemata, with fomentations to the abdomen and complete rest—must be relied upon alone: nor ought such means to be neglected in the former class of cases, whatever else may be done for the patient. It is but too likely, from the nature of these cases, that if the patient live long enough stimulants may be proper; but they should be given with great care, and watchfulness as to their effects. In all such cases the anxiety of the surgeon will naturally be directed to the prevention of the accumulation of the urine in the bladder, which, if it did occur, even in a small measure, might endanger a new effusion of urine into the abdomen. This indication will be fulfilled, so far as possible, by the frequent introduction of the catheter, or, if the patient bear it well, by leaving a gum catheter in the urethra, care being taken not to push it beyond the neck of the bladder, lest, by adding a new cause of irritation, the existing inflammation be increased or renewed.

ARTICLE V.—*Contributions to Toxicology. Cases of Poisoning, with Remarks.* By DOUGLAS MACLAGAN, M.D., F.R.S.E.—(Continued from p. 295.)

Acetate of Lead; Attempted Suicide; Imputed Poisoning.

ELLIOT MILLAR, cloth-finisher, Galashiels, and his wife, had been married for about thirteen years, but for two or three years had lived very unhappily together. One of the parties had communicated syphilis to the other, and, considering the otherwise indifferent character of the husband, there seems to be little room for doubt that he was the culprit in this matter.

Millar's bad usage of his wife had on several occasions induced her to leave his house, and seek an asylum under the roofs of her relatives, and had led her about two years previously to attempt to poison herself with sugar of lead. On Saturday, 3d July 1847, she again resolved to destroy herself, and for this purpose sent one of her sons to a laboratory to purchase a pennyworth of sugar of lead, the whole of which she dissolved in water, and swallowed before going to bed. The boy was instructed to say that the sugar of lead was to be applied to a sore; the quantity furnished to him was half an ounce; it was wrapped in paper, and labelled "Sugar of lead. Poison." This paper she put into her pocket, after swallowing the drug. During the night and following morning, she was sick, and vomited, but suffered no other inconvenience. On rising next morning, Sunday 4th July, she threw out the vomitings, and, as she slept in a separate apartment from her husband, he was not aware of what she had done.

She then prepared coffee for her own and her husband's breakfast. He drank two or three cupfuls of the coffee, she herself

drank two cupfuls at the same time, and another after he had finished his meal. This interruption to her breakfast, arose from his having reproached her with being more active at taking her meat, than in attending to her household duties. This was the beginning of a fresh altercation, in the course of which she remarked, that had she been that morning where she intended, she would not have been there to bear his abusive language, and thereupon she showed him the paper marked "sugar of lead, poison," and told him of her suicidal attempt. He made no further remark at the time than that he did not believe she had attempted suicide, and he left the house. He went up to the house of his brother, who lived above him, and told him of what his wife said she had done, and then said to him that he felt unwell, and believed that he had himself got the poison. His brother advised him, if this were the case, to go to the laboratory and procure a vomit. He accordingly went to the chemist's, showed the labelled paper, again expressed his belief that he had got the poison, and was accordingly furnished with a scruple of ipecacuan, and a grain and half of tartar emetic, with directions to return in half an hour if it did not operate. He went home, mixed the emetic in water in presence of his wife, and carried it into an adjoining room, where he said he swallowed it. He then went up again to his brother's house. The emetic said to have been taken by him, not having operated, nor produced even an appearance of nausea, he was advised by his brother and father to go back to the laboratory again, which he accordingly did, but not for more than an hour after the time when he got the first emetic. The druggist's assistant on this occasion asked him if he "felt a pain in his bowels, and a sweet metallic taste, and if he felt sick," when he said he felt the pain in his bowels, and a slight taste, but not sick. At this moment a medical man, Dr Hutton, happening to pass, was asked to give him the benefit of his advice. Millar, according to the druggist's statement, did not seem apprehensive of himself, nor did he appear to be suffering; when, however, Dr Hutton came in, he seemed "a little afraid." The doctor inquired as to his symptoms; but he said now he had none; "no headache, no pain, no spasms, no sickness; indeed, no symptom whatever." The doctor seems very properly to have attached no importance to the case; he bade the apothecary repeat the emetic, and left him.

Millar carried home his second emetic, and along with it some vinegar, part of which he drank himself, and he made his wife drink some also. This extraordinary antidote for acetate of lead seems to have been a prescription of his own. This second emetic was undoubtedly swallowed, and it operated, speedily producing so much nausea as to oblige him to lie down on his bed. He vomited into a basin, and this, with its contents, he placed, apparently for concealment, in a tub under a bed in a closet, where they were seen

by his wife next day, and left untouched by her. At the time when Miller first accused his wife of having poisoned him, and when the breakfast things were still standing on the table, he took the coffee-pot, and poured out a little of the coffee which remained into a cup. This he placed in a drawer, which he locked, and put the key in his pocket. To all his accusations, his wife protested strongly that she was entirely innocent of poisoning him, as she had no poison in her possession, except that which she had swallowed the previous night. Next day, Monday, July 5, a constable, who had been informed of Mrs Millar having attempted to poison her husband, came to the house, found Millar and his wife there, and got from the former the portion of coffee which he had locked up. The constable returned again in the evening, and then got the vomitings. Both of these he put into bottles, and, having secured them, marked them for identification. On the succeeding day, Tuesday, the procurator-fiscal came, and he, having made some inquiries on the subject, a warrant was obtained, and Mrs Millar committed to prison on the charge of having attempted to poison her husband.

The usual inquiries were now instituted by the authorities, with a view to bringing her to trial for her alleged crime. The prisoner, as usual in such cases, was herself examined on Tuesday the 6th July, and her declaration taken down, and afterwards signed by her. It was again read over to her on the succeeding day, some additional particulars added, and adhered to by her. It contained merely a statement of the facts mentioned in the above narrative, so far as known to her, and a strong protestation of her innocence. Her apparently candid and straightforward manner under examination, had impressed the procurator-fiscal and justice of peace who examined her, very favourably towards her; so much so, that the former, in reporting the case to another of the authorities, actually states his suspicion, that the sugar of lead, which by this time had been tested for, and found in the vomitings and coffee, may have been put into them after they were set aside.

Elliot Millar himself was on the same day, 6th July, examined in precognition as the principal witness against his wife. This evidence was a repetition of his statement as to her having given him the poison in his coffee, admitting, however, the fact, so favourable to the accused, of her having at the same time herself partaken of the coffee. He ascribed their domestic quarrels to her inattention to her domestic duties, and especially to her having contracted and communicated to him venereal disease. He admitted having struck her; but his whole evidence tended to gloss over his brutality towards her, and to magnify all the faults which he imputed to her. He assigned to the magistrates at this examination, as the ground of his belief of the present crime, her having tried to poison him with sugar of lead about two years previously. This statement, which pointed to the former occasion when she

had attempted to destroy herself, was clearly an untruth. At that time he accompanied her to a medical man, Dr Weir, for the purpose of procuring antidotes to the poison which she had taken; but he never breathed a syllable as to her having given any poison to him, and he sought no advice for himself, but only for his wife. Moreover, having occasion to see Dr Weir soon after the present precognition, when his wife was in jail, he gave to the doctor a quite different reason for believing her guilty. He made no allusion to any attempt with sugar of lead two years previously, because he knew Dr Weir to be cognizant of the facts as they then occurred; but he said that she had tried to give him prussiate of potash in his porridge some months before, his acquaintance with this salt arising from his knowledge of its employment as a dye-stuff. Dr Weir at once set down this story as a falsehood; but the inconsistency of his statements to the doctor in private, and to the magistrates at their examination, was not known at the time. Nevertheless, the general character of the statements made by Millar at his precognition, inspired the procurator-fiscal who examined him, with a want of confidence in their truthfulness. Among other suspicious circumstances eliminated, was the fact, that he had himself been in possession of sugar of lead as an application to his venereal sores shortly before this time; but he alleged, that, as he had found it useless to him, he had put it into the fire. The procurator-fiscal, struck with the peculiarity of the whole case, did not think it right to proceed further without consulting the legal advisers of the crown; and therefore he transmitted the various documents to crown-counsel for their opinion. In consequence of instructions from them, he again on 15th July examined Elliot Millar, who adhered to his former declaration of the 6th, and made one unimportant statement in addition thereto.

This being reported to the authorities in Edinburgh, instructions were sent to the procurator-fiscal, to subject Millar to a further examination still, which took place on 31st July. His former declarations were read over to him; but he declared at the outset, obviously with a view to cutting short his examination, that this was unnecessary, as he perfectly remembered all that he had said. To all the questions put to him he answered promptly and decidedly, and he showed very manifestly throughout, the desire to make out a case as strong as possible against his imprisoned wife. At length, towards the close of the examination, the procurator-fiscal asked him directly, if he did not himself put sugar of lead into the coffee on the Sunday morning. He said he did not. The question was pressed a second time, when, after some shuffling and equivocation about his telling nothing but truth, he at length confessed that, having still in his possession a portion of the sugar of lead which he had been using for his venereal sores, and which he had declared he had burned, he had, during his wife's temporary absence from the house, put a pinch of it into the coffee which he had

locked up in the drawer, and thrown the rest among his own vomitings.

This confession speedily made an important change in the relative positions of the parties. Mrs Millar was liberated, and Millar himself was committed to prison, eventually indicted under the wholesome but comprehensive Scottish common law, for "falsehood, fraud, and wilful imposition," and specially, for "wilfully, wickedly, and feloniously, accusing an innocent person to the public prosecutor, as being guilty of a heinous crime, for the purpose of preventing the administration of public justice." He was found guilty on both counts of the indictment, and sentenced to transportation. At the trial, before the Circuit Court of Justiciary, the testimony of the procurator-fiscal, and the rest of the general evidence, was so conclusive, that no important medical or chemical evidence was called for; and I was examined merely to identify my reports, and to prove the fact that there was sugar of lead in the coffee and vomitings.

Millar made an ingenious attempt to get out of the dilemma in which he found himself placed, by the confession of his fraudulent conduct being extorted from him. He forthwith averred that he had said this to the procurator-fiscal merely to get his wife out of prison, and he actually took credit to himself for having procured her liberation. But, unhappily for this pretended conjugal devotion, there was an awkward letter of his in existence, in which he threatened her, if she did not agree to certain requirements of his regarding his children, with being "immediately apprehended for leaving the house when he was at his work, and robbing it of different things which she had no right to carry away." He seems to have had a determined intention to get rid of her by *legal* means, in one sense of the word.

To preserve the continuity of the narrative, I have said nothing hitherto as to the chemical evidence in this case. The coffee and vomitings had been chemically examined on the 5th July, the day after the alleged poisoning, by Dr Macdougal of Galashiels, who reported that both were strongly impregnated with sugar of lead; and these articles were afterwards transmitted to me for corroborative analysis. A few drops of the liquid part of each sufficed for determining the presence of lead by the ordinary tests of sulphuretted hydrogen, chromate of potash, and iodide of potassium. Although, however, the presence of lead was all that I was called to report on at this time, I did not feel contented with determining its existence in the articles sent to me, but thought it right to ascertain its amount also. I was induced to do so from the consideration, that, as sugar of lead is a substance very frequently kept in families for purposes of domestic surgery, it would be a very likely defence set up by a person accused of using it as a poison, that some such thing as a lotion, or other preparation of it, had been taken or given by inadvertence; and I thought that a com-

parison of the quantity found in the coffee and vomitings with that in any lotion, supposing the strength of the latter to be approximately known, might furnish some evidence of the guilt or innocence of the accused.

The method which I followed for determining the quantity of lead present was the following:—The vomitings constituted a turbid white mixture, of sour smell, which, by rest, deposited a quantity of white flocculent matter, like coagulated milk, and some pieces of flesh, apparently salted meat, leaving a clear pale yellow liquid above, on which some fat floated. It was in a small portion of this clear liquid, that the tests formerly mentioned, so distinctly showed the presence of lead. The deposit from the vomitings obviously contained much of the metallic salt also, as it deeply blackened under the action of sulphuretted hydrogen. The whole was digested with a considerable proportion of pure nitric acid, and filtered; and to the filtered acid liquor, pure sulphuric acid was added. The sulphate of lead thus thrown down was washed first by subsidence, and afterwards on a filter. The solid matters separated by filtration, were collected and incinerated in a crucible, the ash treated with nitric acid, the liquor filtered, and precipitated by sulphuric acid. The precipitate was united on the filter with that obtained from the liquid portion. It was further washed and ignited, to destroy some adherent animal matter. As a portion of the sulphate was thus reduced to the state of sulphuret, it was carefully heated in contact with some nitric and sulphuric acid, and weighed. The quantity of sulphate of lead thus obtained was 86·77 grains, which corresponded with 109·09 grains of common crystallized acetate of lead—(Pb O $\overline{A}$+3 HO). A similar process was followed with the coffee, which had by rest deposited a heavy brown sediment, leaving a nearly colourless fluid above. The quantity of sulphate of lead obtained from the fluid ounce and a half of coffee sent to me, was 8·22 grains, corresponding with 10·3 grains of sugar of lead.

These results were duly reported to the authorities, who, in consequence of my directing attention to the quantity of the poison, submitted to me some further points for inquiry; the chief of these were the following:—

1. The cup from which Millar took his coffee being sent to me, to determine, what amount of sugar of lead would be contained in three cupfuls of the coffee (the quantity stated to have been swallowed by Millar), assuming it all to be impregnated in the same degree with the coffee analysed.

2. Keeping in view the fact, that the coffee sent to me was the last of five or six cupfuls which were in the coffee-pot, to state whether the cups first drawn off were likely to be more or less impregnated with sugar of lead, supposing a quantity of that salt to have been put into the pot while the coffee was making.

3. Having ascertained the quantity of sugar of lead which three

cupfuls would yield, to say, whether the vomited matters show more than could have been derived, or probably were derived, from such a source as the swallowing of the three cups of coffee.

4. On the assumption that any sugar of lead was added to the vomited matters after they were discharged from the stomach, to say, whether any test exists for distinguishing the result thus produced, from the result which would have been produced if the same sugar of lead had been infused in coffee first, and then swallowed and thrown up.

Some other questions were put along with these, but they were unimportant.

In relation to the first of the queries, I found, on measuring the cup sent to me, that when filled up nearly to the brim it contained $6\frac{1}{2}$ fluid ounces; that it could be made to hold $7\frac{1}{2}$ without overflowing; that I should estimate the average quantity which such cups would hold, when liberally filled, at 6 fluid ounces;[1] that supposing 10·3 grains of acetate of lead in a fluid ounce and half, to represent the proportion of that salt contained in the coffee generally, three cupfuls of six ounces each would give 123·6 grains, or about 2 drachms, as the quantity swallowed in the coffee. To this, I added, as reply to the third question, that the quantity contained in the vomitings was rather less than this, being 109 grains, and that the loss might be accounted for by some of the fluid having been used in testing for the presence of lead, as well as by some being absorbed from the stomach, and perhaps some lost in the act of vomiting. To the second query it was replied, that there would probably be rather more lead found in the coffee which remained at the bottom of the pot, than in the portions first poured off, on account of the partial subsidence of the insoluble compound formed by the lead with the vegetable matter of the coffee. That this, however, could not be to any great extent, because, if the coffee had been allowed to settle down thoroughly in the pot, its altered appearance must have attracted attention. It was, therefore, most likely, that the insoluble matter was kept suspended pretty equally through the coffee by agitation, and would remain suspended for some little time. Still, it was probable that the portion of coffee at the bottom of the pot would contain rather more lead than that first poured off.

The fourth query admitted of no reply but this; that I knew of no chemical means by which it could be determined that the sugar of lead had been added to the vomitings after ejection from the stomach. The fact, that the quantity found in the vomitings was correspondent with, and rather less than, the proportion contained

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* This estimate was founded on the supposition, that the cups would not be uniformly filled to overflowing, and that allowance must be made for the addition of milk, &c.

in the coffee, was favourable to the supposition that it had really been taken in this form.

I point out these details, because they afford an excellent illustration of the way in which one kind of evidence, if judged of by itself, might lead to erroneous conclusions. The correspondence between the quantity found in the vomitings, and the proportion which probably would have been swallowed in three cupfuls of coffee, such as that analysed by me, is a remarkable coincidence. Yet we know, by Millar's confession, that he did it roughly, by putting a pinch of the sugar of lead into the coffee, and then throwing the rest among the vomitings. Had the fluid ounce and half of coffee contained a few grains less, the disproportion between that and the amount found in the vomitings, would at once have convicted him of false dealing; but it accidentally happened that the proportions were such as to be fairly adducible in support of the truth of his statement. There can be little doubt, that had Millar's acknowledgment not been drawn from him by the acuteness of the procurator-fiscal, and had his wife been brought to trial for her imputed crime, with such an unscrupulous witness to testify against her, and his statements backed by the probabilities of the above chemical evidence, she could hardly have escaped condemnation for an offence of which she was totally innocent. But, to make the proof complete against her, it must have been necessary to bring out some additional facts, which could not have been easily substantiated. The statement made in the chemical report as to the effect of subsidence in altering the appearance of coffee containing sugar of lead, must have compelled the public prosecutor to prove that the coffee, at the time it was poured out for breakfast, must have been kept agitated in such a way, as to keep the insoluble matter formed in it, in a state of suspension. Every chemist knows that acetate of lead is the very agent employed to decompose and decolorize coffee, in preparing its characteristic constituent, caffeine. The precipitate which the lead salt forms in its infusion, if it is allowed to rest, subsides, and leaves a pale-coloured fluid, in no respect resembling that which people are accustomed to drink as coffee. To this fact the attention of crown counsel was specially directed; and a very simple experiment showed, that if Millar took his breakfast with ordinary deliberation, the altered appearance of the coffee must have attracted his attention.

One ounce of ordinary coffee, in bulk nearly half a tea cupful, was boiled for ten minutes in the coffee-pot used by Mrs Millar, with six cupfuls of water, the total quantity prepared for breakfast by her on July 3d. It was allowed to settle for five minutes, and two fluid ounces poured off into a bottle. The decanted portion was of the ordinary appearance of unclarified coffee, dark brown, slightly turbid, and depositing some coffee grounds.

Half an ounce of sugar of lead, being the same proportion to this bulk of fluid as was found in the coffee got from Millar, was

now added; the coffee was boiled again, and allowed to settle for five minutes after removal from the fire. Another similar portion, being decanted, was now found to be a clear transparent liquid, with hardly any colour, except a faint shade of green, and more resembling a weak infusion of green tea than coffee.

It was obvious, therefore, that if during the breakfast the coffee pot remained at any time at rest for five minutes, the next cupful poured off must have been so different in appearance from ordinary coffee, as at once to have attracted attention. The bottles, with the two samples of fluid prepared as above, were placed in the hands of the crown-officers, and inserted, as is required by the Scottish criminal law, in the list of productions to be used against Millar; but from the strength of the case against him in other respects, crown counsel dispensed with this evidence, which would have gone to prove, that it was, if not impossible, at least very improbable, that the acetate of lead could have been administered to him in the way alleged.

The taste of the sugar of lead, must also have attracted the notice, of any person who got such coffee, as that analysed by me. It is true, that, in reply to the druggist's direct question, Millar stated that he had felt a sweet taste. But coffee so impregnated with sugar of lead, has such a marked sweet astringency, that no one could have swallowed a spoonful of it, much less three cupfuls, without being stopped in so disagreeable a process of deglutition. Still less is it likely, that this would have passed unobserved, by a person who averred, that the very party now suspected, had tried on a former occasion to destroy him, with the very poison now alleged.

This case does not afford otherwise much subject for remark, but it seems worthy of being recorded, as a most atrocious and deliberate attempt to impute crime to an innocent party, deservedly punished.

In addition to the general, moral, and confessional evidence, by which Millar's fraud was brought home to him, I would specially indicate as worthy of attention, one feature in the case which appears to be of some importance, as characterising an individual falsely alleging himself to have been poisoned. I allude to his indifference and want of anxiety as to procuring medical aid for his own relief. In the case of Leith, who pretended to have got arsenic from his wife, [November No. p. 284,] it was observed, that instead of being anxious for medical aid, he seemed indifferent to it, and was not so alarmed as his visitor who found him sick in his shop. The same indifference characterised Elliot Millar. He went to his brother, and told him he believed he had got the poison, but he never seemed to think of getting any medical advice, till his relative suggested to him to go to the druggist's and get a vomit. That much he certainly did, but it is more than doubtful if he swallowed the dose. He ostentatiously mixed it in his wife's pre-

sence, but without any assignable reason, he carried it into another room, where he said he swallowed it, but it did not produce even the least nausea. It must be allowed, that it is possible that a scruple of ipecacuan, and a grain and half of tartar emetic, may have been really swallowed, and have produced no effect; but it is at least a suspicious circumstance, that when the dose was repeated about two hours afterwards, and undoubtedly swallowed, it not only produced full vomiting, but caused so much nausea as to make him lie down in bed. To this it may be replied, that this is no more than is often observed with fractional doses of tartar emetic repeated at two hourly intervals, where nausea and vomiting frequently follow a second or third dose, where the first has produced no such effect. Admitting, however, that he did swallow the first emetic, the very failure in the means to get rid of his poison, especially as he now declared to his father and brother that he felt much worse, ought to have increased his alarm. But nothing of the kind was remarked. He had been desired to go back to the druggist's in half an hour if his first emetic failed, but, instead of this, he allowed nearly an hour and a half more to elapse, and he did not go back at all till his friends urged him to do so. Such præternatural coolness contrasts strongly with the flurry and solicitude about medical aid, generally manifested by persons, who believe themselves to have swallowed any thing deleterious.

If we believe Mrs Millar's account of her own conduct, we have another, added to many recorded instances, of the comparative inactivity of acetate of lead as a poison. Half an ounce was the quantity she got, and she affirmed on oath that she swallowed the whole of it. It is true that no one saw her swallow it, nor did any one witness any serious symptoms produced by it; for she states that she took it at night, in a room where she slept in a bed by herself, and its effects had almost entirely gone off by next morning. Nevertheless, she was seen by her son to vomit in the morning before breakfast, without any other obvious cause; and, although she might have a motive for pretending to her husband to have attempted suicide, her established truthfulness in every other particular, entitles her to be believed in making this confession also.—(*To be continued.*)

ARTICLE VI.—*Some Account of the Practice in the Cholera Hospital in Surgeon Square.* By WILLIAM ROBERTSON, M.D., F.R.C.P. Ed. Physician to the Edinburgh Royal Infirmary and Cholera Hospital.

THE Cholera Hospital has been opened for little more than a fortnight, and although in this short time it has received 131 patients, it is of course impossible as yet to give a *fair* statement of the

deaths and cures, *i. e.*, a statement of the slightest statistical value. Of 131 admitted, 76 have died, 28 have been dismissed cured, and 27 (for the most part convalescents) remain under treatment. The proportion of female patients has exceeded that of males in the ratio of 88 to 43. Of 88 females admitted, 10 have been pregnant, 8 have been nursing, and 8 have been suffering from obstructed menstruation or other uterine complaints. Of those ascertained to be pregnant, 9 died, 4 having previously aborted; one, who likewise aborted, seems now well advanced in convalescence. It is remarkable, that in several of the cases occurring in nursing-women, the mammary secretion was not arrested by the attack of cholera. In three, the breasts ultimately became so much distended as to cause great inconvenience to the patients, and to require the use of the breast-pump at short intervals. Dr Douglas Maclagan informs me, that the milk of these patients differs very materially in composition from healthy milk,—that it is of low specific gravity, contains little or no butter, and, when examined microscopically, presents an unusually small number of milk granules. In several of the post-mortem examinations made at the Infirmary, appearances have been observed in the uterus and its appendages indicative of recent menstruation, and some of my patients have distinctly admitted that they were seized with cholera during a catamenial period. It seems worthy of further inquiry, whether there is any thing in the states of pregnancy, lactation, or menstruation, which renders females peculiarly susceptible to the influence of the choleraic poison.

The *poor* have, with a very few exceptions, been hitherto the sole victims of the disease in Edinburgh, and their liability to its attacks seems to be directly proportionate to their wretchedness. The dissipated have, as usual, suffered in greater proportion than the temperate. Cases occurring in habitual drunkards have been distinguished by great malignancy, and in several instances have immediately supervened upon the abuse of spirits. Poverty, filth, insufficient or improper food, residence in an overcrowded locality, or in one in which other cases have been observed, seem the most frequent predisposing causes. The question of contagion I shall for the present avoid, but think it right to state, that three nurses of the Cholera Hospital have already died of the disease. One of these women was a confirmed drunkard;—of the other two, one was of delicate constitution, but, I am assured, of sober habits; the other, to all appearance, a strong, healthy, and respectable person. In none of these three cases could I discover that there had been any communication with the sick, except in the hospital. The building in Surgeon Square is extremely well ventilated, regularly fumigated with chlorine, and in all respects very different from those miserable dwellings which cholera commonly visits.

Space will not permit me to give an account of the symptoms presented by my hospital patients; and as the object of this com-

munication is not to describe the *disease*, but the practice adopted for its mitigation, I shall pass at once to the head of treatment, under which certain symptoms will be incidentally considered.

Bloodletting has been tried in seven cases, in all of which the patient was seen early in the disease. In all these cases, there was vomiting and purging of the characteristic nature; in several, cramps were complained of; in all, there was a tolerably full pulse; in six, all of which are now either cured or convalescent, the temperature had not fallen much, if at all, below the natural standard; in one, blueness of the extremities had commenced; this last case was fatal. I have not practised venesection upon children, upon aged persons, or upon those who seemed of feeble constitution. The quantity of blood drawn has varied from twelve to twenty ounces; the operation has usually promptly relieved the patient from the cramps; and opiates given immediately afterwards have acted well. Collapse has either not succeeded, or has been very slight. I think that venesection, if practised early and upon suitable subjects, will be found to be at least *good* practice, probably the best that can be adopted. If, however, it be deferred till the algide symptoms have established themselves, the experience of most British practitioners condemns it as mischievous, and I am not disposed to question the accuracy of their conclusion.

The means of applying heat, to which recourse is commonly had in the cold stage of cholera, are tinned-iron vessels filled with boiling water, wrapped up in blankets and plentifully distributed round the patient's body in bed, frictions with turpentine, &c. These remedies seem always agreeable to the patient, and procure some mitigation of the cramps, but are seldom observed materially to restore the heat of the surface. A more powerful agent is the hot spirit-bath, which was often used in the Infirmary among our earlier cases, and, I believe, with very little good effect. A plan which I have adopted of late has been found more efficacious. A sheet wrung out of warm water is applied, as hot as the patient can bear it, over his whole body, including and closely embracing the limbs, and leaving no part of the person but the head uncovered. Over the sheet several blankets are tightly wrapped, or "packed," after the fashion of the hydropaths, but without the slightest respect for their pathology, or wish to imitate what they can with justice claim as their exclusive practice. Between the folds of the blankets, vessels full of warm water are disposed at intervals. The patient is then placed in a position which enables him to vomit over the side of the bed, and is supplied with toast and water, hot or cold, *ad libitum*. The remedy is an ancient one, often revived in modern times, and is to be regarded merely as a simple and powerful hot-bath. Whether it acts by restoring the healthy functions of the skin, by preventing evaporation, or by conveying fluids into a system from which they have been previously drained away, may possibly admit of question. It certainly seems to me, when applied in the case of children suffering

from the collapse of cholera, to be a most valuable and rapid mode of restoring the natural temperature. I have seen reaction established in a bad case within two hours after the application of the sheet. It is, however, generally necessary to continue the use of the remedy for six or eight hours. This practice seems less applicable to adults; the extreme restlessness, jactitation, efforts to vomit and to procure drink, usually observed in such patients, render it quite impossible to continue the application of the sheet for more than a few minutes at a time, without more constant nursing than the utmost vigilance on the part of the medical attendants can, in an hospital, insure. Strong patients commonly succeed, erelong, in disengaging their arms, and in throwing the bed-clothes off the upper part of the trunk, thereby exposing an extensive moist surface to evaporation, and totally defeating the object which we seek by the use of the sheet to attain.

Chloroform inhalations, it was not unnatural to suppose, would be found of considerable efficiency in at least removing the painful sensations of the patient during an attack of cholera. Most marvellous accounts of their efficacy in removing the most formidable symptoms of cholera have been published, and most unaccountable it seems to me, that in a large number of experiments undertaken while strongly prepossessed in favour of the remedy, conducted with considerable patience, and all the precautions conceived necessary to insure success,—conducted moreover in a public hospital, and before the eyes of many observers of acknowledged accuracy,—I should have totally failed in achieving similar delightful results. Truth compels me to state, that although chloroform has done much for the *comfort* of my cholera patients, it has done nothing for their *cure*. The cramps and urgent vomiting cease under its use, but recur whenever the patient awakes; and, although the soporific influence be maintained for hours by repeated inhalations, the result is still the same. The acts of vomiting and purging cease for a time, but the fluids are nevertheless still collecting in the stomach and bowels; the pulse becomes smaller and smaller, till it finally ceases to be felt at the wrist; the respiration becomes slower, the temperature of the surface sinks, as in too many cases of fatal collapse, and death closes the scene. I confess it seems to me probable, that chloroform inhalations, when administered during a profound state of collapse, may precipitate the fatal event by diminishing the frequency of the respiratory movements. While I deny the drug all claim to the title of a specific in confirmed cholera, I do not deny its efficacy in removing the earlier and more painful symptoms of the disease, nor decry the propriety of using it at any period when the cramps are severe.

The Russian physicians have strongly recommended the Persian Petroleum, and many trials of this substance have been made in the cholera hospital. The authenticity of most specimens of the drug to be met with in commerce, may with reason be suspected; but by the kindness of Dr Christison I have been enabled to use a sample

which is undoubtedly genuine. When given in doses of ten or twelve minims, immediately after the contents of the stomach have been ejected, either with a tea-spoonful of Tincture of Cardamoms, or suspended in mucilage, I have frequently found it of service in preventing the recurrence of *vomiting*, and believe that when this symptom is troublesome, the remedy is, at least, as certain as opium, acetate of lead, or calomel. In the *majority* of cases it has (like every other drug) failed, and certainly it has no specific action as a cure for cholera.

The injection of a saline fluid into the veins has, by some practitioners, been so strongly recommended, and, on theoretical grounds, seems so rational a practice, that I have not scrupled to resort to it in eight desperate cases. It has never seemed to me a sufficient objection to this practice, that a very large proportion of patients subjected to it die; and had I succeeded in saving a single individual of the eight, by injecting the veins, I should have thought myself fully justified in repeating the experiment. For of these eight cases, not one, according to all human probability, had the slightest chance of a prolonged existence, unless it were by the application of some remedy equally prompt and energetic in its action. Before the hospital in Surgeon Square was opened, the saline injection of the veins was thrice attempted in the Royal Infirmary; in one instance, I believe, with temporary benefit; but all three patients died within a few hours after the performance of the operation. One of these patients, an old woman in the last stage of collapse, fell under my charge, and had five pounds of the ordinary saline solution, recommended by the late Dr Mackintosh, injected cautiously, at a temperature of 98°, into the median basilic vein. The process lasted about forty minutes; the patient did not rally in the slightest degree, and died in my hands. It was not unreasonable to infer, that the sudden death in this case was the immediate consequence of the operation; yet dissection satisfactorily showed that *no air* had entered the heart or veins. In the eight cases treated in Surgeon Square, the solution used consisted of Muriat. Sodæ, ʒss., Bicarb. Sodæ, ϑiv., and Phosph. Sodæ, ʒss. dissolved in ten pounds of water, and then filtered. Care was taken to keep the solution at a temperature of 110° to 115°, and all reasonable precautions were adopted in the performance of the operation. The instrument used was an ordinary Read's syringe, and it was carefully cleaned before and after each injection. If this precaution is not observed, a quantity of *copper* must be injected along with the first portions of the fluid. A little difficulty was sometimes experienced in avoiding a certain degree of injection of the cellular tissue of the arm. The best mode of preventing this accident is to puncture the vein as in ordinary venesection, without any preliminary dissection, to push the silver tube into the vein gently, until the cross plate rests accurately upon the skin, and, above all, not to hurry the process of injection. In only two of my eight cases was even temporary amendment observed,—all died within fourteen hours. The

quantity of fluid injected varied from two to five pounds. These results have been sufficiently discouraging, and have led me seriously to consider if a repetition of the practice is advisable. Cases of the most unpromising aspect do occasionally rally, and get well after a collapse so profound as to leave no reasonable hope of recovery. I may instance a young woman named Hutcheson, whose case seemed quite desperate, and to admit of no rational remedy but injection of the veins. *No suitable vein could be found*, yet the patient is now quite convalescent. Are unexpected recoveries like this more frequent than cures effected by the saline injection of the veins in equally desperate circumstances? Till this question be resolved, I think we may still occasionally attempt a cure by the operation.

The treatment applied to the different stages of the disease, has been briefly as follows. In the earliest stage a mustard-emetic is given, followed by the use of the warm foot-bath, by full doses of opium with calomel, and the application of dry heat in bed. I fear it is not usually possible to cut a case of cholera short, even when its early symptoms are attended to; certain it is, that the above treatment failed in the cases of three hospital nurses already referred to. Another nurse, who had been vomiting and purging at intervals for some hours, had her symptoms promptly relieved, and was subsequently rather severely salivated by the calomel taken; I am however not satisfied that her case was one of cholera. When the early symptoms are unequivocal, and especially if the cramps are severe, a full blood-letting is practised, provided the pulse, age, &c., of the patient do not counter-indicate the remedy. The bleeding is followed by full doses of opium, with or without calomel. In by far the greater number of cases brought to hospital, collapse has already come on. In this stage the application of dry heat, frictions with turpentine, sinapisms to the epigastrium and legs, the moderate use of stimuli, and occasionally the hot wet sheet, are chiefly trusted to. In these different stages of the disease, a number of other remedies are from time to time used to palliate certain symptoms, *e. g.* chloroform, petroleum, calomel, opium, acetate of lead, tannin, and other astringents. Enemata, containing acetate of lead and opium, are occasionally thrown into the rectum. From the earliest period of the disease, I have usually prescribed a mixture containing camphor, cardamoms, and nitric æther, and the hourly use of some saline powder, such as Stevens', with abundance of diluent drinks. These remedies may be useful as stimulants, as diuretics, and as likely to supply to the blood some of the important constituents of which, during cholera, it becomes so rapidly deprived. I must add, that, although in deference to the experience of other physicians, I have given calomel to most of my patients and in various doses, I have never seen any good effect result, which might not be fairly attributed to the opium with which it was combined. The *scruple* dose of calomel has seemed quite inert, when given with the view of checking vomiting. During the stage of reaction the saline powders

and diuretic mixture have been always employed; opium is at this period either altogether abandoned or very cautiously given; moderate doses of Ol. Ricini have been found of service.

The return of the urinary secretion is always looked forward to with much anxiety; it seems in general the first trustworthy symptom of convalescence. When urine is long suppressed, the patient passes gradually into a drowsy state, accompanied with slow respiration, with a peculiar noisy delirium and jactitation, with rigidity of the extremities, grinding of the teeth, sometimes with general convulsions, and terminating in death by coma. Analysis of the blood in these cases shows the presence of *urea* often in considerable quantity. The first urine excreted during convalescence often contains albumen, but in a few days this principle can no longer be detected. The amount of *urea* in the earlier portions of urine is but small; and as this substance is afterwards more abundantly eliminated, the patient returns to his former state of health. There seems consequently a strong indication for the use of diuretics at all periods of the disease, and more especially about the period of reaction. The well-known property, possessed by colchicum, of increasing the amount of urea in the urine, suggests its employment when the comatose symptoms commence to manifest themselves, and I trust erelong to be able to report upon its efficacy.

16*th Nov.* 1848.

Part Second.

REVIEWS.

Leçons sur les Phénomènes Physiques des Corps Vivants. Par C. Matteucci. Paris: 1847.—*Lectures on the Physical Phenomena of Living Beings.* By Carlo Matteucci, Professor in the University of Pisa. Translated under the superintendence of Jonathan Pereira, M.D., F.R.S. London: 1848.

Elements of Natural Philosophy; being an Experimental Introduction to the Study of the Physical Sciences. By Golding Bird, A.M., M.D., F.R.S., F.L.S. London: 1848.

There is not, perhaps, any branch of physical science to which we are more indebted for just conceptions of the phenomena of life, than to electricity. The great similarity between the effects of dynamic electricity or galvanism and the nervous influence, was made so irresistibly evident by the celebrated experiments of the philosopher of Bologna, that numerous investigations were set on foot to determine the identity of the two forces. The researches prosecuted for this purpose by Galvani, Volta, Aldini, Humboldt, and others, though they failed in elucidating the point towards

which they were directed, gave a considerable impulse to the study of animal excitability, and led to the discovery of a multitude of facts in the physiology of the muscular and nervous systems, to the knowledge of which it is probable we might never have otherwise attained. Having thus early become connected with the science of medicine, the introduction of galvanism as a therapeutical agent soon followed, and many wonderful accounts of its efficacy in this department of the healing art were published. Its application, however, was then, and still remains, in a great measure empirical; nor can we expect it to be otherwise, while the prosecution of electrical science continues to be so generally neglected by the physiologist and medical practitioner. It is almost impossible to turn over the pages of our medical literature without being struck by the strange blunders made by writers on electricity. Thus we have seen the action of nerve on muscular fibre explained by comparison with the helix of an electro-magnet. A high authority informs us, that the brain may be the organ of secretion of electricity, "a voltaic pile," on the principle of Zamboni or De Luc, "discharging itself at intervals along the course of the nerves which communicate with the nerves of the heart, and thus exciting its pulsation." In another place, we find a writer gravely describing the effects of a *current of electro-magnetism*, little dreaming that he might with as much propriety write of a current of hydrostatics, as "electro-magnetism" is merely a term expressive of a class of phenomena observed to proceed from the mutual agency of electricity and magnetism. Even the translator of Matteucci's lectures has, at p. 209, while Dr Pereira somewhat relaxed the vigilance of his "superintendence," placed a torpedo between the sole and cover of an electrophorus with the expectation of obtaining a spark from the fish. Many other instances might be adduced indicating the propriety of authors making themselves familiar with the laws, or at least the terms, of this science, on which they sometimes find it necessary to write.

We believe that a work on electricity, explanatory of its physiological and therapeutical action, in the different states in which it is developed by frictional, magnetic, electric, and voltaic apparatus, would, at the present time, be acceptable to the profession. The manual of Dr Golding Bird is perhaps the best guide we have to the study of physical electricity; but even in it we look in vain for clear practical directions as to the administration of the voltaic current, or the relative intensity of different kinds of batteries; and he still maintains, regardless of the researches of Professor Henry on induced currents, that the common electro-magnetic machine affords shocks both in the opening and closing of the circuit of the battery current. In Dr Pereira's translation of Matteucci's lectures, and the excellent chapter on galvanism by Dr Bird, will be found a fair summary of the present state of our knowledge with regard to electricity in its relations to physiology; and we avail ourselves of the

rich mine of facts they contain, to place a concise sketch of this department of science before our readers.

Electro-physiology may be most conveniently studied in two divisions, the first being confined to the description of phenomena attendant on the development of electricity by living animal structures; the second, to the effects produced on the same structures by electrical agencies, either generated in themselves, or derived from external sources.

For some time past, the application of the term "animal electricity," has been confined to the effects produced by certain fishes furnished with organs adapted to the transmission of the electrical discharge, through conducting bodies placed in contact with them. The researches of Matteucci, and others, have rendered it probable that the excitation of this force is intimately connected with the exercise of the nutritive function, especially with the chemical changes accompanying the active processes of destruction and reparation going on in the living muscular tissue. These researches have, to a certain extent, revived the doctrines formerly enumerated by Galvani and Valli, who held that the well-known contractions which occur on making a communication between muscles and their nerves by metallic arcs, were due to the accumulation and discharge of a peculiar fluid existing in the organs themselves, and ascribed to the exterior of the muscle a negative, to the interior and its contained nerves a positive, state of electricity. Galvani, soon after the promulgation of his first experiments, observed that contractions were caused in the leg of the frog by merely bringing the muscles of the limb in immediate contact with the shoulder, after having removed all connexion between the upper and lower extremities except the sciatic nerves; and it was afterwards established by the researches of Humboldt, Aldini, Pfaff, and Müller, that the same contractions occur whenever the trunk of the sciatic nerve, dissected and insulated from the limb, but still organically connected therewith by its branches, is brought into contact with the muscular parts which it supplies, either immediately, or by the aid of a substance capable of conducting electricity; the member thus both supplying the motive force, and constituting a galvanascope capable of indicating its effects. So far it was manifest that a peculiar force resided in the neuro-muscular apparatus, the disturbance of which was attended with contractions in the muscular part of it; but the proof, that such force was really identical with electricity, was reserved for the researches of Nobili. This philosopher observed, on placing the posterior extremities of a frog, prepared as by Galvani, in two glasses of water, so that the lumbar nerves were immersed in one while the legs occupied the other, that the galvanometer indicated the circulation of a current directed from the legs to the nerve, when the extremities of its wires were plunged into the glasses. M. Matteucci, who has confirmed and extended

the experiments of Nobili, succeeded in augmenting the intensity of the current, by employing, instead of a single animal, a pile or battery formed of the extremities of a number of animals; this was effected by causing "the nerves of the first pair of extremities to touch the legs of the second, and the nerves of the second the legs of the third, and so on." He has further ascertained that the current thus generated, which had derived the appellation of "the proper current of the frog," from its having been previously observed in that animal only, may be detected in all other animals.

A current, apparently distinct from that already described, has been detected by M. Matteucci circulating from the exterior to the interior of the muscles, to which he has given the name of the "muscular current;" it may be readily observed by making an incision into the muscle of an animal, alive or recently killed, and conveying the poles of a very delicate galvanometer, the one to the surface of the muscle, the other to the interior of the wound. When a galvanometer is not at hand, the prepared frog's leg may be employed in its place; for this purpose, the muscles and bones of the thigh and pelvis are to be removed, the nerve left as long as possible, and the limb enclosed in a tube of varnished glass. On drawing the nerve across the edge of an incision made in the muscle of a living animal, the "galvanoscopic frog" will contract whenever the nerve touches both interior and exterior of the muscle.

A pile may be formed of the muscles of frogs' thighs, by dividing a number of them in the middle, and alternately connecting their exterior and interior surfaces; the muscular current from such an arrangement increases in intensity with the number of alternations. M. Matteucci has thus succeeded in producing, not only considerable electro-magnetic effects, but also the decomposition of solutions of iodide of potassium, and, with the aid of a condenser, deflections of the gold leaves of an electroscope.

In piles formed of the muscles of animals belonging to different classes, the current possesses an intensity greater as the animal ascends in the scale of being; its duration, on the contrary, is in proportion directly inverse. Thus the weak current generated by the muscles of fishes and reptiles is perceptible for several hours after death; whilst the comparatively powerful one, derived from the muscles of birds and mammals, cannot be detected after the lapse of a few minutes. A considerable influence is also exercised on the pile by the organic condition and temperature of its elements, muscles, the nutrition of which has been checked in any manner, or which have been submitted to a very low temperature (32 Fahr.), being capable of affording only slight and brief evidences of electricity. A similar deterioration of the muscular current is noticed in animals which have been destroyed by asphyxia, and still more so by sulphuretted hydrogen; while it seems to be unaffected in those poisoned by carbonic and hydrocyanic acids, arsenu-

retted hydrogen, or narcotics; in muscles which have been the seat of inflammation, or which have been taken from well-fed animals, the muscular current possesses more intensity, and continues for a longer period.

M. Matteucci is led to believe that the muscular current is independent of the nervous system, from his having formed piles equally efficient with muscles deprived of their nervous filaments with the utmost care; and he considers that its excitation is entirely due to the changes of electrical equilibrium accompanying the chemical actions of the nutrition of the muscular fibre,—that these in the living organism being molecular, it is impossible for the electric current so excited to *circulate* in the muscular masses in the natural condition of the parts; and it is only when the interior of the muscle is placed in artificial communication with the surface, which does not suffer the same chemical action, that any evidence of the existence of a current can be observed. M. Matteucci is also of opinion that the "proper current" has a community of origin with the "muscular current," from his having ascertained, by removing the thighs and spinal nerves from the elements of a pile adapted to the generation of the "proper current," that it was equally powerful, though only derived from the tendinous muscles of the leg; and further, he discovered that in all muscles which terminate in a long thin tendon, as the gastro-enemii of frogs, and the pectoral muscles of birds, a current circulates directed from the tendinous extremity to the muscular surface. Now, it has been demonstrated by Bowman and other anatomists, that the tendon becomes continuous with the elementary muscular fibres at the point where the sarcolemma ceases to invest them; the tendon and muscular fibre may, therefore, be in the same electric condition, and a communication made between the former and the sarcolemma probably constitutes a medium of conduction to the "muscular current."

Electric currents have been detected in living animals by MM. Favio and Zantedeschi, passing from the cutaneous tissue to the cerebro-spinal axis, which they have called "the electro-vital external currents," and others, "electro-vital internal currents," directed from the cerebro-spinal axis to the organs situate beneath the skin. They did not succeed in obtaining evidences of the connexion of electricity between different parts of the same viscus, or from viscus to viscus; but M. Matteucci has observed a considerable deflection of the galvanometer, by applying its wires to plates of platina, the one placed within the stomach, the other on the surface of the liver of the rabbit. On dividing the nerves and vessels as they passed through the diaphragm, the needle receded from 20° to 3°, and retired to its normal position, when the head of the animal was suddenly struck off. M. Favio and Zantedeschi assert, that pain and weakness have the effect of suspending the electro-vital currents, and in some instances of altering their direction.

Hitherto we have considered the production of electricity chiefly by the nutritive processes of the muscular tissue, as rendered manifest by purely artificial arrangements; we have now to direct the attention of our readers to phenomena accompanying the development of the same force by animals naturally furnished with a peculiar apparatus for that purpose—electric fishes. This department of animal electricity has been most successfully investigated in the several varieties of the torpedo and the gymnotus electricus.

The electrical organs of the torpedo are composed of numerous prisms, closely compressed together in flattened masses resembling honeycombs, and occupy the whole thickness of the fish on either side of the head and branchiæ, the bases and apices of the prisms corresponding to the animal's lower and upper surfaces. Each prism is formed of a single row of superimposed vesicles, containing a dense albuminous fluid, and is divided by numerous horizontal septa where the walls of the neighbouring vesicles are in contact, while between the two layers of the septa thus formed, there is extended a network of nerve tubules, repeatedly anastomosing with each other, derived from ramifications of the trifacial and pneumogastric nerves, distributed over the walls of the prisms. The organs in fact consist of a congeries of vesicular bodies, each of which is an elementary electrical apparatus, perfect in itself, and capable, when removed from the prism to which it belongs, of manifesting the signs of discharge. M. Matteucci has frequently caused contractions in the "galvanoscopic" leg by the application to its nerve of a portion of one of the organs not larger than a pin's head. Now the number of prisms in the organs of a large torpedo is estimated by Hunter to be about 1000, each of which contains 150 vesicles; if, therefore, we believe with M. Matteucci that the discharge is proportional to the number of vesicles, we can easily understand how the current acquires the high intensity which it possesses. We think, however, that the intensity of the current ought, judging by analogy with the effect of the number of alternations in the voltaic battery, to be proportional to the number of vesicles in each prism, rather than to the whole number contained in the organs.

Our previous ideas of the excitation of electricity, give us little assistance in arriving at a conclusion as to the mode in which it is effected in the organs of the torpedo. We find it difficult to understand how a mass of nervous and muscular matter can either act as the generator of a current, or as the condensing receptacle of electricity derived from the nervous system. M. Matteucci suggests that the vesicles are thrown into a state of polar tension by the nervous influence, in a manner analogous to the polarization of the tourmaline by heat, an hypothesis which, though one of considerable ingenuity, is not quite original, as a somewhat similar suggestion may be found in the Cyclopædia of Anatomy and Physiology, vol. i. p. 54.

The function of the electric organ is entirely dependent on the integrity of the nerves supplying it and the centres from which they arise, which appear to be the ganglia of the vagi and trifacial nerves, so largely developed as to have obtained the names of the electric lobes. By removing the whole of these parts from the animal, and carefully preserving their connexions with each other, it is proved that the integuments and cartilages by which they are surrounded in no way influence the discharge; it continues after the organ has been sliced and pierced in various directions, and it is only by the gradual death of the part, or the destruction of its chemical constitution by the application of heat or acids, and the consequent coagulation of its albumen, that its peculiar manifestations cease to exist. On the other hand, the section or ligature of any of the nervous branches is attended with instant *paralysis* of that part of the organ to which it is distributed; and any lesion of the electric lobe, after causing violent discharges, is followed by complete destruction of the electrical function.

The effect of voltaic currents on the nerves is very remarkable, as showing the analogy existing between the discharge and muscular contraction. When the poles of a battery are applied to the nerves of an organ recently removed from the animal, a discharge takes place both on opening and closing the circuit, independently of the direction of the current through the nerve; but when the vitality of the parts is weakened, the shock occurs only on the closure of the circuit, when the current is made to traverse from the cerebral to the peripheral part of the nerve, and on the interruption of the current, when the electricity is passing in the opposite direction. By referring to the action of the current in mixed nerves, it will be found that muscular contraction takes place under similar circumstances.

The current of the torpedo is capable of producing all the usual effects of voltaic electricity, viz. the heating of metallic wires, chemical decomposition, the excitation of magnetic polarity in ferruginous bodies, and the phenomena of the electric spark and of muscular contraction. During the discharge the whole of the opposite surfaces of the fish are thrown into a state of electric polarity, the dorsal surface becoming positive, the ventral negative; but the galvanometer indicates that the poles or points of greatest activity correspond to the opposite surfaces of the electric organs, the apices and bases of the prisms; weak currents may, however, be detected passing between any two parts of the integuments, differing in their respective distances from the organs, and even between different parts of the organs themselves.

The electric organs of the gymnotus consist of masses of prisms similar to those of the torpedo, but having a direction coincident with the long axis of the animal's body: hence the poles are situated at the head and tail,—the cephalic pole being positive with regard to the parts of the body situated towards the caudal ex-

tremity. The current of the gymnotus differs from that of the torpedo only in the possession of a much higher intensity, due to the greater length of the prisms contained in the organs of the fish, and the consequently greater number of vesicles polarized in series. We cannot but receive with some hesitation the statement of Faraday, as quoted by M. Matteucci, that the shock of the gymnotus is equal to a highly-charged battery of Leyden jars, having an united surface of 3500 inches; the discharge from such an apparatus would fuse a considerable length of iron wire, a feat surely beyond the powers of the gymnotus.—(*To be continued.*)

A Course of Lectures on Dental Physiology and Surgery, delivered at the Middlesex Hospital School of Medicine. By JOHN TOMES, Surgeon-Dentist to the Middlesex Hospital. London. 1848. 8vo.

WE have intended for some time past to notice this work, and to devote to it a space in our pages which we conceived to correspond with its great excellence, and the originality and importance of the author's views. As we find, however, that objects of more immediate interest have occupied our pages entirely for the present, we must be content with giving our readers a general idea of the style and scope of these lectures, which we have regarded, ever since their publication in the *Medical Gazette*, as a model of what such a course of instruction ought to be. As treated by Mr Tomes, the science and art of the dentist are made mutually to illustrate each other; and, in both departments, he is not only *au courant* with the present state of our knowledge, but prepared to impart to us many new facts and views, the result of patient study and long-continued observation. It is the constant enunciation, and equally constant practical application, of the highest truths of science, which gives to this work a tone so far superior to that of the majority of publications on practical dentistry.

The first two lectures are occupied with an account of the anatomy of the teeth and alveoli. The third, fourth, and fifth lectures give the results of his researches on the minute structure and development of the different tissues composing the teeth; together with the views of Mr Owen on this subject; and a summary of the researches of Mr Goodsir on the development of the pulps and sacs of the teeth. The investigations of Mr Tomes on the development of the dentine, and its analogy with bone, are exceedingly interesting; and we shall place a summary of them before our readers in an early Number of the *Retrospect*. The sixth lecture treats of the eruption of the temporary and permanent teeth; and concludes with some excellent remarks upon the adaptation of the teeth and hard parts of the mouth to their various functions:—

"In taking a retrospective view of the anatomy and physiology of the teeth, we are forcibly struck with the adaptation of these organs to their peculiar

functions. Early in life, when the jaws are small and comparatively powerless, the small and more delicate milk teeth are provided by nature, to perform the necessary office of mastication. When the body has increased in size, and the jaws have become larger and more powerful, the smaller teeth are, by a natural process, removed, and a stronger and more numerous set is developed, with which we are enabled to masticate the more solid food required for the sustenance of the adult.

"Like the bones generally, the teeth are required to support mechanical resistance, and in them, too, we find the structure admirably adapted to this end, but in the teeth the mechanical force is direct in its application; and in them we find a peculiar arrangement of osseous structure, enabling them to withstand without injury the force so applied. And, as a further adaptation, the teeth are endowed with a lower degree of organization than the bones, and are thence less susceptible of injury; while the latter, clothed with soft tissues, and capped with cartilage, are thus defended from the more immediate effects of mechanical force.

"The teeth are said to possess in themselves no power of reproduction by which an injury can be repaired. This is not strictly true. The injury to which, in a state of nature and health, they are most liable, is the wearing away of the masticating surface from use. The worn surface certainly is not renewed, but the teeth increase in density, and the pulp-cavity diminishes in size by the formation of dentine, so that the actual amount of dentine is not diminished, while the density is increased. In each of these actions we may recognise a form of renewal which in some degree compensates for the loss by abrasion. If the whole act of mastication is from any cause thrown upon two or three teeth, then these naturally, by the excessive use, wear away, till at last the whole crown is exhausted. Then, again, they make an effort to resist the inroad of caries, as I will show you when we come to treat of that destructive disease.

"*The teeth are important as organs of articulation;* so much so, that, when lost, we can scarcely make ourselves intelligible. The physiology of the teeth, when limited to this use, is well worth investigating, had we time at our disposal; but, before leaving this part of our subject, I may draw your attention to a few curious facts, in connexion with the form of the mouth, in those possessed of a good voice, and with power to use it in song.

"Wherever you have a fine clear sonorous voice, you will find well-formed and well arranged teeth; each tooth will occupy its proper place. But, what is perhaps still more important, the hard palate will be well formed; that is, it will present a section of a large arch, perfectly free from contraction either from side to side, or from before backwards. There will not be a deep vaulted form, neither will there be a sudden elevation immediately behind the front teeth, so common in those who speak with indistinctness—on the contrary, the palate will rise gradually.

"The mouth, and its dental appendages, are not of the first importance in relation to the voice, yet they are highly important as auxiliaries, and, as such, their condition should not be lost sight of.

"The vocal organ may be good, even first-rate, but the mouth and teeth must be well formed, or the voice will be injured in its passage through the mouth."

Lectures VII. to XIV. are on the diseases of the teeth, gums, and alveoli. The author's opinions on these subjects are very distinct and practical, and apparently the result of great experience. He has collected some valuable information on the liability of the different teeth to caries, by recording the cause of extraction in a large number of individual instances; and has thrown the results in the present work into a tabular form. In regard to the treatment of toothache we find the following suggestion:—

"Most of you know that an ulcerating or a suppurating surface sometimes becomes extremely sensitive and painful, and that immediate relief is obtained by brushing over the surface of the sore with a weak solution of nitrate of silver. A similar condition, I apprehend, now and then obtains with the exposed surface of the dental pulp, for we occasionally find that a solution of nitrate of silver, gr. iij. or gr. iv. to the ʒj. of distilled water, will allay toothache after the failure of what at first seemed more promising treatment."

Lecture XV. is on the operations practised on the teeth; and Lecture XVI. (the last) is on artificial teeth. The author is averse to the use of chloroform in extraction of teeth, conceiving that the effects of the former are sometimes more distressing to the patient than the transitory pain of the latter. We conceive that the patient ought in this matter to choose for himself; and we have no hesitation in saying, that, for our own part, we should not be quixotic enough to undergo the pain of a wrench from Mr Tomes' instrument, while we had the means of annulling it.

We close our extracts with the following remarks upon stopping decayed teeth. After describing the mode of removing the softened dentine, he says,—

"Gold or tin foil are the best materials for making plugs. Whichever of these be chosen, the method of use is the same.

"There are four methods of introducing foil for making a plug. In one the metal is folded into narrow strips, proportioned in width and thickness to the size of the cavity. One end of the strip is, by means of a conveniently shaped stopping instrument, pressed to the bottom of the cavity. The strip is then bent, and a fold passed to the bottom of the hole, leaving the first fold projecting above the surface. Fold after fold is introduced, till the cavity is tolerably full. A wedge-shaped instrument is then introduced, and the gold pressed towards the walls of the cavity; more gold is, by a similar process, pressed into the cavity so obtained. This process is repeated till the wedge cannot be forced into the plug. A flattened instrument is then used to compress the gold in the cavity. When we can make no further effect on the surface of the plug by compression, the surface is filed smooth and burnished. By a careful adherence to this plan, we make a plug composed of layers of metal, arranged parallel to the walls of the cavity, and therefore not liable to fall to pieces or come out. But, on the other hand, had we made the folds at a right angle to the walls, and parallel to the bottom of the cavity, layer after layer would have peeled off, till little or none of the plug remained, and the decay would have proceeded to the destruction of the tooth.

"In the second method, a piece of foil of sufficient size is rolled hard, and spherical between the thumb and finger. This is gradually forced into the cavity, care being taken to get it well in round the outer walls. When the plug has been rendered as solid as possible, the superfluous portion is cut or filed off, and the surface burnished.

"The third method of using metallic foil is a combination of the two preceding ones. A piece of foil that will readily go into the cavity is rolled up loosely. When in its place, a wedged-shaped instrument is passed into its centre, which has the effect of spreading the gold towards the walls of the cavity. The centre is gradually filled with folds of gold in the manner I have described. The wedge is used again and again, till it can no longer be made to enter. The gold is then compressed on the surface, and the superfluous portions removed, and the surface burnished. When the plug is finished in either of the manners I have described, the circumference should be examined by a sharp steel probe. If this can be made to enter at any part, the hole so made

should be enlarged by thrusting in an instrument as large as can be introduced, and the hole filled.

"In the fourth method of plugging, the foil is rolled into short lengths, proportioned to the depth of the cavity to be plugged. These, with the assistance of a fine pair of forceps, are packed into the tooth, much in the manner you would proceed to pack cigars into a tumbler. A wedge-shaped tool is from time to time thrust between the length of the foil, to force them towards the walls of the cavity. When the tool can no longer be made to enter, the surface of the plug is cut level with the surface of the tooth, and burnished.

"Either of the foregoing methods of plugging will answer, if well done. But, of these, I prefer introducing the metal in folds. The situation of the cavity, and also the size, will have something to do with the selection of the plan of operating. Then, again, one person will be more apt at one manner of procedure than another. All these matters of detail must be learned in practice."

The work is very handsomely printed, and the illustrations are admirable.

Report of the Proceedings of the Pathological Society of London. Second Session, 1847–48.

We are happy to welcome a second fasciculus of the Reports of this most important and useful Society; and still more happy to be enabled to say, that it appears to be gaining materially in strength as it proceeds. We can well imagine the difficulty of bringing a popularly constituted society such as this into good working order; a difficulty arising quite as much from the enthusiasm of some of its members, as from the lukewarmness of others. It would be too much to expect that so young an association should be able from the first to temper the zeal of all its members with that discretion which is only the fruit of experience, or raise the scientific character of its contributions to the level that might justly be expected from an older or a more exclusive institution. But we are persuaded from the signs of life which it has already shown, that the Pathological Society will go on improving in this respect from year to year; and when we consider the vast materials which it has at command, we cannot doubt that its transactions will become one of the greatest repositories of important pathological facts and doctrines in this country, or in the world.

In order, however, that this desirable end may be accomplished, it is above all things necessary, that the facts which occupy the attention of the society should be collated and discussed with a view to the elimination of *principles;* and that they should be placed on record in such a form as shall tend to make them useful, not only to those who have seen the objects described, but to the medical public at large. These desiderata can only be accomplished, as it appears to us, by a right understanding on the part of the society of their importance, and a resolution to throw large powers into the hands of a committee of the most able and intelligent of their mem-

bers. These powers should extend to the judicious exclusion of what is unworthy of publication, and the correction or completion, in every possible case, of what is erroneous or defective in description while the objects described are still accessible. That such an authority does not exist at present, we conclude from the note appended by the council to the present part of the reports, declaring that they have proceeded in a great measure upon an opposite principle. And we think that any candid member of the society will readily admit, that the volume already published has probably suffered much from this fastidiousness. It would be invidious to particularize instances; but so long as we find important objects described, as they are in numerous cases, by *mere names*, without any reference to those physical characters of form, colour, size, &c., which alone can convey real information to those who require it; so long as we observe the most careless and flimsy productions occupying the same honourable place in this volume with those that are really the gems of its pages, —we must believe that a careful and judicious superintendence is a desideratum which the society will do well to supply, if it is to perpetuate its own best interests and highest usefulness.

The fact is, that while the power of detailing symptoms and treatment belongs to *many* medical men, the art of pathological description is possessed by very *few*, being both somewhat difficult of acquisition, and very much neglected by the majority of practitioners; and hence it happens, that in very many reports on pathological subjects, the glaring errors and obvious deficiencies are such as to defeat any useful purpose. It should be the care of such a society as this, that none of the important objects submitted for its examination are lost to the public through inadequate description; and it ought, therefore, to be the *duty* of the council to make themselves responsible in some degree for the reports, and by judicious retrenchment, addition, and emendation, to place them in conformity with the specimens presented, and with the demands of the age. In this way the society will become a source of instruction to its own members, as well as of profit to the public; and while it has the power of enrolling in its council, as at present, a large number of names, honoured as those of skilled and tried labourers in the path of scientific inquiry, and in particular of pathological description, we think that no fear need be entertained, that the duties in question would be exercised with delicacy and discrimination.

If the expression of our opinion in this matter, shall have in any degree the effect of inducing the members to confer such an authority, or of strengthening the hands of the council in using the powers they already possess more effectively, our object as critics will be accomplished; and we are satisfied that in saying this, we speak the wishes of many who look to the Pathological Society of London as one of the most promising institutions of the present day.

The section, both in this number and the last, on the malformations of the heart and great vessels, contains a number of most in-

teresting and rare anomalies, and mostly very well described. The greater number bear on the question of *cyanosis*, and they appear to us in most cases to correspond, on the whole, with the views advocated by Dr Mayne of Dublin (see *Retrospect* for June 1848, p. 110); but we have not space at present to enter on an analysis of them. Perhaps the most curious, are a case by Dr Chevers, of congenital obliteration of the pulmonary artery, with nearly complete closure of the *ductus arteriosus;* and another by Dr Peacock, of absence of the same artery, the aorta arising from the right ventricle, and giving off the pulmonary branches through the *ductus arteriosus*.

The most interesting contributions to the pathology of the other organs, are a case of epithelial cancer of the tongue, excised, and apparently cured, by Mr Critchett; a case, by Dr H. M. Hughes, of perforating ulcer of the stomach, communicating by a fistula with a gangrenous cavity in the lung; some curious details by Mr Prescott G. Hewett, tending to show the frequency of ulceration of the duodenum in cases of external burn; and some excellent observations by Dr Handfield Jones on a form of dysenteric ulceration. The same author has a note on the scrofulous deposits found in the ureters, showing that they are probably deposited subjacent to the mucous membrane. Dr Williams presented a kidney from a gouty person who died of phthisis, in which the tubes contained crystals of urate of soda, which was also deposited in the joints. Unfortunately, there is no description of these crystals, or of the case, which would have been very desirable, and could have been easily supplied.

We heartily wish the Pathological Society of London success, and hope that, as that success becomes confirmed, every new volume will show increased care and vigilance on the part of the council to reform all imperfect description, and to encourage well-regulated discussion and generalization.

The Periodoscope; with its Application to Obstetric Calculations and the Periodicities of the Sex. By W. Tyler Smith, M.B., Lond., Obstetric Lecturer in the Hunterian School of Medicine.

We have before us a small book, containing a table or dialplate, designed to assist the practitioner in making obstetric computations, by converting for him, without need of calculation, calendar into lunar months. This is accompanied by remarks upon the different alleged periodicities of the female sex.

The table itself is a neat and ingenious expedient, which may be useful for class-teaching, or in the wards of a maternity charity. It must, however, be always borne in mind, that the great difficulty in prognosticating the duration of pregnancy, and the periods of danger during pregnancy, lies not in the calculation of days, but in the determination, as near as possible, of the period

of conception; and further, that at best the results are only probable.

To one who has not the faculty of easily dealing with numbers by a process of the mind, the periodoscope will be a valuable aid; but to an experienced obstetrician, accustomed to the calculations which the periodoscope supplants, it will not prove of much service. Dr Smith's theoretical remarks upon the periodicities of the sex are well worthy an attentive perusal; but we doubt if they can be deemed of much practical value.

Physiological, Anatomical, and Pathological Researches. By JOHN REID, M.D., Fellow of the Royal College of Physicians of Edinburgh, Chandos Professor of Anatomy and Medicine in the University of St Andrews, &c. &c. Edin. 1848. 8vo, pp. 659.

FEW names stand so high in experimental physiology as that of Dr John Reid. His labours in this department have always been distinguished by a completeness and perseverance which went far to exhaust every branch of the subject, and leave the path of inquiry for a considerable time barren to future investigators. Of this kind were his experiments on the eighth pair of nerves, and on asphyxia; notwithstanding that eight or nine years have passed since their publication, these subjects still remain *his own;* nor have the subsequent investigations of Volkmann and Valentin, or of Mr Erichsen, availed to deprive him of the honour of having thoroughly investigated every branch of these subjects, so as to make his researches the basis for all others. And although, from the period of rapid transition through which the anatomical and pathological sciences have passed of late years, he had not been able to monopolize any similarly exclusive domain in these departments, still his researches on the medulla oblongata, heart, and placenta, and his reports on fever, will long be regarded as standard authorities on these subjects.

The secret of his success, we think, lies chiefly in two circumstances; in that clear and truly practical character of mind, which enables Dr Reid at once to apprehend where investigation is most needed, and what character it should assume; and also in the pains which he invariably takes to form a candid estimate of the labours of his predecessors in the inquiry into any subject. The latter characteristic (which was well exemplified in the admirable history of the subject prefixed to his paper on asphyxia) fitted him admirably for the office of critic; and we may be excused for bearing testimony in this place to the advantages which we have ourselves derived, at different times, from his co-operation with us in this branch of our editorial duties. We have great pleasure in seeing two of these anonymous contributions to medical literature enrolled in the present volume; we allude to the reviews of Wattmann on air in the veins, and Burrows

on the cerebral circulation. In the last of these, a question which had become peculiarly embarrassed by imperfect conceptions on all sides, and positive errors of reasoning on the side of Dr Burrows, was discussed by Dr Reid in so clear and satisfactory a style, and was so plainly shown to require for its solution only an acquaintance with the simplest principles of natural philosophy, that we cannot help wondering how any one could have been deceived, and still more, that the fallacies of Dr Burrows should, in several instances of late, have been reiterated, and the peculiarities of the cranial circulation denied, by writers of reputation and merit, who cannot, we believe, have been acquainted with the complete refutation which these fallacies had received. We hope that all persons in future writing on this subject, will refer to Dr Reid's book, or to the original review in this Journal for August 1846.

In the present volume, all the more important of Dr Reid's contributions to science have been collected; and the author has been at the pains to append to most of them remarks, bringing down not only the results of his own experience, but also that of others, to the present time. In the case of the inquiry on the eighth pair of nerves, this addition extends to thirty-seven pages, and gives a most complete history of all that has been done on the subject. In the appendix to the article on asphyxia, he explains some differences between Mr Erichsen's results and his own, depending on a peculiarity in the form of the hæmadynamometer used by him. He dissents from Mr Erichsen's theory of the pulmonary stasis in asphyxia, and reasserts his own, which is the same as Dr Alison's—that it depends on the cessation of the chemical changes on the blood in the capillaries. We confess that we have never been able to see how the existence of these changes should advance, or their cessation retard, the progress of the blood; we should rather expect a contrary effect. On the other hand, it is most clear and intelligible, that a contraction in some part of the vascular channels would have the effect in question; and although we do not feel called on to assert this as positively the cause of the pulmonary stasis, and still less to adopt Mr Erichsen's view on contraction *of the veins* (which is, we think, pretty well disposed of by Dr Reid), yet we by no means think it proved that such contraction may not exist in the capillary vessels of the lung. It is not enough to say that capillaries have not been *proved* to contract; for as there are several very strong reasons for supposing that this is the case, and no objection worth mentioning has ever been urged against their contractility, we are disposed in the mean time to adopt this view, as the explanation of many phenomena very difficult of solution on any other hypothesis. We must admit, however, that Dr Reid's arguments, in favour of the power of the capillaries to propel the blood independent of contractility, are among the most ingeniously stated that we have seen.

Two new articles are added; the one, on sensational and emotional reflex actions; the other, on the value to be attached to experiments

on the nervous system. Both are sound and good; in the latter, the author defends the validity of such experiments against the indiscriminating aspersions which have been thrown on them, while he clearly shows the sources of fallacy to which they are liable.

The articles in this volume are twenty-seven in number. It is illustrated by plates and wood-cuts; and, from the whole style of its execution, is one of the most elegant, as well as useful volumes for a medical library which has ever emanated from the press of Edinburgh or London.

Clinical Lectures, delivered in the Theatre of Mercer's Hospital, during the Session of 1847-8. By JAMES F. DUNCAN, M.D., T. C. D., Assistant Physician to the Hospital, &c. &c. Dublin, 1848. 8vo, pp. 122.

THE lectures, which are here collected into a volume, were published during the spring and summer of the present year, in the Dublin *Medical Press*. The author appears to enjoy considerable reputation as a clinical teacher, and is also a lecturer on the theory and practice of medicine in Park Street School. As a specimen of the clinical teaching of the present time in Dublin, his work might have been critically compared with the excellent lectures of Dr Graves, which now lie before us in an improved second edition by Dr Neligan, and which may be considered to represent the older and more experienced school of Dublin physicians; but this would scarcely be just to the present author, whose light and occasional sketches of disease in this volume, delivered *viva voce* and scarcely re-touched or elaborated in any material degree, bear the impress of quite a different object and character from the large and almost systematic work of Dr Graves. We shall therefore delay noticing the latter till next month, when we hope to accord to it a space commensurate with its importance.

The lectures of Dr Duncan are written in an easy and flowing style, free from mannerism and obscurity, and in every way well adapted for instruction. To the matter of them we have already been indebted in the pages of our *Retrospect*, having read some of them with interest and approbation in the form in which they were originally published. In others we find many objectionable statements, arising sometimes from too great eagerness in adopting the views of others, and sometimes from errors of observation; to some of these we shall presently allude. Where his data are sound, the author's conclusions are generally just and apposite, and moreover unwarped by that spirit of exclusive nationality which finds its way into so many Dublin productions, covering many excellencies with a garment of outworn prejudice, by which nature and decency are alike outraged.

Lecture first is on diarrhœa, and contains nothing remarkable

excepting the strong recommendation of strychnia as a remedy. He conceives this medicine to be peculiarly, or rather exclusively applicable to the cases in which the bile is deficient in the evacuations; and ascribes its action to a power of restoring the secretions, and particularly the bile, to their natural quantity. He therefore recommends strychnia as useful, precisely in the class of cases in which opium fails.

Lecture second treats of several chest affections—emphysema, bronchitis, mitral and aortic valvular disease. In treating of the diagnosis of aortic disease by the pulse, he directs attention to the effects of posture.

"In the erect position—that in which most persons happen to be placed when the physician pays his visit—it is full, and hard, and strong; in the recumbent, on the contrary, it is weak and compressible: the reason of which I believe to be this—In the erect posture, the blood, being acted on by gravity, is forced down into the principal trunks, especially from the carotids; the vessels hence being tense and rigid, the pulse acquires a fictitious strength, which disappears as soon as the mass is more equably distributed by the patient assuming the horizontal position. Perhaps there may be a second cause operating to produce this result. The ventricle having to propel the blood in the erect posture against the force of gravity, by an instinctive effort contracts with greater vigour than at other times, when it has not this disadvantage to contend with.

Now there is some truth and much error in this statement. The *truth* is, that the pulse in aortic disease is full, bounding, and —more especially—*visible*, in proportion as the wrist is elevated, and the blood gravitates from it towards the body; for the very simple reason, that this peculiar character of the pulse depends on the diminished tension of the arteries, and is increased by every thing which diminishes the fluid pressure within them. The *error* is in assuming that the aortic pulse is ever in any posture *hard;* and as this is asserted repeatedly in the course of the lecture, we presume that Dr Duncan has not distinguished with sufficient accuracy between strength and hardness of pulse; the fact being, that however full and strong, the aortic pulse is *always* very compressible. We need not criticise the explanations given by Dr Duncan; indeed we cannot understand their application.

The *third Lecture* is on chlorotic anasarca, of which it gives a very good account.

Lecture fourth is on spinal arachnitis, an uncommon disease in this country, but which has been observed to occur in an epidemic form both in Ireland and on the Continent. The distinction of the disease from hysteria with spinal irritation—to which it bears considerable resemblance, from the tenderness of the back and abdomen, and the vomiting—is placed by the author in the acute febrile character of the inflammatory disease, and in the urine, which, being of high specific gravity, and often loaded with lithates, is very different from the pale limpid and watery urine of hysteria.

The *fifth Lecture* is on Bright's disease of the kidney, a disease

which the author seems to think has passed from obscurity into light, in consequence of the discovery that fatty granules in the urine constitute its only real sign. We recommend him not to trust to any such test, but to stick to his test-tubes and urinometer, until he has some more consistent and sure microscopic phenomenon. We beg to assure him that he will fail, in a majority of instances, to find oil granules present at all; and further, that if he finds them present, they are not to be trusted as unequivocal signs of the disease in question. The treatment is a great source of congratulation to Dr Duncan. Since the true nature of Bright's disease has been discovered to be fatty degeneration of the kidney, it has become "easy, intelligible, and simple; and, though we may not anticipate success in every instance (!), we can clearly perceive that there is nothing in the nature of the affection to render it necessarily incurable or fatal." Is it possible that Dr Duncan can think such a violent change of our views, and such sweeping assertions, practically warrantable in the present state of the pathology of this disease, without careful and repeated trial? To our mind, this whole lecture is most crude and unsatisfactory, and not the less so that we do not believe the data on which it rests to be well founded.

The next lecture—the *sixth*—is by many degrees the best in the volume. It relates to a case of pneumonia affecting the upper lobe of the right lung, with emaciation, and some of the general characters of phthisis. The diagnosis is very well and clearly stated. As we have been obliged to condemn the preceding lecture, we have much pleasure in quoting the following excellent and compact description of acute phthisis:—

"In its general aspect, acute phthisis resembles an attack of typhus fever; there is great disturbance of the system at large, and well-marked pyrexia. Often the attack is ushered in with a distinct and severe rigor; often it succeeds to the ordinary causes of common fever, such as exposure to cold, a heavy wetting, or sitting in damp clothes. In all cases the skin is hot, the pulse quick, the appetite fails, thirst becomes developed, and there is great prostration of strength. Still a careful observer would detect at once that it differs from real fever, in the exemption of the nervous system from participation in the general disturbance; the intellect is clear, the eye bright, there is no headache nor stupor, and the patient's repose is comparatively undisturbed. His countenance, too, fails to indicate the oppression that is constantly exhibited in fever, and which a practised eye can so readily detect."

The exemption of the nervous system, however, is not always such as is here described; the tubercle not unfrequently extends to the membranes of the brain; and, whether this be the case or not, that organ is apt to be oppressed and sluggish, as a consequence of the dyspnœa. The following passage on the characters of cough is also sufficiently good and characteristic:—

"It is not perhaps easy to describe these different varieties of cough in so many words; the only true method of becoming familiar with them, is to use your own ears on every occasion, and not to be satisfied with the simple statement, that the patient's cough is troublesome, or the reverse. In pleuritis the cough is short, impeded, and suppressed; in bronchitis, full and open; in hoop-

ing cough, it occurs in paroxysms threatening instant suffocation; in consumption, it varies with the stage; in the early period, before softening has taken place, it is short and dry; in the advanced period it is hollow and moist; in pneumonia, on the other hand, it is mixed; the hardness and restriction which arise from pulmonary consolidation being modified by the relief that follows the effort of expectoration. In the case of acute phthisis, the cough has all the hardness, frequency, and absence of expectoration that characterise the early period of tubercular irritation, while the minuteness of the deposit, as contrasted with its ordinary development, causes it still to present some resemblance to the cough of bronchitis."

The remaining lectures, four in number, are upon different chest affections; and although characterized by all the author's good qualities, and some of his faults, contain nothing on which we are desirous of remarking, excepting the assertion in page 70, that pneumonia may be distinguished from tubercle by the enlargement which takes place in the lung. We thought that Laennec had long ago demonstrated that no such enlargement takes place; and in the case here related, we have no doubt that it was owing to the pleurisy.

The reader will not fail to have observed that the present volume is scarcely a safe one to put into the hands of a young student, although, with a little time, discrimination, and weighing of particular expressions, it might readily be purged of its errors. We hope that Dr Duncan's next production will demand a more unqualified approval.

Surgical Anatomy. By JOSEPH MACLISE, Surgeon. Fasciculus I. London: 1848. Folio.

THIS simple and short title denotes the beginning of a very extensive and important work on regional anatomy by Mr Maclise, who is well known to the profession through his beautiful illustrations of Mr Quain's work on the arteries, and through his own works on comparative anatomy. Mr Maclise is indeed a very thorough anatomist, and has studied the subject in the most scientific and enlarged spirit, in relation both to the theoretical and practical departments of science; and the present work, as a system of relative anatomy, is not merely intended for the surgeon, but as an illustration of whatever, either in surgery or medicine, depends upon an intimate knowledge of the relations of the different parts of the human frame. In a singularly nervous preface, he insists upon the importance and beauty of anatomy as a science, when cultivated in its fullest and widest acceptation, and seems to chafe at the very idea of the narrow limits of mere human anatomy which the present work imposed upon him, and at the anatomical terms of the middle ages, which he characterizes as a "barbarous jargon of words, barren of all truthful signification."

"If there may be any novelty now-a-days possible to be recognized upon the

out-trodden track of relative anatomy, it can only be in truthful and well-planned illustration. Under this view alone may the anatomist plead an excuse for reiterating a theme which the beautiful works of Cowper, Haller, Hunter, Scarpa, Soemmering, and others, have dealt out so respectably. Except the human anatomist turns now to what he terms the practical ends of his study, and marshals his little knowledge to bear upon those ends, one may proclaim anthropotomy to have worn itself out. Dissection can do no more, except to repeat Cruveilhier."

With all his scientific fervour, however, Mr Maclise seems fully to appreciate the practical nature of his subject; and his love of the new spirit has not induced him to sacrifice the old terms, which, we suspect, are in fact as well adapted to our wants as any thing that could be substituted for them.

The plates in the present fasciculus are four in number; one being devoted to the thorax, and three to the neck and its surgical regions. They are all extremely well executed; and Plates II. and III., in particular, are in many respects among the best planned dissections of the neck that we have seen. The descriptions fully correspond in clearness and usefulness with the plates; only we have at times to complain of certain eccentricities and imaginative peculiarities of style, which Mr Maclise ought not to throw away upon a work of *mere human* anatomy. By a little care, the succeeding parts may be easily kept free of every thing of this kind. We shall look for the results of the *perfervidum ingenium* of the author rather in well-planned and delineated dissections, than in such traits of manner; and we shall expect to observe in the succeeding numbers many excellent new views of hackneyed subjects. Of this Mr Maclise has given fair promise.

One most important merit of the work is its cheapness; the four large coloured plates of this fasciculus, with the descriptive letter-press, only costing five shillings. The paper and execution are beautiful, and the work is in every way creditable to the enterprise of Mr Churchill, the publisher.

A Hand-Book of Physiology. By WILLIAM SENHOUSE KIRKES, M.D., assisted by JAMES PAGET, Lecturer on General Anatomy and Physiology at St Bartholomew's Hospital. London, 1848. 12mo, pp. 705.

THIS work was originally planned as a hand-book, on the basis of Müller's Physiology; and was intended to render the facts and views in that work more available for the purposes of students, by throwing them more into a form adapted to the portion of time usually devoted to physiology in an academical course. In proceeding, however, the authors found it necessary to adopt a bolder principle, giving the work a form and arrangement of its own, and drawing the materials from the original sources of supply, in the departments

which chiefly require it. In this they have done well. The form of Müller's Physiology has been always felt to be complex and ill arranged, especially for beginners; although, as a repository of physiological information, it is decidedly the best yet published.

From an examination of several of the articles in this volume, we are enabled to say, that the authors have succeeded in producing a work well adapted for students. The copious references made, both to elementary works, and to original papers, are most valuable; more especially as this species of research is of great importance to students, and is apt to be neglected by the authors of manuals. The volume is also well illustrated, many of the wood-cuts being those of Müller's Physiology.

Surgical Experience of Chloroform. By JAMES MILLER, F.R.S.E., Professor of Surgery in the University of Edinburgh. 8vo, pp. 60. 1848.

MR MILLER'S pamphlet has a more than ordinary claim on the attention of the profession, from the circumstance that he was the first to test the value of the new anæsthetic in operative surgery. It was evident to all who witnessed the operation in question, that a new era in the history of anæsthesia had begun, and, before many days had elapsed, chloroform was in general use over Europe and America.

Mr Miller has had occasion to employ chloroform extensively, both in public and private practice, and he has embodied the result of his experience in this pamphlet, which contains by far the best account yet published of its surgical applications. The first nine sections are devoted to details in reference to the purity of the chloroform, its mode of administration, &c. The remainder of the pamphlet is subdivided into two parts; the first of which treats of the advantages derived from anæsthesia in the performance of operations, and the second enumerates those affections, in which, though unconnected with operations, important benefits are obtained from the use of chloroform. The reader will derive much useful information from the perusal of the entire pamphlet; but we recommend especially to his notice the sections on the use of anæsthesia in dislocations, in the examination of injuries, in the treatment of irritable stricture, in the operation of sounding, and in stone of the bladder complicated with diseased kidney, under which heads Mr Miller's observations are of a highly practical and excellent character.

We had contemplated a more lengthened notice of this valuable essay, and had marked several passages for insertion; but the subject of chloroform has already occupied a large space in our pages.

Part Third.

MEDICAL NEWS.

THE CHOLERA.

THE epidemic which we announced in our last month's publication as having invaded this country, has continued to rage with great virulence in Edinburgh, where the number of cases now amounts to between seven and eight hundred (including Leith), of which nearly a half have already proved fatal, about 230 remaining under treatment. A severe epidemic broke out at Loanhead (a village six miles to the south of Edinburgh), and many cases have also occurred at Lasswade, Gilmerton, Portobello, Cockpen, and other villages of the neighbourhood. Several cases have been reported in Glasgow, mostly in very crowded and miserable localities: and the disease is also reported to exist in Dumfries as well as in Falkirk, a town whose sanitary condition is far from satisfactory. In England the number of cases has not approached that of the northern division of the kingdom; and, although in London the number has been considerable, yet, relatively to the population, it is very small compared with Edinburgh. On the week ending October 4th, 65 deaths were recorded in the Registrar-General's returns from cholera in London; on November 11th, 62; on November 18th, 54. The epidemic appears therefore, on the whole, to be decreasing in violence in London. Every where the disease appears to have been almost confined to the most wretched and miserable of the population; a few trivial exceptions only to this rule having occurred in London and Edinburgh. The mortality has been every where nearly the same,—viz., about two-thirds of the recorded cases which have been brought to a termination. It is probable however, that no trustworthy returns of the whole cases in any place have been yet published.

On the continent it has declined in a great measure at St Petersburg, Berlin, and Hamburg, where, however, cases still occur. On the other hand, Rotterdam has been attacked with great severity since the 1st of October, and Dantzic, from a similar date, has suffered under a still more severe visitation. It is stated that in the small town of Gartz, in the district of Stettin, there has been the extraordinary mortality of 102 persons out of a population of 700.

A considerable number of cases of a choleroid disease prevailed in the beginning of the month in Dunkirk; and a few have occurred in the neighbouring villages, and in Calais. M. Magendie was sent by the Academy of Sciences to investigate this alleged ingress of the disease into France; and reported the cases not to be true epidemic cholera. If we may trust the report of a discussion in the *Société de Médecine Pratique*, several cases having the strongest resemblance to the disease appear to have occurred in Paris; but there seems to be, on the part of the French physicians, a strong disposition to doubt or deny its presence.

The General Board of Health has displayed considerable activity, but not all in the right direction. The truth is, that it has been hard beset. Composed, as it is in great part, of non-medical members, and forced to act in an emergency, the single medical head connected with it, has not been able to save it from being thrown upon the suggestions and recommendations of busybodies; the effect of which was the framing of a document full of inconsistency, and founded on most imperfect knowledge of the real views of the profession. The College of Physicians of Edinburgh was the first to disclaim the views of the Board of Health, as we noticed in our last Number; and this was followed by a most wise and temperate document on the part of the London College of Physicians, recommending hospitals and houses of refuge, and showing the absurdity of the attempt to dictate particular dietetic systems to persons who may be very dependent on many of the forbidden articles. Nearly the whole medical press of the country having likewise joined in tolerable unanimity on this subject, the General Board has been induced to remodel its in-

structions; and we have now before us a set of additional directions for England, and another for Scotland, in which particular orders are given for the construction of cholera hospitals and houses of refuge, and for the employment, where necessary, of additional medical assistance.

We are happy to find that all the medical authorities have agreed to consider the question of contagion an open one, and have concurred expressing sentiments of a moderate character on this subject, closely corresponding with those which we endeavoured to impress upon our readers in the last Number of the *Journal*. We hope that in all hospitals and public institutions, facts relating to the propagation of cholera will be scrupulously and carefully recorded. Reports should be drawn up of the *whole* of the nurses and other persons exposed to contagion in such institutions; with the view of ascertaining the proportion in which they are affected with the disease. We believe that a large body of returns of this kind would be of more essential service than any other species of evidence on the subject of contagion.

A committee of the Edinburgh College of Physicians and Surgeons has been appointed, to procure evidence in relation to the present epidemic of cholera in Edinburgh and Scotland. Circulars have been issued to all the medical practitioners in Edinburgh, and the adjoining villages which have been invaded with cholera, enclosing blank schedules, to be filled up with information in a short form, in regard to any cases which may occur. We take this opportunity of impressing upon medical men the importance of attending to this point, as the labours of the committee can only be rendered available for scientific purposes, by the co-operation of a large number of persons; and the trouble imposed on them by filling up the schedules, is, from the simplicity with which these are drawn up, extremely trifling.

The most remarkable circumstance as to the treatment of cholera, arising out of the present epidemic, is the employment of the inhalation of chloroform in ten cases successively in Peckham Asylum, without a single fatal result. This favourable experience has not been borne out in Edinburgh, where this remedy has been very assiduously tried in the Cholera Hospital without the least apparent good effect. The weekly and daily journals are full of remedies, but few of these rest on any large experience.

We have received a number of publications on the treatment of cholera, some of which we mean to notice next month. In the mean time, we shall not add one word to the remarks of Dr Robertson, in a paper which will be found among our original communications, and which is the result of by far the largest experience yet furnished by the present epidemic. We have great pleasure in bearing witness, from personal observation, to the care and judgment with which Dr Robertson's experimental trials have been conducted.

We have given, in the Practice of Physic section of the *Retrospect*, the results of practice in India for some years past in cholera—as collected from a large series of medical reports by Mr Samuel Rogers. We propose to notice this important work more fully in next Number.

GUANO A "DRUG" IN THE MARKET.

Dr Vincenzo Gonzales, writing to the Academy of Bologna from New Grenada, states that guano, either in the form of baths, frictions, or decoction, is in that country successfully used against lepra, (probably elephantiasis arabicum.) The patient is first given an ounce of guano in a quart of barley-water, taking care to mix well. The next day he has a bath at the ordinary temperature, to which are added ten ounces of guano in fine powder. The patient then is put into a warm bed, and an hour afterwards frictions with guano ointment are made. On the day when the bath and the frictions are used, no guano is taken internally, and *vice versâ*. The patients sleep on mattresses stuffed with guano, The government of New Grenada has sent a circular all through the country, to recommend guano against lepra. *Is this a Transatlantic hoax?*

MEDICAL JOURNALISM IN ITALY.

Our readers will be surprised at the following numerous list of medical journals and transactions of medical societies published in Italy. Three appear in the Papal states, *Il Bolletino Medico of Bologna, Gli Annali Medico-Chirurgici of Rome, Il Raccoglitore Medico of Fano;* three in the Neapolitan states, *L'Osservatore Medico, Il Filiatre Sebezio,* and *Il Sarcone;* two in Lombardy, *Gli Annali Universali di Medicina,* and *Gazetta Medica* of Milan; three in Tuscany; one in Piedmont, *Journal de la Société Medico-Chirurgicale de Turin;* lastly, two in the Venetian states, *Il Giornale per Servire ai Progressi,* and the *Memoriale della Medicina Contemporanea*; in all fourteen, and we believe the names of two or three are omitted. This formidable *journalistik* is one among other injurious effects of the subdivision of Italy into so many independent principalities, each of which must have its medical school and journal. The consequence of this ruinous competition is a great dearth of good original matter, and, generally speaking, a low standard of excellence in the majority of the publications above named, which consist chiefly of exracts from foreign, and more especially the English journals. In the pages of two or three, however, which form honourable exceptions to this remark, may be found valuable contributions to medical science. For the information of the reader, we may state, that probably the best, as it is the oldest, Italian medical journal, is the *Annali Universali di Medicina,* published at Milan. Few of the others find their way to England; but excellent analyses of their more important papers are furnished by the *Gazette Médicale* and *L'Union Médicale* of Paris, from which they are again occasionally transplanted, for the benefit of our readers, into the "Monthly Retrospect."

BOOKS RECEIVED.

1. Graves' Practice of Medicine. Edited by J. Moore Neligan, M.D. Dublin. 8vo. 1848.
2. Hassall's Microscopic Anatomy. Part XIV. 8vo. London. 1848.
3. West on the Diseases of Infancy and Childhood. 8vo. London. 1848.
4. Neligan on Diseases of the Scalp. 12mo. Dublin. 1848.
5. Handbuch der Gewebelehre. By Dr Jos. Gerlach. 8vo. Mayence. 1848.
6. School Chemistry. By Dr Thomson. 12mo. London. 1848.
7. Guy's Hospital Reports, Oct. 1848. 8vo.
8. Peacock on the Influenza of 1847-48. 8vo. London. 1848.
9. Lee's Clinical Midwifery. 12mo. Lond. 1848.
10. Chapman on Ulcers of the Leg. 12mo. London. 1848.
11. Taylor's Medical Jurisprudence. Third edition. 12mo. London. 1848.
12. Bowman's Introduction to Practical Chemistry. 12mo. London. 1848.
13. Miller on Surgical Experience of Chloroform. 8vo. Edin. 1848.
14. Bainbrigge on Chloroform. 8vo.
15. Pharmacopeia ad usum Nosocomii Phthisicorum et Pectoris Morbis Aegrotantium accommodata. 12mo. London. 1848.
16. Hygiea, Medicinsk och Pharmaceutisk Mänads-Skrift, Tionde Bandet. 8vo. Stockholm. 1848.
17. Tylee, Practical Observations on Galvanism, &c. 12mo. Bath. 1848.
18. Bartlett, Inquiry into the degree of certainty in Medicine. 8vo. Philadelphia. 1848.
19. Green—Observations on the Pathology of Croup. 12mo. New York. 1848.
20. Medical Report of the Lancaster Lunatic Asylum. 8vo. Lancaster. 1848.
21. Peacock on Influenza. 1848.
22. Pathological Society's Reports, 1847-8. 8vo. London.
23. Ethnological Journal. No. 6.
24. Kirkes' Handbook of Physiology. 12mo. London. 1848.
26. Carpenter on Medical Study. 8vo. London. 1848.
27. Keir, Leared, Billing, Paterson, Charles Bell, and Grieve, on Cholera.
28. Journal of Public Health. 1847-8.
29. Transactions of the American Medical Association. 8vo. Philadelphia. 1848.
30. Maclise—Surgical Anatomy. Folio. London. 1848.
31. Tracts for the Improvement of our Popular Literature. No. 3.
32. The Idea of Life. By S. T. Coleridge. 12mo. Lond. 1848.
33. Gay on Femoral Rupture. 4to. Lond. 1848.

Report of Edinburgh Medico-Chirurgical Society's meeting next month.

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*The Attitude and Positions, Natural and Preternatural, of the Fœtus in Utero, acts of the Reflex or Excito-Motory System.*

By J. Y. Simpson, M.D., F.R.S.E, Professor of Midwifery in the University of Edinburgh, and Physician-Accoucheur to the Queen in Scotland, &c.

In numerous important points, the physiology of the human fœtus varies from the physiology of the human adult. The mechanism of some of the highest functions in the economy is different in the one from what it is in the other. The mode, for instance, in which nutrition is effected in the infant before it leaves the uterus, differs greatly from the mode in which nutrition is conducted in the individual after birth. The means by which respiration is accomplished during intra-uterine life, are very different from the means by which it is accomplished during extra-uterine life. The mechanism and course of the circulation is not the same in these two states of existence. After birth the circulation is double (systemic and pulmonic); before birth it is carried on as a single circulation, in an apparatus intended afterwards, and ultimately, to serve for a double circulation. The common attitude and position of the fœtus and adult are equally different. The adult in his waking state, and whether walking or standing, carries the body vertically, with the head uppermost. The fœtus in utero, for some time before birth, usually also carries the body vertically, but with the head placed undermost. The position of the head in relation to the body differs in the two states of being—in the adult it is placed higher than the body, in the fœtus it is placed lower than the body.

At, and for some time before the full period of utero-gestation, the mode in which the child is situated in utero is generally as fol-

lows:[1]—The trunk and spine are usually slightly curved, and the head is bent forwards, with the chin approaching toward the sternum. The thighs are drawn upwards and flexed upon the abdomen; and the legs are flexed and laid along the posterior surfaces of the thighs. The feet, which are sometimes crossed, are thus placed near the nates, and in front of them. In the interspace between the knees and face the upper extremities are situated, the arms being laid across the sides of the thorax, and the forearms flexed and crossed in front of the chest.[2] The whole fœtus is thus flexed and rolled up into an *ovoid mass*, of such a form as to occupy the least possible space. One end of the ovoid is formed by the head, the other by the breech of the child.[3]

This ovoid is, in obstetric practice, found to be placed over, or to present at, the os uteri at the time of labour, in three different modes. 1. In a large majority of cases, the head or cephalic end of this ovoid mass is placed lowest in the uterus, and consequently presents at the time of birth. 2. Sometimes, however, the nates or pelvic extremity of the fœtal ovoid is the lowest and presenting part. 3. Still more rarely the fœtus is placed with its long axis lying transversely to the long axis of the uterus, and the presenting part is the side of the ovoid mass of the fœtus; or, more strictly speaking, one of the shoulders or arms.

The following table shows the proportion in which these three leading genera of positions or presentations of the fœtus at the full time of pregnancy, occurred in four large reports of cases furnished by Lachapelle, Boivin, Clarke, and Collins.

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[1] Through the kindness of Professor Goodsir, I am enabled to give, in Plate I., a sketch of the attitude of the fœtus in a subject brought into the dissecting rooms during the present winter. She had died of cholera, near the full time of utero-gestation. The placenta is seen situated on the right side of the uterus, opposite the right foot of the infant. In injecting the vessels, some water escaped in the interspace between the two feet, and probably slightly altered their position.

[2] "The most common situation of the extremities is not to be determined, as they are found to be a little different in different dissections; and in the living body they vary almost every moment: thence the hands are seen indiscriminately on the head or face, or across one another, or around the knees or legs, or the legs are sometimes extended, and the feet are placed by the face, or one in that position, and the other contracted and the foot downwards."—*Dr William Hunter's Anatomical Description of the Gravid Uterus.* P. 62.

[3] "The fœtus in utero," says Hunter, "is naturally contracted into an oval form, adapted to the figure and circumstances of its habitation. The vertex of the head makes one end of the oval, and the nates the other. One side or edge of the oval is formed by the occiput, the back part of the neck, and the incurvated trunk; the other is made by the forehead, and the mass of contracted and conglomerated limbs. The chin is close to the breast, the trunk is bent forwards, the knees are close to the fore parts of the hypochondria, the legs drawn to the back parts of the thighs; the feet, or lower parts of the legs, decussating each other; and the upper extremities contracted into the vacant space betwixt the forehead and knees."—*An Anatomical Description of the Human Gravid Uterus.* 1794. P. 62.

Table of the Relative Number of Cases in which the Presentation of the Child was found Cephalic, Pelvic, and Transverse.

Reporter.	Total No. of Cases.	No. of Head Presentations.	No. of Pelvic Presentations.	No. of Transverse Presentations.
Lachapelle,	37,126	35,550	1390	186
Boivin,	20,517	19,810	611	96
Clarke,	10,387	10,094	245	48
Collins,	16,654	16,102	504	48
Total,	84,684	81,556	2750	378
Proportions,		96 in 100	1 in 31	1 in 224

The preceding table shows the immense proportion of cases in which the head of the child is placed over the os uteri, and presents at the full term of pregnancy. In the above table, head presentations are in frequency, in proportion to all other presentations, as 26 to 1. The presentation of the pelvic extremity of the ovoid of the fœtus, is to that of the cephalic extremity as 1 to 32. The presentation of the pelvic extremity, and its frequency in comparison to that of the head, is as 1 to 32 according to Desormeaux; as 1 to 33 according to Osiander; as 1 to 34 according to Carus; as 1 to 35 according to Meckel.[1] The transverse position of the fœtus, with its long axis across, or at right angles to the long axis of the uterus, is by far the rarest of the three forms of presentation. In the preceding table of cases, it occurred only once in every 224 labours. Rigby[2] estimates it as occurring in about 1 in every 230 cases; Churchill[3] estimates it as occurring in 1 out of every 261 labours.

Various opinions, and modifications of opinion, have been suggested in order to explain why the head of the infant is thus, before birth, usually placed lowest and over the os uteri, and why some special circumstances should occasionally change this law, and produce malpositions. None of the doctrines hitherto proposed seem to have at all proved satisfactory. Indeed, so much so is this the case, that latterly some authors have looked upon any successful inquiry into the causation of the attitude of the fœtus in utero, as a matter beyond the legitimate limits of physiological investigation. Thus, M. Virey[4] has strongly argued that we ought to content ourselves with considering the fact of the position of the head of the fœtus opposite the os uteri, simply as an ultimate law in animal physiology, and a law quite general to the whole animal kingdom.

[1] See Adelon's Physiologie, tom. iv. p. 160, and Burdach's Physiologie, tom, iv. p. 224.

[2] System of Midwifery, p. 167.

[3] Theory and Practice of Midwifery p. 356.

[4] Mémoire sur une loi de l'economie animale relative à la position des embryons et des fœtus dans l'uterus.—*Révue Medicale*, July 1833.

The law is perhaps not so general as M. Virey[1] supposes. But granting that it were even universal, still this admission could form no kind of logical reason against the propriety of investigating how in man, or in any other species, the effect in question is accomplished, and what mechanism is employed by nature for its production. At all events, if such an investigation be improper, then almost all our other physiological investigations are equally so. Every animal, from the lowest zoophyte up to man, respires, is nourished, and is propagated by generation. In other words, respiration, nutrition, and reproduction, are all of them ultimate and general laws in the animal kingdom; but their being so surely furnishes no reason for discarding all physiological inquiry regarding them, and would not entitle us to argue, as M. Virey does, that it is hence hopeless and improper to attempt to trace, for example, regarding reproduction or respiration, the variety of means and mechanism by which these ultimate laws are brought about and accomplished in man, or in the different series and classes of the animal kingdom.

At the present time, two opinions are principally or alone held by physiologists and obstetricians with regard to the mode of causation of the attitude of the fœtus in utero with the head lowest. It is regarded by some as a result of the mere physical gravitation of the head. Others look upon it as a result of the action of vital or mental influences of an instinctive and voluntary character. In the sequel I shall first speak of these two opinions, and of the objections to which they are open. Afterwards, I shall endeavour to show that the natural attitude of the human fœtus before birth, is the result of excito-motory movements on its part. I shall then consider the circumstances which lead to preternatural positions of the fœtus, with a view of demonstrating that they are not only explicable by, but at the same time illustrative of, this new doctrine of the causation of the attitude of the fœtus. And lastly, I may take an opportunity of offering some brief practical inferences and remarks.

Section I.—Gravitation of the Head as the cause of the Attitude of the Fœtus.

The head is, proportionably to the body, far larger and heavier in the fœtus than in the adult.[2] Most authorities have supposed that the position of the fœtus in utero, with the head undermost, was

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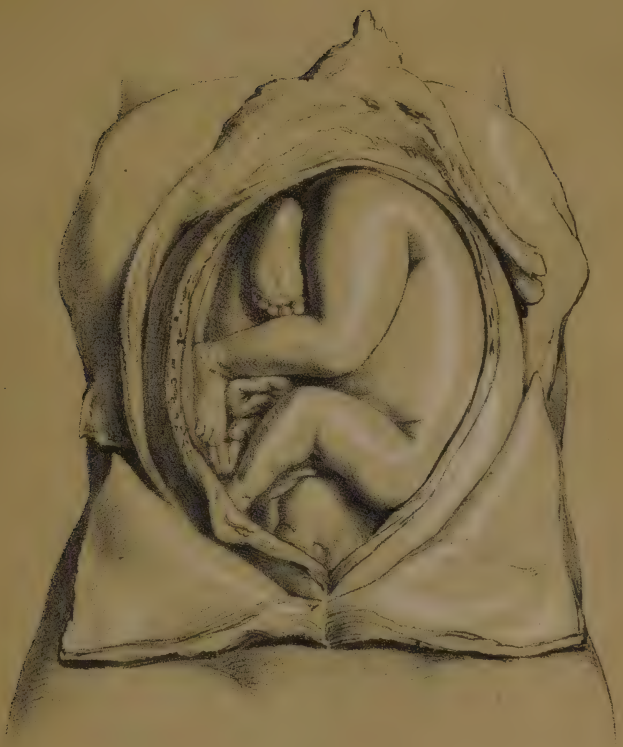
[1] I have more than once had an opportunity of dissecting the female of the common blue shark, (Squalus cæruleus), sometimes caught by the fishermen on our coasts here. The animal is viviparous; and in the distended uterus or oviduct, I found the enclosed fœtus placed in nearly equal frequency with the head and tail directed towards the uterine opening.

[2] The encephalon of a new born child, according to Tiedemann, is so large as to be, relatively to the size of the body, as 1 to 6; in a full grown man it is as about 1 to 40.—*London Philosophical Transactions*, vol. xxvi., p. 503.

Plate I.



Attitude of the Fœtus
near the full term of utero-gestation.

Fr. Schenck Lithog.r Edin.r

owing to the greater specific gravity of the head determining this part, in the erect position of the mother's body, to fall to the lowest part of the uterine cavity, or toward the os uteri.

The *period* of utero-gestation at which the preponderating gravity of the head is supposed to make it the lowest part of the fœtus, has been differently stated by different authors. Some, as Baudelocque,[1] Termanini,[2] Capuron,[3] Meigs,[4] and others,[5] maintain that, from the earliest periods of pregnancy, this is the common and natural attitude of the fœtus. Others uphold, as Mauriceau,[6] Rœderer,[7]

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1 System of Midwifery—Heath's Translation, vol. i. p. 260.

2 "During the first six months of pregnancy," says Termanini, "it is certain that the fœtus enjoys so much mobility, that its position cannot but be determined by the respective weights of its constituent parts. The head of the fœtus is, in fact, the heaviest part relatively to all the others after the second month, and it results from this, that it tends constantly to direct itself inferiorly towards the orifice of the uterus."—*Archives Géneralés de Médecine*, vol. vi. p. 288.

3 Journal Universel et Hébdomadaire, tom. x. p. 437.

4 "The natural presentation is that of the head, which is turned towards the os uteri from the earliest period of pregnancy. The navel-string is nearer to the pelvis than to the head of the child, the head therefore hangs downwards; but when the cord, by the growth of the ovum, has become of a very considerable length, the child ceases to be dependent from it, for the cord is not unfrequently from twenty to thirty inches long.—*Meigs, Philadelphia Practice of Midwifery*, p. 229.

5 See Onymos, Dissertatio de Naturalia Fœtus in Utero Materno situ; in Schlegel's Sylloge ad Artem Obstetricam, vol. i. p. 525; Gehler, De Situ Fœtus in Utero; in *Ibid.* p. 537.

"It is not," says Burdach, "as has been hitherto believed, towards the last period alone of intra-uterine life, and by a sort of culbute, but from the second month of this same life that the human embryo brings its head towards the inferior part of the uterus, or near its orifice. This phenomenon is the mechanical result of the erect position of the mother, since the umbilical cord is inserted at the inferior extremity of the trunk, and the upper half of the body, being the heavier, finds itself hanging."—*Traité de Physiologie*, vol. iv. p. 5.

"All the observations," says Hunter, "that I have been able to make in dissections, and in the practice of midwifery, would persuade me that the child's head is naturally downwards, through all the later months of utero-gestation; and that neither reason nor instinct teaches it, at any particular time, any trick of a tumbler or rope-dancer."—*Dr William Hunter's Anatomical Description of the Human Gravid Uterus*, p. 64.

6 "The spine of the child's back is placed towards the mother's; the head uppermost, and the feet downwards. It keeps usually this posture till the seventh or eighth month, at which, the head being grown very big, is carried downwards by its weight towards the inward orifice of the womb, tumbling, as it were, over its head, so that then the feet are uppermost, and the face towards the mother's great gut."—*Mauriceau, Maladies des Femmes Grosses, Chamberlen's Translation*, p. 147.

7 "I conclude," says Rœderer, "1. That the head of the embryo, which is extremely slender, and supported upon the chest, occupies the top of the uterus after conception; that it inclines towards the abdomen; and that the face is turned towards this part. 2. That it remains in this situation when some obstacle prevents it from descending; for example, a too small quantity of liquor amnii. 3. That it descends gradually according as it becomes heavier. 4. That

Levret,[1] Lamotte,[2] and others,[3] that the position with the head downward, and directed to the os uteri, is only taken towards the latter months; and that it is assumed either gradually, or at last by a sudden movement and toppling on the part of the fœtus (the movement of *culbute* of the older authors). Sir Fielding Ould [4] and Burton [5] supposed that this downward movement of the fœtal head did not occur till the commencement of labour.

Whatever explanation may be given of the position of the infant in utero, one fact has been sufficiently established in regard to it by modern investigations; namely, that the position with the head lowest is not assumed till the latter months of utero-gestation. Before the beginning of the sixth or seventh month, the ovoid mass of the fœtus lies in utero, in nearly an equal number of cases, with its cephalic and its pelvic extremities lowest, or over the os uteri; and in a considerable proportion of instances it is still placed across or transversely, so that the trunk is opposite the os uteri. Hence in abortions, presentations of the feet or pelvis are nearly as frequent as presentations of the head; and presentations of the arm

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at last, having become very large, it is carried downwards by its own proper weight. 5. Several causes may, however, prevent its descending, or derange its direction. I place in this class defect of the liquor amnii, obliquity of the uterus, tumours in the uterus, premature efforts, and external violence."—*L'Art des Accouchemens*, p. 41.

[1] "The fœtus, after the fourth month of pregnancy, has the head above, the breech below, the belly in front; but when *it approaches* the period of its birth, it is the back which is in front, the head below, and the breech above. This observation proves that the infant, at the later periods of pregnancy, brings down its head to where its breech was before, and that it does so by bending forwards, whether gradually or all at once, and this is what the ancients have called the *culbute*."—*Levret, L'Art des Accouchemens*, p. 74.

[2] "All authors," says Lamotte, "agree that the child in the uterus has its back towards that of the mother, the heels backwards, the hands upon the knees, with the head resting on them *till the seventh month*; that at this time the head, growing heavier by the increase of its bulk, draws the body down, making it topple, and that then the head is below and the feet above, the face towards the mother's back; as to any thing else, remaining in the same posture it was in before, which is the situation in which it remains to the ninth month, and in which it comes into the world."—*Lamotte, Treatise of Midwifery, Translation*

[3] See Heister's Compendium Medicinæ, p. 111; Manningham's Compendium Artis Obstetricariæ, p. 10; Haller's Elementa Physiologiæ, vol. viii. p. 412.

[4] Treatise of Midwifery, p. 25; and Preface, p. xiv.

[5] "We must therefore," says Burton, "assign some other cause, which according to Ould seems to be as follows, viz., that the whole spine is curved, and its head looks down, so that the fontanelle is just opposite the fore part of the mother's belly; and therefore as the first and greatest efforts for the expulsion of the child are in the bottom of the womb, which presses directly on the back of the head, as is evident from the posture of the fœtus in utero, and from the foregoing description of the womb, they must of course immediately turn the head downwards towards the vagina, and its face to the mother's back, especially as it is at this time floating in the waters. Hence it happens, that the change of the posture of the fœtus does not happen in a natural way till the first labour pains begin."—*Burton's Essay towards a New System of Midwifery*, p. 100.

are by no means uncommon. In the Maternity Hospital of Paris, Professor Paul Dubois found, that out of 121 fœtuses, born alive or dead before the end of the sixth month, 65 presented by the head; 51 by the pelvis; and 5 were cross-births, or presented by the arm or shoulder. Further, the position of the head towards the os uteri, appears to be taken up more and more frequently and certainly from the end of the sixth month onwards. Out of 73 instances in which the child was born prematurely but alive, during the currency of the seventh month, Dubois found it presenting by the head in 61 cases, by the pelvis in 10 cases, and by the shoulder in 2 cases. Hence, while before the end of the sixth month the proportion of head presentations amounted to 55 in the 100, during the course of the next month, they were found already amounting to 82 in the 100; at the full time they amount, as we have previously stated, to 96 or 97 in the 100.

But at whatever period of the pregnancy the attitude of the fœtus is supposed to be taken, the whole idea of its head being placed lowest and opposite the os uteri, by physical gravitation merely, is a doctrine to the adoption of which there are the following insurmountable objections.

1. The doctrine presupposes that the mother's body is in the vertical or upright position, in order that the gravitation of the fœtal head may have the effect attributed to it. But during her hours of sleep and rest her body is placed horizontally and not vertically, and ought to afford many more chances than the statistical results show, of the head falling by mere gravitation into other positions and localities, than its usual and normal locality in the cavity of the cervix uteri. Besides, most practitioners have repeatedly seen patients restrained to the horizontal position for months before labour came on, without this position of the mother producing any deviation from the common position of the fœtus.

2. The doctrine further presupposes that the child is suspended in the uterine cavity by the umbilical cord.[1] But this is not in ac-

[1] "The fœtus," says Capuron, "is generally suspended from the uterus by the umbilical cord, and represents a kind of lever or hydrostatic balance with two arms immersed in the liquor amnii. These two arms commence at the umbilicus, and terminate the one at the apex of the head, the other at the heels or feet of the fœtus. * * * The head, which is placed at the extremity of the super-umbilical arm of the balance, must obey the gravitation which forces it down to the neck of the uterus."—*Mémoire sur la Situation du Fœtus pendant la Grossesse;—Journal Universel et Hébdomadaire, &c.*, tom. x. p. 437. "The reason," says Hunter, "why the child's head is commonly downwards may be supposed to be this: the child is specifically heavier than the liquor amnii, and therefore, in the various attitudes of the mother, is always in contact with and supported upon the depending part of the uterus. This, in the more common attitudes, is the cervix uteri. The child's head and upper part of the trunk contain more matter, in proportion to their surface, than the lower part of the body; thence the head will more generally fall down to the

cordance with fact, at least it does certainly not hold good at that advanced period of pregnancy when, as we have just stated, the child no doubt assumes the position with the head downward, viz., in the latter months. In order to suspend the fœtus by the cord, the placenta, from which the cord springs, would, in the upright position of the mother's body, require to be fixed at the fundus uteri. The placenta, however, is almost always implanted on one side of the uterus,[1] rarely at the fundus. Further, in order to suspend the fœtus, the cord itself, as the medium of suspension, would require to be at least considerably shorter than the length of the uterine cavity, in which it is supposed to perform this function of suspension. But it is far too long to effect such a purpose. It is in general from eighteen to twenty inches, while the whole depth of the pregnant uterus is not above twelve or fourteen inches. The physical conditions, in short, necessary for the supposed physical suspension of the fœtus by the cord, assuredly do not exist in the latter periods of pregnancy, when the fœtal head first comes to assume the position downwards.

3. When physical conditions exist the very opposite of those that have been supposed necessary to effect the suspension of the fœtus by the umbilical cord, these conditions do not make any corresponding deviation in the position of the child. When the cord is twisted and fixed around the neck of the infant, it makes the lower half of the fœtus, from the neck downwards, by far the longest and heaviest end of the supposed balance, and hence ought, according to the doctrine of gravitation, to make, in these circumstances, the pelvic instead of the cephalic end of the child the presenting part. Here, however, fact does not accord with theory. In about one in every seven cases, according to Dr Churchill[2] (in 174 cases out of 1271 labours), the cord is found coiled around the child's neck or limbs, and yet without the usual position of the head being altered by it. Out of 1417 labours among the patients of the Edinburgh Maternity Hospital,[3] the cord was twisted around the child's neck once, twice, or oftener, in 164 instances, or in one in every eight cases; and in seven other instances, it was coiled both around the neck and limbs; but without changing in any of the cases the position of the infant from its normal direction with the head downwards.

4. If the physical gravitation of the head of the child were the cause of the normal position with the head lowest, then this position ought to be found with more frequency and certainty when

lower part of the uterus."—*Anatomical Description of the Human Gravid Uterus*, p. 66.

[1] See Naegele on Auscultation, Translation of Dr West, p. 79; Velpeau's Traité d'Accouchemens, vol. i. p. 297.

[2] Researches on Operative Midwifery, p. 281.

[3] See Monthly Journal for November 1848.

the gravitation of the head from any cause was rendered proportionally greater than natural; with less frequency and certainty, when from other causes the gravitation of the cephalic extremity of the infant was rendered proportionately less than natural. The very contrary, however, of all this is the truth. In cases of intra-uterine hydrocephalus, the child's head is larger and heavier than usual; and sometimes it is so to an excessive degree. But this condition of the head, this increased preponderance and gravitation of it, does not render head presentations in these cases more common than usual, but the very reverse. In an excellent thesis on hydrocephalus of the fœtus as a cause of rupture of the uterus—my young friend, Dr Thomas Keith, last year collected the histories of 69 cases of intra-uterine hydrocephalus at birth. Of these 69 hydrocephalic fœtuses, 11 presented preternaturally, or 1 in 6. When the head and fœtus is normal, preternatural presentations occur, in proportion to other presentations, as 1 to 26. Hence, preternatural presentation was four times more frequent in hydrocephalic than in natural cases; while it ought to be more frequent in the former than in the latter, if the doctrine of gravitation were true. Again, anencephalic fœtuses, with the whole brain and arch of the cranium wanting, are still often found presenting naturally. I have been present at the birth of three anencephali that had reached the full term of pregnancy. All of the three presented with the deformed and diminished cephalic extremity over the os uteri.

5. When a human fœtus of the latter months is placed experimentally in fluid, in descending through the fluid the head does not turn and fall first to the bottom of the containing vessel, as the theory of gravitation takes for granted that it would. Thus, if a dead new-born fœtus be plunged into water, contained either in a vessel shaped like the uterus, or in a large bath, the part which gravitates, and strikes the bottom of the vessel or bath first, is the back or scapula, and not the head. M. Dubois first pointed out this fact as the result of numerous experiments which he had made on fœtuses from the fourth to the ninth month; and in several instances in which I have repeated the experiment, I have found the same result.

6. When the child dies in utero, it still continues to be subjected to the same physical laws as when it is alive. The mere death of the fœtus does not in any way remove it from the agency of gravitation. When all its vital actions have ceased, the body should in fact be more subject than heretofore to all influences, such as gravitation, which are merely physical in their character. The dead infant ought therefore, as frequently as the living, to have its head placed as the presenting part over the os uteri, provided the mere physical gravitation of the head were the reason and cause of

that position. Experience, however, amply proves that this is not the fact. In other words, experience proves that malpositions of the child, or the presence of other parts than the head at the cervix and os uteri, is in the latter months a far more frequent occurrence when the child is dead, than when it is still alive.

During the seven years that Dr Collins had charge of the Dublin Lying-in Hospital, 16,654 children were born within the house. Of these 16,654 children, 15,533 were born alive, and 1,121 dead. Of the 1,121 dead children, many no doubt died during labour; but 527 of them were in a putrid state, and consequently may be correctly regarded as having perished in utero, and before labour commenced. Among the 15,533 children born alive, 278 presented preternaturally, or 1 in 57; 250 presented by the pelvic extremity, or 1 in 62; 28 presented by the upper extremity, or 1 in 555; 15,255 presented by the head, or 98 in 100. On the other hand, among the 527 children born putrid, as many as 94 presented preternaturally, or 1 in 5; 88 presented by the pelvic extremity, or 1 in 6; 6 presented by the upper extremity, or 1 in 88; 433 presented by the head, or 83 in 100. This contrast between the proportions of preternatural and natural positions among the living and dead children, may be expressed in a tabular form as follows:—

Relative Proportion of Natural and Preternatural Presentations among Living and Putrid Children.

Presentation.	With 15,533 Living Children.	With 527 Putrid Children.
Cephalic Presentations . .	15,255, or 98 in 100	433 or 83 in 100
Pelvic Presentations . . .	250, or 1 in 62	88 or 1 in 6
Transverse Presentations . .	28, or 1 in 555	6 or 1 in 88

7. The position of the fœtus in utero, with the head lowest, is a vital action, and dependent upon the existence and continuance of life in the infant; and consequently it is not simply a mechanical result, and dependent upon the mere preponderating weight of the head of the child. The last paragraph (6.), proves that the position of the head at the os uteri, is by no means so frequent when the fœtus is dead, as when it is alive; that, in other words, its *vitality* is in some way or another connected with the existence of this particular position, with the head undermost and over the os uteri. One objection may be urged against this deduction, as derived from the preceding data regarding putrid children, viz., that as "putrid" fœtuses are often born prematurely, the frequency of their malposition may depend upon the prematurity of their birth, and not upon the fact of their death. In some of the data drawn from the Maternity Hospital of Paris, and published by Professor Dubois, there is an answer to this objection. I have already stated, that out of 73 children born prematurely, but alive, during the currency of the

seventh month of pregnancy, Dubois found that 83 per cent., or 61 in the 73 cases, were head presentations; the pelvic extremity presented in 10 instances out of the 73, or in one case in 7; and the shoulder presented in 2 instances. These children were, I repeat, born *alive*. But when the children born during the course of the same seventh month of utero-gestation were already *dead*, the results were very different. Out of 46 children thus born prematurely, but dead, during the currency of the seventh month, 45 per cent. only, or 21 in the 46 cases were head presentations; the pelvic extremity presented in 21 instances out of the 46, or in about 1 case in 2; and the shoulder presented in 4 instances. The following arrangement expresses these curious facts in a tabular form:—

Table of the Relative Presentations among Children born prematurely during the Seventh Month, as affected by the Life or Death of the Child.

State of Child.	Total Cases.	Shoulder.	Pelvis.	Head.	Proportion of Head Cases.
Alive .	73	2	10	61	83 in 100
Dead .	46	4	21	21	46 in 100

The data which I already adduced in a preceding page, prove that the fœtus does not take any very determinate position till nearly the commencement of the seventh month; and that during the course of the seventh month, the position with the head downwards begins to be assumed with considerable frequency and certainty. The data in the present table further prove, that the position with the head downwards is lost, or not taken, provided the infant be dead. Or, in other words, the table shows what I have stated at the commencement of the present paragraph, that the assumption and maintenance of the position of the fœtus with the head undermost, and over the os uteri, is dependent upon the existence and continuance of *life* in the child, and consequently is not a physical, but a *vital* action.

The next question, therefore, which meets us is this,—What is the nature and character of the *vital* action by which the fœtus obtains and keeps up its position in utero with the head lowest? The assumption and maintenance of any position of the body, if dependent not upon physical but upon vital causes, can result from no other known vital action than a muscular action. Human physiology presents us with no other known action or power whatever, except muscular action, that can produce motions capable of altering or regulating the position either of the whole body or of any of its parts. And the real question, therefore, resolves itself into this—What is the nature and character of the *muscular* action by which the fœtus assumes and maintains its position in utero with the head lowest?

SECTION II.—INSTINCTIVE AND VOLUNTARY MUSCULAR ACTIONS AS THE CAUSE OF THE ATTITUDE OF THE FŒTUS.

Some physiologists, as Cabanis, Ennemoser,[1] &c., have upheld that the unborn fœtus is already endowed with physical powers, and performs acts referrible only to the existence and exercise of mind. They maintain that metaphysicians have as yet one department of their science to investigate, viz., the state and degree of development of psychical life in the intra-uterine fœtus. Cabanis[2] holds that the unborn infant already possesses the consciousness of its own existence with the first traces of fundamental ideas, and has already wants and desires, and both the will and the power of exciting volitional muscular movements.

Long ago, Ambrose Paré,[3] and Chamberlen,[4] attributed the position of the head of the infant at the os uteri in labour to a psychical rather than a physical cause. But the author who has principally maintained and developed the idea, that the position of the child is a psychical result, is Professor Paul Dubois of Paris.

In 1832 this distinguished obstetrician communicated to the Academy of Medicine of Paris, an elegant and remarkable essay on the subject. M. Dubois' essay is published in the second volume of the Academy's memoirs, under the title of "Memoire sur la cause des Présentations de la Téte pendant l'accouchement et sur les determinations instinctives ou volontaires du fœtus humain."

After showing, by some of the arguments that I have stated in the preceding section, that the position of the fœtus, with the head lowest and over the os uteri, is certainly not the result of gravitation, M. Dubois, by a kind of reasoning by exclusion, comes to the conclusion, that the position in question must be the result of instinctive and voluntary determinations on the part of the fœtus,

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[1] Historich-physiologische Untersuchungen ueber den Ursprung und das Wesen der menschlichen seele. Bonn 1824.

[2] Rapports du Physique et du Moral de l'Homme, tom. ii. p. 431.

[3] After speaking of the infant, at the full term of pregnancy, requiring more food than it can now obtain through the vessels of the cord, Paré describes it as endeavouring violently to escape from the uterus to supply its wants, and thus, by its strong violence (*grande impetuosité*), breaking the membranes. When the womb then begins to open, "the childe (says he), pursuing the aire which hee *feeleth* to enter in at the mouth of the womb, is carried with its head downwards."—*Paré's Works,* English translation, p. 899.

[4] Of the celebrated obstetric family of the Chamberlens—the inventors of the forceps—we have few or no literary remains. Hugh Chamberlen, one of the sons, translated the work of Mauriceau, and has added a few sparse notes of his own. To the passage which I have already quoted in a preceding page from Mauriceau, regarding his belief in the preponderating weight of the head of the fœtus being the cause of its common position, Chamberlen affixes as an annotation, that the fœtal head comes lowest, and to the os "rather by a natural *propensity* than any weight of the head."—*Translation of Mauriceau on Diseases of Women with Child,* &c., p. 147.

for he confesses himself unable to conceive any other influence by which it could be affected.[1]

The terms, however, used by M. Dubois, "instinctive and voluntary," are so much of a conventional character, and are liable to have such different significations attached to them by different writers and readers, that it is necessary for us to understand the meaning which M. Dubois himself affixes to them, in order that we may justly appreciate his views and reasoning on the subject. M. Dubois enables us to do this by an illustrative explanation, which he himself gives of the terms, in a note appended to his essay, while, at the same time, he confesses how difficult it is, in relation to some notions and actions, to separate and limit the effects of instinct from the effects of volition and individual mental intelligence.

A bird builds a nest. The building of the nest is, says M. Dubois, an instinctive act, and the object of an irresistible impulse; as well as are the form or general plan and materials of its construction in each particular species. But, in his opinion, the means by which this instinctive determination is attained, such as the choice of an expedient place, and the search for, seizure, and carriage of the materials appropriate for the edifice, so far involve and imply, in different shades, acts of individual mental activity and volition—"voluntary determinations"—on the part of the bird.

By the word "instinctive," therefore, as applied to the attitude of the fœtus, M. Dubois implies the fact, that the situation of the head over the os uteri, is an act performed by the infant, independently of all experience and all imitation; and with the object and view of placing its head in the position which is most favourable for its safety and protection during labour. The word, in short, signifies that the *final cause* of the position is one of wisdom and foresight; and every person will, I believe, readily grant this part of the proposition. Any other position would endanger far more greatly the safety of the child, and consequently the whole object of the function of reproduction, viz., the continuance of the life of the species. The principal peril which the fœtus undergoes during labour, is the danger of asphyxia from compression of the umbilical cord. In head presentations, however, the risk of this accident is immeasur-

[1] "M. Dubois," says Dr Ramsbotham, "has ascribed the general situation to an instinctive impulse implanted in the fœtus, which inclines it to take the most favourable position for its escape—as the needle points mysteriously to the pole. But such a mode of reasoning and illustration cannot be considered either as argumentative or conclusive; it is, in fact, completely evading the question, after attempting to elucidate it; and the method he has taken can only be regarded as a cloak for human ignorance. It would, in my opinion, be much better not to endeavour to explain the secrets of nature, so deeply hidden, but to content ourselves with referring this also to a general, though not invariable law—a part of the great system which shows the design, and exemplifies the harmony that reigns throughout the whole works of Providence."—*Obstetric Medicine and Surgery*, p. 309.

ably less than it is in pelvic or transverse presentations. The whole figure of the child is that of a cone,—the head forming the basis of the cone, and the feet its apex. When the head or basis of the cone presents, and dilates the passages first, then, after the birth of the head, a single pain generally expels the body, and the chances of compression of the cord come thus to be rendered exceedingly slight. But when the apex of the cone (the feet or pelvis), is placed at the os uteri, and advance first into the passages, the cord is subject to much greater chances of compression between the mother and infant; for the higher and broader end of the cone then slowly, and with gradually increasing difficulty, makes its transit through the pelvis, the cord becoming more and more liable to compression during this transit. Many more children are consequently born dead under transverse and pelvic, than under cephalic presentations.

We acknowledge then willingly in this sense, the truth of M. Dubois' expression of the position of the child being an "instinctive determination," in as far as the final cause or object of the position is concerned. But we believe that he takes an erroneous view of the subject when he adds, that the mechanism by which nature effects this instinctive determination of the head downwards to the os uteri, consists of an almost constant series of movements, produced by "intentional determinations," and "small volitions" *(petites volontés)*, on the part of the fœtus. The position of the fœtus is, I believe, certainly the result of the movements which it itself performs in utero; but these movements are not of a volitional, and consequently mental origin, but simply of a reflex or excito-motory character. Before pointing out the proofs of this view, let us glance at what the intra-uterine movements of the fœtus really are; and in doing so, I shall principally follow and abridge the lucid statements of M. Dubois himself.

When we apply and press our hand over the pregnant uterus, in the latter months of pregnancy, movements of the fœtus are often produced, and these movements are simultaneously perceived by us and by the mother. Touching and irritating the fœtal head through the os uteri during labour, generally leads to the same result. The pressure of the end of the stethoscope upon a projecting part of the body of the fœtus, almost always produces fœtal movements. Sometimes it can be felt striking its limbs, as it were, against the instrument, or against the uterine parietes, when we have occasion to use auscultation during pregnancy or labour. "Pregnant females," says M. Dubois, "can generally make and provoke fœtal movements at will; for there are few mothers who, in attempting to seize or tickle through the abdominal walls the small fœtal feet which sometimes press painfully against and protrude these walls, have not felt the infant immediately draw them back, in order to take up another position, or answer by a rapid blow to the impression which it perceives." Warm or cold bodies when swallowed, or when applied

to the abdomen externally, have sometimes a marked influence upon the movements of the fœtus in the way either of increasing or suspending them.[1]

"Thus," adds M. Dubois,[2] "not only the susceptibility and the power of locomotion are exercised during fœtal life, but even the regular and almost constant succession of impressions perceived, and of movements resulting from these impressions, indicates sufficiently that in the fœtus there is the same connexion between these two functions as exists in the adult. The fœtus in the uterine cavity has the feeling of external impressions, and these command (so to speak) the exercise of its powers of locomotion; and this faculty is found nowhere in the animal kingdom without being associated with spontaneous and intentional determinations, and consequently with internal perceptions, which ordain them."

These fœtal movements, M. Dubois further shows, are generally exerted and repeated under certain determined conditions, as, for instance, under great changes of position on the part of the mother, as when she suddenly sits up or lies down. In some mothers, particular positions on the side, back, &c., always produce them. When the mother's stomach is empty, the fœtal movements sometimes become excessive. And he adds, "When it happens accidentally, during pregnancy or labour, that the umbilical cord of the child becomes compressed, the fœtus testifies by repeated movements both the sentiment of restraint which it feels, and its wish to be relieved from it."

From these facts M. Dubois[3] draws the following conclusions:— "The human fœtus, therefore, possesses the faculties of sensation and motion, and these faculties of sensation and locomotion are already related the one to the other in the same manner as they are destined to be during the whole life of the individual. In the narrow sphere of its existence, it has wants, the feeling of well-being and perhaps of pain; habits, and perhaps desires, and volitions; its movements are often the decided indications of these feelings and desires, as its cries will be after its birth. Further, if any of its movements appear to us to be altogether automatic, it is only because

[1] In this instance—viz., the apparent fœtal movements following the impressions of cold or heat applied to the surfaces of the abdomen or stomach—the uterine walls are probably *first*, if not alone, affected through their reflex system of nerves. All the different varieties of movements mentioned in the text have been maintained by Eggert (see *Rust's Magazin*, vol. xvii. p. 62) to be movements in the uterine or abdominal walls alone, and not in the fœtus. Every physician and mother can easily prove, by their own daily experience, the unsoundness of such a view. In speaking of the fœtal movements apparently following sudden external impressions of heat and cold, M. Dubois states, that "in rural economy they take advantage of this fact to ascertain if chickens have perished in their shells before the end of incubation. With this view they plunge the suspected eggs into cold water, and immediately the movements of some show the enclosed fœtuses to be still alive; the immobility of others show that in them the chicks have perished."

[2] Mémoires de l'Académie Royale de Médicine, Tom. II. p. 280. [3] Ib. p. 284.

we have not studied them so profoundly as to discover their cause or guess at their intention."

"Having demonstrated that the fœtal movements have an object, sometimes fixed, sometimes only to be guessed at, and that they may, consequently, be looked upon as true instinctive determinations, we have only further to prove that it is in consequence of a determination of this sort that the head of the fœtus in the mammalia is generally found situated at the pelvic end of the uterus. This point we shall demonstrate in very few words, for the proof itself results from the very facts which we have already adduced. If by some of these facts we have demonstrated that it is not by the laws of gravitation that the head of the fœtus is drawn towards the uterine orifice,—if by other facts we have shown that, during intra-uterine life, some acts depend upon an instinctive or voluntary action, then it appears to us altogether impossible not to class among these last the almost constant position of the head at the os uteri; for it is only possible to choose between this cause and mere chance (*le hazard*); the choice of intelligent men in this case cannot be doubted."

These conclusions of M. Dubois are, I believe, not what the facts themselves warrant and point to. We fully admit the truth of his premises regarding the extent and variety of muscular movements performed by the fœtus; we further believe, as will be subsequently shown, that these movements constitute the immediate cause and mechanism of the peculiar attitude and positions of the fœtus; but we altogether dissent from the deductions which he has drawn from his data with regard to the actual physiological nature or character of these movements. They are not the result of the "spontaneous determinations" of the fœtus: they do not prove, as he supposes, that "the faculties of sensation and motion are developed and exercised long before birth," already connected together, as in the adult, by intermediate mental operations, such as sensation and volition; they are not *psychical* nervous actions, but *physical* nervous actions; they are not, in short, volitional or sensorio-volitional movements, but movements entirely of a reflex or excito-motory character; movements not dependent on the cerebral system, but referable entirely to the "true spinal system."

It is well known that modern physiology and medicine stand indebted to the masterly and beautiful investigations of Dr Marshall Hall, for systematizing and elaborating our knowledge of a large and most important class of animal muscular movements, that are fundamentally altogether independent of sensation, consciousness, or volition, and consequently are not mental in their origin or working; that are connected with the spinal cord as their nervous centre or centres, and not with the brain, and may hence be performed when all influence from the brain is entirely removed;—and that originate in impressions, made principally upon mucous and cutaneous surfaces, which impressions being conveyed from these surfaces by afferent, excitor, or incident nerves (as they are variously termed)

to the spinal centre, lead thus indirectly to many and often complex muscular movements through some of the motory nerves connected with that centre becoming in consequence secondarily excited. The resulting movements are termed reflex or excito-motory, because they are induced by excitations transferred or reflected from one set of nerves to another, that is, from the excitor to the motory nerves, through an intermediate nervous centre. The movements of the fœtus in utero are of this reflex or excito-motory character. That they are so is, I think, proved by the fact, that in all important points they correspond with the most fixed characteristics of other undoubted forms and varieties of reflex or excito-motory muscular actions. The principal characteristics of true reflex movements consist,—1. In the anatomical conditions under which they are capable of being performed; 2. in the origin and character of the movements themselves; and 3. in the physiological object or objects with which they may be performed. I proceed to show briefly, that in these various particulars the movements of the fœtus in utero correspond to the proper characteristics of true reflex or excito-motory movements.

1. *The Anatomical Conditions under which the Movements may be performed.*—Dr Hall and others have shown, that true reflex or excito-motory movements persist in decapitated animals, when all influence of the brain, and consequently of sensation and volition, is necessarily removed. In instances of paraplegia in the human subject, dependent upon complete destruction and division of the spinal cord in the dorsal region, and hence when the agency of the brain was entirely cut off, reflex muscular movements sometimes remain in the muscles of the lower extremities, and are capable of being excited, sometimes violently, by slight irritations applied to the soles of the feet, &c.—the patient *seeing*, but not feeling, or being otherwise aware of the resulting movements in his own limbs.[1] That the fœtal movements are of the same character, is so far proved by the anatomical fact, that they may occur and go on during the period of intra-uterine life, and even for a day or two after birth, when, from malformation, the brain is entirely absent. The histories of cases of anencephalous monsters show that the intra-uterine movements are the same with them as with other children; and I have seen one survive for thirty-six hours after birth, performing various excito-motory movements during that time,—swallowing, crying, and moving the limbs when the skin was pinched or irritated.[2] These cases afford us evidence of this amount—that all the usual fœtal movements are capable of being

[1] Cases of the same kind have been recorded by Lallemand, St Hilaire, &c., &c. See Dr Hall's Work, p. 134; or St Hilaire's Histoire des Monstruosités, Vol. II.

[2] See the interesting cases recorded by Dr Barlow, Dr Budd, &c.; or Dr Marshall Hall's own work, On the Diseases and Derangements of the Nervous System, p. 234, &c.

performed without a brain, and consequently without the agency of mind, and hence are not of a sensational or volitional character, but fall within the class of reflex or excitory movements.

2.—*The Origin and Character of the Movements.*—If *after* birth we irritate the sole or palm of the new-born infant (whether awake or asleep), muscular movements are excited in the limb. These are acknowledged by all physiologists to be excito-motory movements, as much so as the closure of the lips around the finger or nipple. If, *during* labour, the foot or hand present, and we irritate in the same way the protruded limb, it will excite motion in it of the same kind, as I have in several cases taken an opportunity of ascertaining. During labour, irritation of the scalp of the child with the finger, in common head presentations, is usually followed in the same way by some movement of the head. Go a step further back in the life of the infant, and apply similar irritations to the limbs or surface of the infant through the abdominal walls *before* birth, or during pregnancy, and, as we have seen, the same results are obtained; movements of a similar kind are excited, and the irritated limb is moved away. But these movements are to the feelings of the mother exactly the same in character, as those which are usually and constantly perceived by her as made by the unborn fœtus within her. Their independence of the brain, and true excito-motory character, is proved by their existence in ancephalous fœtuses *after* birth, as stated in the last paragraph. They originate in excitations or impressions made upon the cutaneous surfaces, as many other reflex movements do. We shall afterwards see that the almost incessant and persistent character of the movements in some fœtuses during their intra-uterine life, is another evidence of their excito-motory nature. For the "true spinal system never sleeps."

3.—*The Physiological Object of the Movements.*—The acts and physiological objects of the reflex or excito-motory system are stated by Dr Marshall Hall in the following terms:—"These acts," says he, "are found to preside over two important classes of functions, viz., those of the *preservation* of *the individual*, and of the *propagation* of *the species*."[1] It would be difficult to describe in more terse and apposite words the objects aimed at and attained by the position of the head of the fœtus over the os uteri at the time of labour. The excito-motory character of the fœtal movements, as leading to this position, is so far proved by the result of the movements fulfilling both the ends for which reflex motions seem specially designed.

But *how* do excito-motory or reflex motions on the part of the fœtus place the head of the infant below its body, and over the os uteri? In the next sections I will attempt to answer this question,

[1] New Memoir on Nervous System. (1843). P. 51.

by showing by what means and mechanism these fœtal reflex or excito-motory actions make the child assume and maintain the common position with the head directed downwards; or force it to place itself in malpositions or preternatural presentations under various preternatural circumstances.

(*To be continued.*)

Article II.—*Suggestion of a National Decimal Pharmacopœia.*

By Marshall Hall, M.D., F.R.S., &c.

Very many years ago, a druggist's apprentice in Newark sold, by mistake, a powder containing opium for one containing jalap, the colour, and the position on the shelves of the two powders, being nearly the same. The medicine was destined for a child. The mother came shortly after its administration, in the utmost consternation, to say that the little patient had fallen asleep, and could not be roused.

Galvanism was then in the ascendant; and I went with an early friend, R. Cooke, Esq., of Gainsborough, taking the galvanic apparatus. The child was restored!

A few weeks ago, three grains of strychnine were, in the same manner precisely, sold for three grains of salicine: the effect was instant death to the unhappy patient.

It is, of course, impossible by any act of legislation to guard against such carelessness. But these facts afford an additional argument for the proposition which I am about to bring forward.

It is impossible to look over our Pharmacopœias without feeling that their complexity and diversity are very serious, as they are very unnecessary, obstacles in the way of the student and practitioner of medicine.

As to the student, his memory is taxed to the very utmost before he can venture to appear before the examining boards. The practitioner is not less "let and hindered" in his daily career of prescription.

An English physician would scarcely know how to prescribe in Edinburgh or Dublin; and even in England he will sometimes find it difficult to prescribe certain important remedies, the hydrocyanic acid for example, in a manner at once efficacious and safe—a difficulty which is increased on the introduction of any new remedy or remedies. In a word, no principle guides us or guards us in this most important department of our art. In order to set forth the defectiveness of our present Pharmacopœias, I need only advert to those preparations, each containing opium,—the pulvis cretæ compositus cum opio, the pulvis kino compositus, the pulvis ipecacuanhæ compositus,—not, however, in the same proportion, but in that of one-fortieth, one-twentieth, and one-tenth! It is the object of this very brief paper to

suggest such a principle—a principle which will become, I think, of equal utility to the student and the practitioner, and safety to the patient, under all these varied circumstances. It is that of a uniform Decimal Pharmacopœia in all the British dominions.

The first step to be taken will be to get rid of the varied modes of determining *quantities*. If we would attain accuracy, these must be limited to one, viz., *weight;* all other modes, such as that by *drops*, and that by *measures*, being discarded altogether.

Our next step is to discard all the confusion, even in regard to weights, which have so long prevailed, especially that between avoirdupois and apothecaries' weight. In vain is one weight cast circular and the other square. I am persuaded that, to this day, one ounce of the one is confounded with one ounce of the other, and this is a confusion between 480 and 437 grains.

But let *one grain*, however this may be determined, be the *unit*, and let the greater weight be *ten*, *one hundred*, *one thousand*, *decem*, *centum*, *mille*,—progressive multiples of that unit.

But especially let the officinal preparation of each energetic, and therefore dangerous remedy, solid or fluid, be such, that the medium dose shall be one grain, or more frequently ten grains, or one unit or decem by *weight;* that of others, as the tinctures generally, the infusions, &c., being such, that 100 grains or nearly $\frac{1}{4}$ oz. (℥i ℈ij) a centum, 1000 grains (℥ij ℈ij) or a mille, or nearly two ounces respectively, may be at once an efficient dose, yet far from inconvenient. The phials containing each of those distinct *orders* of preparation should be of *proportional* size; and those of each *order* should be arranged together, and distinctly from the rest. Every means should be adopted which can avert mistakes and danger.

Lastly, let the number of grains, or the terms decem, centum, mille, or millia, be written instead of those remains of antique darkness and mystery, ℈i. ℥i. ℥i. Ten grains are about the medium dose of the pulvis ipecacuanhæ compositus. All other important remedies may, like the opium and the ipecacuanha of this preparation, be so mingled with some appropriate inert substance, as to make the medium dose *ten* grains or one *decem*. The medium dose of the officinal preparations containing the chloride or the bi-chloride of mercury, of arsenic, of morphia, of hydrocyanic acid, and of strychnine, should, in like manner, be ten grains by weight. The same rule will apply to all the metallic salts, to iodine and the iodides, to the alkaloids, and to every remedy of energy and power. In like manner the medium dose of the liquor hydrargyri bi-chloridi, the liquor potassæ arsenitis, the tinctura opii, the acidum hydrocyanicum, should be *ten* grains.

If now a mistake be made in the hurry of prescription or of composition (and who is infallible?), no fatal or injurious consequence can follow. No one will be afraid of prescribing the ascertained safe or medium dose of the most powerful medicines even, his sanction, in the case of any untoward peculiarity of constitution, being

the authorized Pharmacopœia, founded on the experience of the most experienced in the profession. Each practitioner will, of course, judge whether, in the case of the delicate and the young, a minor dose then the medium dose should be first prescribed.

It may also be a question for careful consideration, what modification of the plan proposed may be required for infants and children. The plan itself will require the alteration and modification of the whole Pharmacopœia; but no great good is ever obtained without a proportionate effort. A principle, at least, will be introduced into our national Pharmacopœia for the first time. There is nothing in this arrangement of the Pharmacopœia to curtail the just freedom of prescription or of commerce, only I would distinguish the pharmacy from the warehouse and the shop.

Article III.—*On the Coagulability of the Urine in Scarlatina.*

By James W. Begbie, M.D.

The invariable presence of albumen in the urine, associated with the œdema or more general dropsy following certain cases of scarlatina, is a circumstance now almost universally acknowledged. From the various and very different statements published in regard to the condition of the urine in this disease, it is, however, difficult to estimate the exact relation in which the dropsy and the presence of albumen in the urine stand to each other. Martin Solon found the urine albuminous in twenty-two out of twenty-three cases of scarlatina. On the other hand, Philipp observed in Berlin, where scarlatina was prevalent, and anasarca could not be warded off, at least sixty cases in which the urine was tested both by heat and nitric acid, and no trace of albumen could be detected. In two cases of scarlatina, treated in Romberg's clinical ward for children, and in which there were no dropsical symptoms, Simon could find no albumen. In the case of a man, aged twenty years, which occurred in Schönlein's clinical wards, the urine was very albuminous during the period of desquamation, and continued so for four days without the occurrence of dropsy; and in another man who had no dropsy, Simon failed to detect any albumen.—(Simon's *Animal Chemistry*, edited by Dr Day, vol. ii. p. 280.) These statements are sufficient to show how easily a difference of opinion may exist on a subject, the facts recorded in regard to which are so very different. Simon remarks, the observations regarding the presence of albumen in the urine during the period of desquamation after scarlatina are so contradictory, that it has become a matter of very great interest to settle these conflicting statements by further researches.

Being led from various circumstances to take an interest in the question of the coagulability of the urine in this disease, I proceeded to the careful examination of the urine in every case of

scarlatina which presented itself to my notice, whether occurring in hospital, or submitted to me for examination through the kindness of friends. In this way I have conducted the examination of a large number of specimens of scarlatina urine, whether the disease became afterwards complicated with dropsy or not. In what I have now to state in regard to these, I wish to be understood as referring to twenty-one cases of simple scarlatina, not followed by dropsy. These cases must not, however, be considered as selected; they were, on the contrary, recorded in the exact order in which they occurred. I might have added others, and some bearing with equal force on the truth of the fact I am now desirous to advocate; but of others, the notes I possess are not sufficiently accurate, chiefly on account of my not having been able to examine the urine as often as in an investigation of this kind is desirable.

In all these cases albumen manifested itself in the urine, as recognised by the application of heat, or the addition of nitric acid. With two exceptions, the period of the disease at which the albumen appeared was a few days after the commencement of desquamation. In a few the process of desquamation had being going on for several days, and in one case was almost completed before the albumen appeared.

In the two instances noted as exceptions, albumen appeared in the urine from the first examination during the state of fever, and, while the eruption was present, in both cases brilliant. Its presence was satisfactorily explained by microscopic examination; in the one there existed blood, though not in such amount as to colour the urine, or indicate its presence by simple ocular inspection. The amount of albumen present in the urine was very small, and daily examination detected its regular decrease, till about the fifth day of desquamation, when its amount became much more appreciable, and continued so for some time. But the microscope ever after failed to detect any blood globules in that urine.

In the other case of exception, the early presence of the albumen was found to depend on the existence of pus. Though the changes noted in the former case were not so well marked in this, nevertheless a very decided increase in the quantity of albumen occurred during the progress of desquamation; and though the albumen continued to manifest its presence, the pus corpuscles shortly afterwards disappeared. An interesting circumstance in connexion with this case, was the existence of suppuration of both tonsils while pus occurred in the urine; and careful examination showed its presence to be unconnected with any disease of the external organs; evidences of the same action at work in different parts of the system in the same disease.

In regard to the duration of the albumen in the urine in these cases, the shortest period it continued was forty-eight hours, and excluding the two cases mentioned as having the albumen manifested from a very early day of the disease, the longest period dur-

ing which I noticed it was ten days. The usual duration was four, five, and continues six days. When the albumen once manifested itself, it continued to appear, or rather when, after having been observed, a future examination failed to detect it, I always found it a final disappearance—it never reappeared. The amount of albumen present in these cases was not very various; it was in general present in such quantity as to allow of the urine being called moderately coagulable. I never found the urine of simple scarlatina highly coagulable; in several cases, especially in those where the albumen appeared for the first time at a late period of the disease, it was only faintly coagulable.

The albumen, I think, will be found to appear earliest in cases most severe, and in these continue longest; and, on the other hand, its appearance will be retarded in less severe cases, and its existence be less obvious and more transient. Of these points I am not however quite certain.

From what has been stated in regard to the appearance, continuance, and amount of the albumen in this urine, it will be seen that a careful examination is at all times required for its detection. From the fact, especially of its continuance being limited, often almost transient, I believe that its presence in many cases may be overlooked. All of the urines of which I have spoken, were submitted to daily, some even to more frequent, examination; this I conceive being the only satisfactory way of determining the question.

I believe, then, that if careful examination of the urine be instituted, albumen in small amount will be found to exist in every case of scarlatina; the period of its first manifestation will generally be a few days after the commencement of desquamation, its continuance will almost always be limited to a few days, and its amount will generally be small.

Independent of the larger amount of albumen usually found in the urine, of the anasarca after scarlatina, there exists a much more valuable and invariable distinction in the amount of urine voided; this, in the case of dropsy, is always small, sometimes the secretion is almost entirely suppressed; on the other hand, I have never found the urine of the simple scarlatina, though containing albumen, to be diminished in quantity. Whenever the quantity begins to fail, dropsy may be apprehended.

For the detection of the albumen in the urine of scarlatina, it is necessary to employ both of the usual tests, the application of heat and the addition of a few drops of nitric acid. No appearance in the urine of desquamation is more common, than that of phosphates deposited by heat; and I have frequently found that the disappearance of the albumen has been followed by the presence of phosphates.

While the albumen continues in the urine, the density usually remains good, even high, 1·020 to 1·028 being the standard I have

generally found. As desquamation proceeds, and the albumen disappears, the density falls; but I seldom found it very low. It was usually lowest when the albumen became replaced by the phosphates.

Frequently the albumen in the urine was accompanied by a deposit of amorphous urates; and in one case by crystals of oxalate of lime.

By the microscope, I have never failed to detect the moulds or casts of the tubes of the kidney in the urine of the dropsical cases, associated usually with a dense deposit of epithelium. On the other hand, I have never observed these casts in the albuminous urine of the simple scarlatina.

ARTICLE IV.—*A Case of Acute Rheumatism, with Arachnitis and Pericarditis, supervening on Chronic Arachnitis and a remarkably Diseased Heart.* By HENRY ANCELL, Esq., late Surgeon to the Western General Dispensary.

THE subject of the following narrative was a man of rather a spare habit of body, of low stature, visage thin and pale, dark hair, a keen penetrating dark eye, great muscular power, and undaunted moral courage. He had been brought up in the wine trade; became a traveller at the age of eighteen; and from that period, if not before, was in the habitual use of wine in excess, frequently becoming intoxicated. His age was thirty-two years at his death; and he dated the whole of his illness from an attack of rheumatic fever ten years previously, produced by getting wet while travelling in Wales. In a letter from one of his connexions, addressed to me since his death, this attack is called "rheumatism of the heart." All the particulars I can collect respecting it are, that he was extremely ill, and his recovery so little expected, that his medical attendant declared he would not leave his room alive; which opinion having been communicated to him, he is said to have set it at nought, very soon afterwards ordering himself to be dressed, when he got down-stairs and walked into the garden. Contrary to the prediction of all around him, he recovered and removed to Bristol, but from that time diseased heart continued to manifest itself in a greater or less degree. A few months subsequently, while at Bristol, he appears to have had an attack of influenza and hæmoptysis, and to have again been reduced to "death's door." At this time his case must have excited considerable interest, probably from the existence of symptoms of that pathological condition of the heart which was found after death, since several gentlemen of eminence were named as having attended him, and from time to time examined his chest. He removed to Southampton, and was married in 1837. Six months subsequently to this, he had "a very severe attack of the heart, and likewise pleurisy;" he was

under the impression that he nearly lost his life in that illness, owing to a surgeon having bled him too largely; and he informed his wife, that he had no doubt at his death the surgeons of Bristol would be glad to give a good sum for the opportunity of examining his body. From this time, till the year 1840, he was temperate and steady, and although sometimes very poorly, he had no serious illness; his wife hoped that for many years he might preserve a fair state of health and strength.

In 1840, he again became very dissipated, and frequently had serious attacks of illness from inebriety; he was warned that if he continued his excesses they would produce delirium tremens, but it does not appear that a decided attack of that disease ever occurred. A surgeon attended him in Southampton, from whose advice he always considered that he had derived much advantage; this gentleman recommended the use of ether, which he was ever after in the habit of resorting to against syncope, to which, with other cardiac symptoms, he was now very frequently liable. The use of the shower-bath was also recommended at this time, which he continued till within a fortnight of his death.

Towards the end of 1842, "his health became much better, and he promised to amend his life." Shortly afterwards, however, he left his wife, and came to reside in London, where he carried on the business of a wine importer; he led a life of dissipation, and is said to have had four mistresses; his habits of intoxication progressively increased, and in the last few months of his existence, he was at times in a state of absolute frenzy. He considered that his health was much improved, and made a joke of his heart disease; but the parties with whom he lived described his disposition as a good deal altered; from being comparatively mild, he had become irascible and extremely violent. During the last eight months of his existence he was my patient. I was in the habit of seeing him occasionally after paroxysms of intoxication and attacks having some of the characteristics of delirium tremens, which, under the aid of a calomel purge, followed by diffusible stimuli and an opiate, soon passed off. The shower-bath had of late appeared to disagree with him, producing paleness and "a strange sensation," which induced him to jump out as quickly as possible. He complained of restless nights, and took an opiate of his own accord, occasionally, with considerable advantage. Although formerly subject to very frequent syncope, particularly in first rising in the morning, he had not fainted for a year past; he had two children by the woman he last lived with, and she was seven months pregnant with a third at his death. While in London he was frequently engaged in broils, was subject to very rough handling, and once had two of his ribs broken.

When he first became my patient, in February 1846, he called my attention to his chest, as an object which had excited the interest of many professional men. There was a slight enlargement of the

left side, but it was otherwise well formed, and there were several large cicatrices, particularly one produced by an issue, which had been placed over the region of the heart. I found a dulness over a larger surface than usual, and a loud bruit, which was a combined *rushing* and *rasping* sound in a high key, synchronous with the pulse at the wrist, and heard most distinctly over the region of the orifice of the aorta, being traceable along the course of this vessel; with a second natural short sound, distinctly audible, and but little or no interval between the former and the latter. The usual interval was distinct between the second and the first sound. There was a strong impulse, and sometimes a thrill given to the hand placed on the chest over the region of the heart. The pulse at the wrist was rather weak, about natural in frequency, and sometimes there was an intermission, or rather inequality, of force. After this, I prescribed for him from time to time. Under the influence of stimuli the rasping sound was louder, and the pulse more regular and equal; when the effects of the stimuli subsided, particularly if the action of these had been carried to intoxication, the pulse became quick, weak, unequal, and intermitted very much; but, under all circumstances, the second sound was perfectly distinct and natural. The medicine from which he derived the greatest comfort and advantage, was ether. From February, to the last illness in September, he led the dissolute life which has been described; drinking, driving about in a gig, getting into broils and quarrels, and sometimes exhibiting evidence of very great muscular strength.

On Tuesday, September 22d, he got wet through, and to this circumstance he referred his last illness. On the 23d he drank two bottles of wine, a pint or more of strong beer, and some brandy and water, and went to bed intoxicated. On the two following days he did not leave his home. On Saturday the 26th he went out, and was dancing in the evening.

I first saw him on *Sunday the 27th*. He complained of pains in the wrists and joints. A calomel pill and an aperient dose were administered, and a dose of opium and ipecacuanha at bedtime. *On the 28th* his skin was hot and dry, and he complained of pain in the head; his pulse was 80; tongue white and moist; urine rather high coloured; he knit his brows, as he was in the habit of doing on all former occasions of my visiting him; his pupils were rather contracted, but dilatable, and there was no apparent intolerance of light. Saline draught, with colchicum and hydr. c. creta, and the opiate dose at bedtime, were given. *On the 29th* he complained of pains in the wrists, but there was neither redness nor swelling; he felt "cold and curious about the heart," and was afraid of rheumatism attacking it, in which case "he was sure he was done for." His pulse was now between 100 and 110, and his heart tumultuous. Calomel and opium pills were administered at regular intervals, a saline draught between each dose, and sinapisms were applied to the chest. *On the 30th*, pain was complained of in the region of

the heart, and there was a very slight friction sound; the other symptoms being the same. He continued the calomel and opium; a large blister was applied to the chest, and a dose of castor oil administered. *Oct.* 1*st*, Pulse very quick, weak, and irregular; rasping bruit greatly diminished in force; slight mercurialization; no pain either in the cardiac region or the limbs, but he complained of weakness, and there was some tremor; skin moderately warm, and rather moist; urine bright-coloured, and scanty. The calomel and opium to be omitted till bedtime, and an opiate pill substituted. *Friday, Oct.* 2*d*, This morning I found, that instead of going to sleep, as he had hitherto done, he had been restless and delirious, becoming furiously so during the night; this subsided towards morning, and he was quite sensible on my visit, but he gradually lost the power of utterance, and was speechless by twelve o'clock at noon. There was considerable secretion of saliva, which he hawked up with ease, and he also swallowed without difficulty; tongue moist and white at the edges, deep brown in the centre and at the root. There were occasional very severe and remarkable attacks of tremor cordis, lasting a few minutes at a time, accompanied with violent tremblings of the muscles, so as to shake the whole frame, and followed by a state of exhaustion which threatened immediate dissolution; the rasping bruit was entirely gone. Ether and ammonia were administered at short intervals, and his accustomed stimulus of wine, in the form of negus, with nourishment frequently. 3*d*, The bruit had returned under the use of the stimuli; still quite speechless; brows constantly knit; pupils contracted, but not greatly so, and still sluggishly dilatable; head very hot; recurring paroxysms of tremor cordis; tongue and other symptoms as before. He takes nourishment with avidity, and turns himself from side to side without assistance. Contin. med. Ice to the head. 4*th*, In the early part of the day he appeared to be somewhat better; head cooler. He was still speechless, but appeared to be conscious of all that was going on around him, and to know the persons about his bed. He coveted and swallowed nourishment with avidity; involuntary micturition, but he was apparently conscious of it; brows constantly knit, and he made efforts to articulate and to put out his tongue, but was unable. The treatment was continued; but towards evening awful paroxysms of tremor cordis followed, and at length became constant, on which he rapidly sank exhausted.

Autopsy.—The heart was greatly enlarged, and its apex somewhat rounded; it weighed above twenty-three ounces, the increase of substance being nearly confined to the left ventricle, which was enormously hypertrophied, being an inch and a half thick at the septum, which projected into the right ventricle, and an inch and a quarter elsewhere; all the cavities were enlarged, particularly those of the left ventricle and the right auricle; the endocardium lining the auricles the tricuspid and mitral valves and a part of the chordæ tendineæ were opaque, thickened, and easily peeled off the

muscular structure; the edges of the mitral valve presented a beaded appearance, produced by hemispherical granulations, about a line or a line and a half in diameter, of a dull white colour, soft and velvety; the semilunar valves of the aorta were replaced by a rough, calcareous mass, presenting an irregular-ovoid, convex, polished surface towards the aorta, an inch and three quarters long by an inch and a half wide, in which the tripartite division of the valve was lost; the surface of this mass presenting to the ventricle was concave, still more irregular, and preserved the appearance of an obscure tripartite division. The mass was from half an inch to an inch thick, in some parts dense, and in others loose and friable, resembling mortar, with numerous granular nodules, some jagged laminæ, and projections resembling stalactites; one of the latter was in the form of a pyramid, projecting half an inch into the ventricle, with its sides and front applied against the auricle, above the base of that division of the mitral valve which covers the entrance into the aorta. Through this calcareous mass there was a straight fissure about seven lines long, but extremely jagged and irregular on the convex or aortic surface, and narrower and still more irregular on the concave or ventricular surface. In the recent state, the fissure admitted of being opened by pressure on the ventricle, to the extent of two lines, and it admitted of complete closure. The aorta was very large at its origin, but healthy; and the coronary arteries, quite pervious, were seated at either extremity of the long diameter, and just above the calcareous mass. This mass appeared to have compressed the coats of the aorta, upon the concave side, against the parietes of the left auricle; and a dirty fluid, with calcareous granules, and a diseased state of the muscular fibres, proceeded nearly, if not quite through, to the thickened endocardium, just above the adherent border of the mitral valve. There was also a patch of fibrinous deposit, about the size of a split almond, in that part of the left auricle which corresponded with the diseased muscular structure and the pyramidal projection from the main mass of calcareous deposit. The pericardium contained the usual quantity of limpid fluid; its reflected portion, in various parts of the heart's surface, was thickened and opaque; there were several dirty white rough patches, about six or eight lines in diameter, about the apex; and the base of the heart was glossy and uneven, from a very thin layer of coagulable lymph. The aorta was very large at its origin, its coats healthy; the pulmonary artery and its valves were also healthy.

The liver, kidneys, spleen, and pancreas were very large for the size of the individual; the spleen was considerably softened, and the pancreas enlarged.

On separating the calvarium and dura mater, there was found a subarachnoid effusion of a yellow, opaque, soft, but solid lymph, slightly greenish, and not at all odorous, from an eighth to a quarter of an inch thick, spreading over the anterior and lateral surface of both hemispheres, dipping slightly between the convolutions, and

admitting of being peeled off. The arachnoid covering the posterior lobes of the cerebrum and the cerebellum was highly vascular and congested; and there was about two ounces of serum at the base. The substance of the brain was healthy, being firm, of good colour, and fully but not morbidly studded with red points.

I have detailed the more remarkable symptoms only of this interesting case, which I consider worthy of record in several points of view. There is, perhaps, not a single instance to be found, which so well illustrates the extent to which disease of the semilunar valves of the aorta, and hypertrophy of the left ventricle may proceed, and still be compatible, not merely with the continuance of life, but with the greatest mental and physical activity; and without necessarily giving rise to urgent symptoms. The average weight of the heart in thirteen[1] fatal cases of this disease, was found to be a little above fifteen ounces, and the maximum under twenty-two ounces; but in the present instance it exceeded twenty-three ounces; yet death was not produced simply by the disease of the heart. There can be no doubt that, apart from other causes, death would have occurred, most likely very suddenly, from one of two circumstances—the increasing difficulty of propulsion of the blood into the aorta with a loss, or without a corresponding increase, of power in the ventricle,—or by the embarrassment of the circulation from a passage being formed into the left auricle, or even into the pericardium, by the destructive pressure of the calcareous mass. In this case also, the pathological results of endocarditis, and the association of this disease with diseased valves and hypertrophy, are well illustrated. The morbid systolic bruit and natural and distinct diastolic sound, viewed in connexion with the post mortem appearances, indicative of perfect closure during life both of the aortic and mitral orifices, is consistent with the explanation now generally received of the mode of production of the heart's sounds. The circumstances under which the rasping sound was diminished, or altogether lost, also show how it happens that, in some cases of the disease, no abnormal sound is produced; when, for instance, notwithstanding the hypertrophy, the power of the heart is diminished either from disease or debility of its own structure, or from the absence of appropriate and sufficient stimuli. As respects the brain, the pathological appearances leave no doubt that a state of chronic or subacute arachnitis existed long previous to the fatal attack.

The practical reader must not be too critical as to the treatment in this complicated case. An ill expressed attack of rheumatic fever, arachnitis with but few if any well marked symptoms, pericarditis with an extremely diseased heart, and a liability to sudden death from any rapid defection of power, in an uncontrollable patient, with worse than questionable attendance, was a state of things coincident, not very amenable to a rational plan of curative indications. Al-

[1] Bouillaud's Cases, quoted in Copland's Dictionary, vol. ii. p. 166.

though I have no knowledge of it as a fact, it is just as likely that a bottle of wine was administered, on the night he became delirious, as the medicine. Under all the circumstances, it will probably be considered that the moderate mercurial ptyalism produced was the most that art could do to obtain a respite of his fate.

ARTICLE V.—*Case of False Aneurism of the Anterior Tibial Artery successfully treated by Compression.* By JAMES ALEXANDER, Surgeon, Wooler.

As the treatment of aneurism, when it occurs in the arteries of the lower extremity, by compression applied to the femoral trunk, is a subject of considerable importance, and one on which there still exists room for difference of opinion, I am induced to record the following case, as it is only by a careful collection of individual instances that a general principle can be arrived at, or its necessary modifications ascertained:—

On the 20th December 1847, John Gordon, a man employed in watching the game on the Earl of Tankerville's estate, was fired at by a poacher from a gun loaded with small shot. Part of the charge took effect in the front and on the inside of the man's thighs, some few drops being as high as the groin and lower part of the abdomen, and one solitary lead drop having entered between the tibia and fibula of the right leg, about two and a half inches below the knee. As many as possible of the shots were removed when he was first seen, all of the wounds were lightly dressed, and the man directed to keep quiet, and observe a strictly antiphlogistic regimen. Under this mode of management, every thing went on favourably for a time; some of the drops were discharged by suppuration, and the wounds from others which had penetrated more deeply healed over, including the one already referred to below the right knee. In less than a fortnight after the injury, he could move about a little; but on the fifteenth day, after perhaps rather more exertion than usual, he suddenly felt as if something had given way deep between the bones of the leg. The limb swelled rapidly from the knee to the ankle, and the pain was excessive. In a few days the general swelling of the limb had somewhat subsided; but between the upper part of the tibia and fibula there was a manifest fulness and bulging forwards. On applying the hand, a distinct throbbing pulsation (obvious even to the eye when the circulation was excited) was felt, and an unequivocal aneurismal bruit was heard on applying the stethoscope. Pressure on the femoral artery arrested the pulsation, and diminished considerably the swelling and tension in front of the leg, which reappeared immediately on its discontinuance. The man complained of great pain, and there was considerable constitutional disturbance.

There seemed no doubt that the anterior tibial artery had been injured by the shot which took effect below the knee; that the walls of the vessel had given way at the time when the symptoms were so suddenly developed; and that now an aneurismal tumour had formed. It was evident from the situation of the wound, that to secure the vessel at the seat of the injury would be a matter of extreme difficulty, if not altogether impracticable; and upon a careful consideration of the whole circumstances of the case, it appeared a very fair one in which to try the treatment by compression.

After some time spent in allaying general irritation, and getting rid of any drops that seemed likely to come in the way of the compressing apparatus, two of Signorini's tourniquets were applied in the course of the femoral artery,

which were alternately slackened and tightened, as the man found it totally impossible to bear continuously the pressure of one. Although the pressure was scarcely ever sufficient to arrest entirely the current of blood in the vessel, but only to diminish materially its impetus, in less than a fortnight there was a visible diminution of the tumour, with some apparent consolidation of its contents. It required, however, the application of the tourniquets for *fifty-three days*,—during thirty-five of which graduated compresses, carefully adapted to the tumour, and supported by a bandage from the toes upwards, were employed in addition,—before pulsation finally ceased, and all sound became perfectly inaudible.

It is now upwards of nine months since the treatment of the case may be said to have terminated. The tumour has disappeared, and the limb is sufficiently restored to enable the man to follow his usual employment (that of a shepherd). Many anomalous sensations of pain, stiffness, and numbness remained long after the final cessation of the disease, and have not yet altogether subsided. The leg has a somewhat full and congested appearance, and is slightly œdematous about the ankle. The man (who is nervous and irritable) complains of what he describes as tearing interiorly when the leg is long in a dependent position, and the veins in some places are slightly varicose. Altogether I should be inclined to say, that the leg is scarcely in so satisfactory a state as those few cases have been, after a similar interval, in which I have had occasion to secure the femoral artery for popliteal aneurism.

On reviewing this case, and comparing it with those formerly recorded, it cannot be denied that there is no comparison between the pain of the ordinary operation, and the prolonged suffering which patients undergo during the application of the compressing instruments. The effects of the unavoidable interruption of the venous circulation, and of long-continued pressure on the nerves, must likewise be carefully estimated, before it can be admitted finally that the treatment by compression is capable of superseding in all cases the old mode of management. Still not a few cases will occur in which, from similar and various other combinations of circumstances, it may be of the utmost importance to be able to have recourse to means less formidable in appearance, and perhaps attended with less risk, than a surgical operation; and, in such cases at least, it cannot be doubted that the treatment by compression forms a most valuable addition to our remedial measures in the treatment of some aneurisms.

ARTICLE VI.—*Note on the Occurrence of Oxalate of Lime Crystals in the Urine.* By W. H. WALSHE, M.D. Prof. Clin. Med. Univ. Coll. Lond., &c.

IN the article "Adventitious Products," recently published in the Cyclopædia of Anatomy, I have referred (p. 79) to a table drawn up from a daily record of eighty-four unselected cases (forty-two in each sex) occurring in my wards in University College Hospital,[1] in which the absence or presence of oxalate of lime

[1] Session 1846–47, Winter.

crystals in the urine was carefully noted. I here present the table *in extenso*.

Diseases.	Males.		Females.	
	Oxalates present.	Oxalates absent.	Oxalates present.	Oxalates absent
Colica pictonum	...	1	...	...
Gastrodynia	...	1	...	...
Chorea	...	2	...	...
Softening of brain	...	2	...	...
Cerebral hemorrhage	...	2	...	...
Concussion of brain	...	1	...	...
Delirium tremens	...	1	...	...
Scarlet fever	1	...	...	...
Continued fever	4	...	2	2
Intermittent fever	...	...	1	...
Phthisis	...	4	1	...
Empyema (fistulous)	...	1	...	...
Pleurisy	...	1	...	...
Pleuro-pneumonia	3	3	...	...
Pericarditis, pneumonia	...	...	...	1
Bronchitis, simple	...	1	...	...
Do. plastic	...	...	...	1
Rheumatism, gonorrhœal	1	...	...	...
Do. acute articular	1	6	1	4
Do. chronic	...	2	...	...
Spermatorrhœa (hypertrophy of heart and mitral regurgitation)	1	...	...	...
Morbus cordis chronicus	...	2	...	3
Do. with recent pericarditis	...	...	...	1
Secondary syphilis	...	1	1	1
Neuralgia (intercostal)	...	...	...	1
Poisoning by opium (recov.)	...	...	...	1
Amygdalitis	...	...	1	...
Pyelo-nephritis (chronic)	...	...	1	...
Bright's disease	...	...	...	1
Metritis	...	...	...	1
Fibrous tumour of uterus	...	...	...	1
Dilatation of abdominal aorta	...	...	...	1
Hysteria	...	...	1	5
Obstructed Bowels	...	...	...	1
Anæmia	...	...	4	1
Erysipelas of face	...	...	...	1
Jaundice (tumour of liver)	...	...	1	...
Nephralgia	...	...	...	1
	11	31	14	28

It appears, then, that oxalate of lime crystals occurred in 28·57 per 100 of these cases; in about 26 per cent. of males, and 33 per cent. of females. Fifteen, of twenty-five cases in which they were present, were of acute disease; four others were cases of anæmia.

In seventeen of nineteen cases (those of acute disease and of anæmia), the crystals appeared at the onset of convalescence; so frequent is their appearance towards the convalescent period of acute affections, that their discovery may be considered to betoken coming recovery. To judge from these cases, the frequency of oxalate of lime crystals in the urine of rheumatic patients has been overrated; in the case of spermatorrhœa they appeared from time to time only, and did not seem to have any connexion with the abundance of seminal discharge.

Theoretical explanations might easily be adduced of the precipitation of this salt in the diseases, and at the periods referred to; but as none of them are susceptible of proof, they may as well be omitted. I need scarcely add, that these cases of temporary presence of oxalates are not to be confounded with others, in which oxalates are more or less permanently precipitated as the evidence of a peculiar diathesis.

ARTICLE VII.—*Statistics of the Cholera Hospital in Surgeon Square, from October 28, to December 16.* By WILLIAM ROBERTSON, M.D., F.R.C.P.Ed.

IN the outset of the observations which I feel it my duty at present to lay before the public, I must acknowledge my obligations to the industry of Mr James B. Balfour, the resident clerk to the Cholera Hospital, for what appears in a tabular form, and my own responsibility for the accuracy of all the statements contained, either in the tables or succeeding comments.

Our hospital experience has been roughly as follows:—

TABLE I.

	Males.	Females.	Total.
Admitted, ...	96	152	248
Died, ...	64	90	154
Recovered, ...	26	50	76
Sent to Infirmary, . . .	1	1	2
Still under treatment, . .	5	11	16

These cases, be it observed, cannot be *all* regarded as genuine cases of cholera, and consequently the table cannot, without correction, be used in estimating the exact ratio of hospital mortality. In cases which have not passed into the state of collapse, every one familiar with the disease knows, that there is occasionally difficulty in deciding whether a patient has or has not cholera; no *one* symptom can be esteemed pathognomonic; and, although the association of

several is our chief means of diagnosis, it is not always sufficient. The succession of certain symptoms may, it is true, materially assist us: thus a poor half-starved creature who lives in an infected locality, and who, after a few hours of diarrhœa, becomes affected with cramps and vomits his wretched meal of half-cooked potatoes, is often, and very properly, sent to the Cholera Hospital. Yet such patients sometimes do promptly recover, after the use of very simple remedies, and we have no certainty as to the precise nature of their disease. I say no *certainty*, because cases precisely similar are observed to run a fatal course when no treatment is adopted, and occasionally even in defiance of the most energetic practice. I have been in the habit, in the hospital, of considering and treating every case as cholera, which, on admission, presented the following groups of symptoms and probabilities:—Diarrhœa and cramps; diarrhœa and vomiting (if the evacuations are of the rice-water character); profuse diarrhœa, succeeded by notable reduction of the temperature of the body; sudden invasion of cramps, with or without diarrhœa, if the temperature be reduced, or the secretion of urine arrested. The probabilities are heightened, if the voice or expression be *choleraic*, if there be evidence of residence in an infected locality, of previous intemperance, or of abject poverty. Some such evidence is but rarely wanting in cases which are received into an hospital.

Of the 248 admitted, 18 were certainly not cases of cholera. Of the 18, there were 8 females, of whom 5 have recovered, 3 are yet under treatment; 10 were males, of whom 3 have been cured, 2 are under treatment, and 5 have died. The nature of the fatal cases was as follows:—1 had anasarca and effusion into the great serous cavities, occurring as sequelæ of scarlatina; 1 acute dysentery; 1 typhus, complicated with retention of urine from stricture of the urethra; 1 grey hepatization of the greater part of both lungs; 1 acute pneumonia and pericarditis. The table of true cholera cases should stand thus,—

TABLE II.

	Males.	Females.	Total.
Admitted, ...	86	144	230
Died, ...	59	90	149
Recovered, ...	23	45	68
Sent to Hospital (after cholera),		1	2
Under treatment, . . .	3	8	11

Of 149 deaths, at least 110 took place during the state of collapse. The other deaths occurred during the so-called consecutive fever, either during the short period of vascular excitement which succeeds the collapse, or far more commonly after imperfect reaction had given place to coma connected with suppression of urine. One male died after distinctly relapsing into the state of collapse.

The following table shows the ages of all admitted:—

TABLE III.

	Males.	Females.	Total.
From 0 to 10, . . .	22	12	34
10 ... 20, . . .	10	18	28
20 ... 30, . . .	17	52	69
30 ... 40, . . .	25	37	62
40 ... 50, . . .	12	22	34
50 ... 60, . . .	6	6	12
60 ... 70, . . .	2	3	5
Above 70, . . .	1	2	3
Unknown, . . .	1	0	1

An attempt has been made to ascertain the habits of all admitted, by questioning both the patients and their friends, and the result of these inquiries has been embodied in the following table:—

TABLE IV.

	Males.	Females.	Total.
Dissipated, . . .	37	39	76
Irregular, . . .	13	39	52
Doubtful, . . .	2	19	21
Sober, . . .	43	55	98
Unknown, . . .	1	0	1

It must be observed, that of the male patients there were 26 under the age of 14, and consequently not addicted to the abuse of spirits. If these children be deducted, it will be found that of 70 male adults, only 17 had, even upon their own confession, led tolerably regular lives. Deducting for the same reason 12 from the total of females, we find, out of 140 sick, only 43 reputed sober.

There are certain circumstances which seem to render females far more susceptible of cholera than males. In a former communication I instanced the states of pregnancy, of lactation, and of menstruation, together with disorders of the uterine system, as apparently favourable to the operation of the choleraic poison, and in corroboration of the opinion then expressed, beg now to submit a numerical statement.

Of 152 females admitted, 140 were *adults;* of the 140, 16 were pregnant, 9 were nursing, and 26 were the subjects of suppressed catamenia, or other uterine affections. Another cause probably operates in predisposing females to attacks of cholera; I allude to the fatigue and exposure to contagion inseparable from the duties of a sick-nurse—duties which devolve almost exclusively upon women. With every deference for the opinion held by many members of the profession, and promulgated by the London Board of Health, I feel constrained to admit the evidence in favour of the contagious properties of cholera, or at least, without insisting upon the literal appli-

cation of the term *contagion*, to believe that there is something in the presence of the sick likely to induce the same disease in the sound. If we grant the existence of a morbific poison emanating from the bodies of the sick, it seems natural to infer, that it must be energetic in proportion to its degree of concentration, and that the clearest manifestations of its virulence may be looked for in hospitals. Since the 28th of October, 18 nurses have been employed at different times in the Cholera Hospital; 5 of these have had severe attacks of cholera, and 3 have died. A sixth, who, for twelve hours, had frequent vomiting and purging, was treated as a cholera patient, and recovered. Yet, at the same time, a very large number of nurses were employed in the neighbouring buildings of the Royal Infirmary, and escaped the disease. The circumstances, habits, and diet of both sets of nurses are the same; the work is not more severe in the Cholera Hospital than in the Infirmary: the arrangements for ventilation and cleanliness are not defective in Surgeon Square; but, for nearly two months past, *no cholera patients have been admitted into the Royal Infirmary*. In illustrating the contagious properties of typhus fever, we are accustomed to regard evidence of this sort as tolerably conclusive.

Other two cases have occurred to me, which seem interesting in connexion with this subject. About the 20th November a young woman was suddenly seized with illness on the street; she complained of pain in the abdomen, and was immediately sent by some of the passers-by into the Cholera Hospital. On admission there was no evidence of cholera; but the patient was in labour, and in an hour or two gave birth to a fœtus in the third month. After four days' residence in the hospital, the patient was, at her own request, dismissed. She was, a few days afterwards, re-admitted, with unequivocal symptoms of the disease, and stated that the premonitory diarrhœa had manifested itself two days after her dismissal, while she was an inmate of the House of Refuge. This patient died after a few days' illness.

The second case relates to the importation of the disease from an infected locality into one which had for some weeks been healthy. A woman who had premonitory symptoms of the disease while residing in an infected lodging in the Horse Wynd, removed to Currie's Close in the Grassmarket, and, after passing thirty-six hours in this new locality, was brought into the Cholera Hospital. It had been remarked as rather singular, that for several weeks before no case had been reported from Currie's Close, notoriously one of the filthiest closes in the Old Town of Edinburgh; and it was not unnatural to expect that, after this importation of the disease into a locality where the predisposing causes existed in such perfection, other cases would speedily show themselves. Within the next few days several *did* originate in Currie's Close; and had not a number of its miserable inhabitants been speedily removed to the

Quarantine Establishment, in Forrest Road, there is little doubt that many of them would have suffered.

The following table embraces all the localities from which cases have been sent to Surgeon Square.

TABLE V.

Stowe, ...	1	Spring Gardens, ...	1	High Street, ...	14
Broxburn, ...	1	Canongate, ...	33	Soc. Close, Netherbow,	1
Newhaven, ...	1	Leith Wynd, ...	4	North Fowlis Close,	1
Clyde Street, ...	1	St Mary's Wynd,	3	South Fowlis Close,	2
Rose Street, ...	3	House of Refuge, ...	1	Bailie Fyfe's Close,	3
Shandwick Place,	1	Pleasance, ...	4	North Grey's Close,	1
Greenside Street,	1	Cowgate, ...	37	Fountain Close, ...	1
Greenside Row, ...	1	Horse Wynd, ...	3	Cant's Close, ...	3
Calton Street, ...	1	College Wynd, ...	16	Borthwick's Close,	1
High Riggs, ...	1	Hastie's Close, ...	2	Lawnmarket, ...	2
Bristo Street, ...	1	Forrester's Close, ...	2	West Bow, ...	1
Windmill Close,	1	Old Assembly Close,	5	Grassmarket, ...	42
Potterrow, ...	1	Burnet's Close, ...	1	West Port, ...	3
Middleton's Entry,	1	Bell's Wynd, ...	1	Quarantine House,	2
Leith. Cableson's Wynd,	2	Stevenlaw's Close,	2	Cumberland Street,	1
Leith. Tolbooth Wynd,	1	Blackfriars' Wynd,	3	St Andrew's Street,	1
Leith. Vessel in Roads,	1	Toddrick's Wynd,	8	Gayfield Square,	1
Cholera Hospital,	6	South Grey's Close,	3	East Richmond Street,	1
Abbey Hill, ...	1	Old Fishmarket Close,	5	Unknown, ...	1

Since my last communication to the *Monthly Journal*, little alteration has been made on the plan of treatment; but I can now speak more confidently of the merits of two of the remedies employed. Of the practice of *bloodletting* in the early stage of the disease, I have now had considerable experience, and am confirmed in my opinion of its efficacy in preventing or modifying the more formidable symptoms of the disease. Of 33 patients bled at the arm, 2 were not treated for cholera, but laboured under pneumonia and died; a third was bled to a trifling amount (ʒj) during the stage of reaction, and recovered. Of 30 who were bled to amounts varying from ʒxii to ʒxxx, 7 were males, 23 females; of the latter, 2, now convalescent, *had not cholera*. The result of the treatment applied to 28 cholera cases has been as follows,—of 7 males bled 2 have died; of 21 females, 4; 5 are still under treatment, but are believed to be convalescent. Of these 28 cases, it is quite possible that 3 or 4 may not have been real cases of cholera. The diagnosis of the disease in its early stage is far from certain; but had these patients not seemed to other practitioners as well as myself to be labouring under the epidemic pestilence, they would neither in the first instance, have been sent into hospital, nor after admission been subjected to the remedy.

The most marked effect of venesection seems to be the prevention or mitigation of the stage of collapse. Of the 28 patients bled, 12 had no distinct collapse; and of 16 who had algide symptoms, 10 recovered or are now doing well. The remedy is, I think, most

efficacious when cramps and oppression at the chest are prominent symptoms; at least the immediate relief to the patient has, in such circumstances, been most remarkable; and when the venesection has been followed by the administration of opium in full doses, the progress of the disease has sometimes been apparently arrested.

The injection of a saline solution into the veins has been resorted to in eighteen cases. In several of these there was no appreciable effect from the operation, and the patients died in the state of collapse. In others, the immediate improvement was most astonishing; patients who seemed pulseless and at the last gasp, were in a few minutes so far revived, that convalescence was looked forward to with some confidence. Hopes thus inspired were, however, uniformly disappointed; not a single patient survived the operation for more than nine hours. Instead of the brass syringe (the use of which is attended with considerable trouble and danger), I have of late substituted a column of fluid in a glass tube as the means of injecting the veins. The apparatus is constructed on the plan of the mercurial tube used by anatomists in injecting the lymphatics, and is extremely simple, consisting of a wide tube of thirty inches in height, open above to receive the fluid, and terminating below in a short caoutchouc tube, which is adapted by means of a tinned nozzle and stopcock to the vein-tube. The injecting force may be reduced by holding the glass tube in an inclined position. This apparatus has answered well, *mechanically* speaking; but the death of eighteen patients in succession has induced me finally to abandon the practice of injecting fluids into the veins, and I shall not resume it till I find that some other practitioner has met with more encouraging results.

Part Second.

REVIEWS.

Lectures on the Principles and Practice of Physic; delivered at King's College, London. By THOMAS WATSON, M.D., Fellow of the Royal College of Physicians; late Physician to the Middlesex Hospital, and formerly Fellow of St John's College, Cambridge. In Two volumes; the Third Edition revised. London: 1848. 8vo. Pp. 852, 881.

Clinical Lectures on the Practice of Medicine. By ROBERT J. GRAVES, M.D., M.R.I.A., &c. &c. Second Edition. Edited by J. MOORE NELIGAN, M.D., M.R.I.A., &c. In Two volumes. Dublin: 1848. 8vo. Pp. 586, 570.

WE hail with unfeigned pleasure the appearance of new editions of these excellent works. It is an unmistakeable evidence, that amongst the mass of trash issuing often under high pretensions from the

medical press, the profession are still able and willing to recognize and appreciate works of sterling value. By some unaccountable oversight on the part of our predecessors in the editorial duties, Dr Watson's work was never formally noticed in the pages of this Journal. It has now become, beyond all question, the standard work on the subject of which it treats; it is in the hands of every physician, surgeon, and senior medical student in every country in which the English language is spoken; it has passed scathless through the perils of criticism. Never within the memory of authors or publishers, has a medical work in two thick octavo volumes attained the enormous circulation of Watson's Lectures, a third edition having been called for within the space of five years, and being we believe already nearly exhausted; and, in addition to this, it must be recollected that these lectures also appeared in the Medical Gazette, and have been reprinted in America. We mention these facts as affording a sufficient reason why, in attempting to do tardy justice to the merits of this work, our notice of it will be comparatively brief. A work that has passed through so many editions, and that is already so widely diffused through the profession, is in one point of view, that is to say, in so far as any opinion of ours can influence its popularity, beyond the critic's province. We shall therefore confine ourselves almost entirely to the additions that have been engrafted into the two last editions, and perhaps venture to suggest a few alterations for the next.

With Dr Graves's work we shall deal in a somewhat different manner. Although it was noticed at some length in an early Number of this Journal (June 1843), the present edition contains so large an amount of new matter, that we shall be enabled to draw freely from its stores, without in the slightest degree trenching on the previous article to which we have alluded.

The first three lectures, two of which are devoted to the subject of clinical instruction, and the third to the proper mode of studying physiology and morbid anatomy, are very little altered. We regret to see that the statement regarding the system of clinical instruction in Edinburgh, the inaccuracy of which was pointed out in the review of his first edition, remains unchanged. If Dr Graves has been able to learn that "the professor of pathology is not only an avowed homœopathist, but has written a book with a view of proving the *truth*, and promulgating the doctrines of that ridiculous sect of quacks," and that the professor of chemistry is a "professor of animal magnetism," he might without much additional trouble have ascertained that clinical instruction is now carried on in Edinburgh in a very different manner from that which he has described. Indeed we may now tell Dr Graves, that by a series of judicious improvements, both in its medical and surgical departments, the clinical school of Edinburgh presents in its present condition all the best features of the German method, so strongly advocated by him; and, as some of these improvements were alluded to in our former review,

we think Dr Graves was not justified in reprinting his charges without alteration, or with such alteration only as was depreciatory. As public journalists we trust that we shall never be blind advocates of any special school, or any special party. It may be the object of a London writer to decry every thing north of the Tweed, of an Edinburgh writer to undervalue the labours of his brethren in the south, or of a Dublin writer to prove that Dublin is the metropolis of medicine; but for ourselves we can conscientiously aver, that we are influenced by no such local feelings, and while we gladly hail the progressive improvements that are perceptible to the unbiassed mind in the principal schools of the three countries of the United Kingdom, we shall resolutely expose every attempt to exalt one at the expense of another.

The fourth lecture is "on the pulse,"—a subject on which Dr Graves wrote a paper nearly twenty years ago.[1] Our readers are doubtless acquainted with the influence which is exercised by position on the frequency of the pulse, in the healthy human subject. We know comparatively little of the extent of this influence in disease; and although the following observations on the pulse, in cases of hypertrophy of the heart, were published in the paper we have referred to, we much doubt whether they are generally known to the profession.

"In all other diseases in which I have investigated this subject, I have found a difference between the frequency of the pulse in the erect, sitting, and horizontal positions; *but in six cases of hypertrophy with dilatation of the heart, no such difference was perceptible, although all these patients, at the time of my making the experiments, were in a debilitated state*, while in that in which the changes induced by position are the most remarkable. In four of these cases the existence of hypertrophy with dilatation has been ascertained by *post mortem* examination; and of the other two, a man and a woman, at present in the Meath Hospital, there can be no doubt of the state of the heart in one of them, while in the other the existence of hypertrophy is more than probable."—Vol. i. p. 467.

Dr Graves believes that this independence of the pulse on the position of the body only exists in those cases where the hypertrophy and dilatation are very great. This circumstance (as he observes), while it may detract from the value of the observation, *so far as regards the diagnosis between functional and organic diseases of the heart*, does not diminish its physiological interest. It is probably in a great measure, if not altogether, dependent on the increased strength and energy of the hypertrophied left ventricle, its contraction becoming so forcible as to conceal or overcome the minor influence exercised by change of position.

The following conclusions are drawn from a great number of observations on the effect of change of posture, on the frequency of the pulse in other diseases:—

"1st, That the greatest difference occurs in patients labouring under fever,

[1] *Dublin Hospital Reports*, vol. 5, 1830.

or in a debilitated state in consequence of fever or any other cause. It may amount to thirty, forty, or even fifty, between the horizontal and erect postures.

"2d, That this difference decreases after the first quarter of an hour in most cases, but always remains considerable as long as the same position is observed.

"3d, That, in persons not much debilitated, the difference is much less than that stated above, and often does not amount to more than ten.

"4th, That when the patient lies down the pulse rapidly falls to its normal standard.

"5th, That in some the increase in frequency is greater between the horizontal and sitting position, than between the latter and the erect; while in others the contrary takes place, so that generally the frequency in the sitting posture may be taken as a *mean*.

"6th, In persons convalescent from fever or acute diseases, I find it is extremely useful to the physician to ascertain the comparative frequency of the pulse in the horizontal and in the erect position. The greater the difference, the greater is the debility of the patient; *and consequently the more guarded must his medical attendant be in allowing him to sit up for any length of time*, particularly if the pulse on his lying down does not resume its usual degree of frequency." —Vol. i. p. 48.

In the *Retrospect* for October we offered a few remarks on the dicrotous or double pulse. Dr Graves regards it as a prognostic sign of much practical importance in many diseases.

"In fever, a dicrotous pulse, which is at the same time hard, is *a very bad* symptom, if it last more than twenty-four hours: when succeeded by epistaxis, and when it disappears after moderate epistaxis, it is not bad; it may, in the same fever, thus appear and disappear several times, but each time it becomes more serious. When in fever, a hard dicrotous pulse lasts for many days without any tendency to hemorrhage; the case in nine out of ten, ends fatally.

"In hemoptysis, epistaxis, and internal inflammation, a very hard dicrotous pulse sometimes occurs, which resists all treatment, and portends a fatal issue; no matter how much the other symptoms may improve, so long as the pulse continues of this character, the patient is in imminent danger."—Vol. i. p. 50.

There is nothing in the fifth and sixth lecture to call for any remark.

Lectures seven to twenty, inclusive, are devoted to the consideration of *fever*—the typhus fever of Ireland. Fully and excellently as this subject was treated in the first edition, we find in the present volume a considerable quantity of additional matter; and as no reference was made to this disease in our notice of the first edition, we shall enter pretty freely into it in the present article. The first of these lectures contains a short historical sketch of the epidemic of 1847; and various proofs are brought forward of the causes by which the epidemic outbreaks of typhus fever are produced in Ireland, and of the contagious character of the disease.

The ninth and four following lectures are devoted to the general treatment of fever. Dr Graves very properly exposes the awful fallacy of the assertion, that the treatment of fever is a matter of indifference. We fully agree with him, that "there is no disease in which diligent attention and skilful treatment are more frequently successful than in fever, nor is there any affection of equal importance in which our therapeutic means are more efficient and valuable." The im-

portance of securing an experienced fever nurse; the impropriety of allowing a fever patient to know the situation or extent of his danger; the necessity, in a bad case of typhus, of seeing the patient more than once a day, and rules for the choice and due ventilation of the chamber, are laid down with much judgment. Great stress is laid on the room being sufficiently large, to admit of conveniently holding two beds. The comfort afforded in many cases by the removal of the patient from one bed to another, every twelve or twenty-four hours, is very obvious. He objects to the use of chlorine in the sick-room as possibly injurious to the patient.

We proceed to his remarks on diet and medicines. He believes that "the starving system has, in many instances, been carried to a dangerous excess, and that many persons have fallen victims to prolonged abstinence in fever;" and he forcibly points out how the very symptoms resulting from this system have been regarded as indicative of inflammatory action, and how they have been met by even more rigorous abstinence. His practice is always to prescribe mild nourishment after the third or fourth day, and to continue it throughout the whole course of the disease. The greatest care must, however, be taken, that in protecting our patient from starvation, we do not fall into the opposite extreme, and overload the stomach,—a proceeding fraught with the greatest danger.

For the first three or four days, water, weak barley-water, and whey, will be sufficient. We may then give some well-boiled gruel made of groats, and flavoured with sugar, and, if there be no tendency to diarrhœa, with a small quantity of lemon juice. During the latter part of the first and the beginning of the middle stage of fever, Dr Graves frequently orders a little thin panado, morning and evening, in addition to the gruel. As the fever advances we give the gruel and panado more frequently, and allow a little good chicken broth, or animal jelly. If the broth or jelly give rise to heaviness, nausea, flushing of the face, and increased feverishness, we must not try any thing more than the gruel and panado for the next day or two. Grapes, oranges, and roasted apples, are strongly objected to, as producing tormina, flatulence, diarrhœa, and intestinal irritation.

Effervescing draughts and soda water are seldom given to the fever patients in the Meath Hospital, although they are a most grateful means of cooling thirst; their too frequent use often produces a tendency to tympanitis. Thirst, says our author, can be sufficiently assuaged by the use of whey or common water, acidulated with currant jelly or raspberry vinegar, in small quantities. A light infusion of cascarilla, with a few drops of hydrochloric acid, often succeeds admirably in allaying thirst; but whatever drink be taken, two or three spoonfuls, swallowed slowly, are as much as are necessary at a time.

Our space does not permit of our following our author through his remarks on tympanitis, low delirium, intestinal hemorrhage, and hiccup, as occurring in typhus fever; neither can we do more than present a very brief sketch of his general treatment of fever.

He clearly believes, in opposition to the views of most physicians of the present day, that an attack of fever may be cut short by the administration of an emetic. We quote the following paragraph on this subject:—

"If I were called to visit a patient who had been attacked with shivering, headach, quickness of pulse, increased temperature of skin, and lassitude, during the prevalence of an epidemic, or after exposure to contagion, and happened to see him a few hours after the attack, I should certainly bleed him, and administer an emetic; and I think he would have a very good chance of escaping the disease. A few hours make a vast difference in the chances, and after the lapse of twenty-four hours there is, generally speaking, very little hope of extinguishing the disease."—Vol. I. p. 138.

Although we have never been able to satisfy ourselves that we have actually cut short a true case of fever by the above means, yet we fully grant, with Dr Graves, that at the commencement of an attack, it is very good practice to administer an emetic. In cases where an emetic seems contra-indicated, he advises that, in order to moderate the feverish excitement, and render the disease milder and more manageable, we should prescribe very small doses of tartar emetic (1-20th of a grain in a table-spoonful of barley water every second hour), or two or three grains of James's powder, with an equal quantity of mercury and chalk, every three or four hours. We are glad to see a strong protest against the administration of powerful cathartics in fever cases. It is strange that, notwithstanding all that has been written on the subject, the practice is still very common. Almost the only cases in which active purgatives are required in fever, are when there is determination of blood to the head, and when there is preternatural fulness of the abdomen.

With regard to bleeding, Dr Graves observes, that there can be no doubt that, in conjunction with an emetic, it is often successful in arresting an attack of fever; it is, however, only during the first twelve or twenty-four hours that we can hope to extinguish it by this means.

"You may also," he adds, "have recourse to venesection within the first day or two, for the purpose, not of arresting fever at once, but of lowering inordinate vascular action, in persons of robust habit, and when the fever sets in with violent headach, great heat of skin, and a firm, bounding pulse. We do not, however, at present meet with many such cases, nor are we often called in at a period when venesection might be advantageously practised. The physician seldom sees a case of fever till the third or fourth day, and then it is too late to think of general depletion by the lancet."—Vol. I. p. 146.

Moderate as Dr Graves is in reference to bleeding, we are not inclined to go along with him on this point. We believe it will be found that, in the most successful practice (we are now speaking of fever as it occurred during the last eight or ten years in London), venesection is *never prescribed unless there is some well-marked local inflammation;* and even then no blood should be wasted. The following remarks on the application of leeches, in all respects coincide with the results of our own experience:—

"You may leech then, freely, and without any particular caution, in the commencement of fever, whether it be for cerebral, or for thoracic, or abdominal symptoms; but, as the fever advances, you must exercise more discrimination and care, both as to the number of leeches you apply, and the time you allow them to bleed. . . . . In leeching the chest or abdomen, in particular, I advise you never to have recourse to fomentations with the view of getting more blood from the leech-bites. Fomentations are too often a source of fresh mischief in cases of this kind, leading to exposure of the patient to cold, and to the annoyance of having his linen and bedding kept wet for hours together. Always give directions to have cupping-glasses, or hot dry flannel cloths applied as soon as the leeches drop off; and you will have less difficulty in arresting its flow afterwards—a point of some importance in cases where the loss of such a trifling quantity of blood is often of great moment, and likely to have a very powerful effect on the state of the patient."—Vol. I. p. 148.

The use of blisters in fever is next considered. They may be employed either as stimulants, or as evacuants and derivatives. With the former object in view, they are applied as flying blisters; that is to say, for a space not exceeding two or three hours, and solely with the view of producing a stimulant effect. We have occasionally seen their good effect, when employed in the manner recommended by Dr Graves, in cases of great depression, when the heart's action was feeble, the respiration short and imperfectly performed, and there was a tendency to faintness and sinking. One of the principal uses of blisters as evacuants and derivatives, is in the cases of cerebral excitement, which we suspect are much more common in Dublin than in London. Dr Graves relates several desperate cases, in which life seems to have been saved by blistering the shaven scalp, or rubbing it with tartar emetic ointment.

When we have recourse to blisters in pulmonary affections, whether they be simple or complicated with typhus, we must "recollect that much good may be effected without leaving the blisters on for a long time, or until they rise fully; and also that, when risen, it will not be necessary to cut them at once and let out the effused serum." We can personally bear evidence to the success of this mode of applying blisters to children; and, while little or none of the effects of the remedy is lost, there are never any of those troublesome sores which so often follow the ordinary mode of application. We are glad to see that Dr Douglas Maclagan's method of dressing the blister surface with cotton—a process first described in the pages of this Journal—is noticed in terms of high commendation.

After a few remarks on the advantage of hot fomentations to the head in certain forms of cerebral disturbance, and some directions showing the inutility of administering mercury to fever, we come to the subject of tartar emetic in this disease.

In cases of great cerebral excitement in fever, Dr Graves lays great stress on the medicine, duly associated with leeches and blisters. When the patient refuses his medicine, it may be secretly mixed with his ordinary drink, till its effect is shown on the cerebral circulation. Another expedient is to give it in the form of enema. Two or three grains may be dissolved in four or five ounces

of mucilage of starch or isinglass, and injected with the aid of a long flexible tube, so as to pass high up into the bowels. "In this way," says Dr Graves, "you can secure all the good effects of tartarized antimony, in overcoming the congestion of the brain, and procuring sleep."

Two chapters, and very instructive ones they are, are directed to the consideration of the use of tartar emetic and opium in combination. We believe that the entire credit of introducing this combination, with the view of allaying cerebral symptoms and inducing sleep, is due to Dr Graves. We have only space for a few of our author's concluding remarks on this subject :—

"One fact connected with the cases just related is very striking, viz., the small quantity of laudanum which in most of them was sufficient to induce sleep ; a circumstance only to be accounted for by the presence of the tartar emetic, which no doubt exerts, when given in duly regulated doses, a powerfully tranquillizing effect on the nervous system. It is also deserving of remark, that the combination very seldom gives rise to any of the unpleasant symptoms that so frequently arise when opium alone, or any of its preparations, are given with a view of producing sleep at an advanced period of fever. The addition of one ounce of mucilage and one ounce of simple syrup to the [eight or ten ounce] mixture, seems to render it less likely to disagree with the stomach. Towards the termination of fever, it not unfrequently happens that a sudden or gradual determination of blood to the head arises, and which requires a repetition of a modified system of antiphlogistic treatment, aided by blisters. This state, I have reason to believe, may be often prevented from occurring, by a timely attention to procuring sleep ; for a patient in fever, who has passed several sleepless nights, or on the verge of cerebral congestion or inflammation, as is testified by headach, wandering, and the redness of the conjunctiva. Here it is that the treatment I recommend is so advantageous, when timely applied ; for if it be deferred till cerebral inflammation has set in, opium in any shape is worse than useless."—Vol. I. p. 227.

"Before concluding, it is right to remark, that the relative proportions of tartar emetic and laudanum in the mixture, must be varied according to circumstances. When congestion of the brain is known to exist or is feared, the tartar emetic must not fall short of four grains in the eight ounces, while the laudanum should not exceed half a drachm ; but when nervous symptoms predominate, the laudanum may amount to one drachm, and the tartar emetic to two grains ; no general rule, however, can be laid down, *and the practitioner must in all cases watch the effects of this medicine, from hour to hour,* until he ascertain whether it agrees with the patient or not. Where a life is at stake, we must spare no pains, and must not reject a remedy because its powers render it an instrument of good or evil, according as it is administered carefully or otherwise."—Vol. I. p. 228.

We have given these extracts because we have a deep feeling of the importance of this mode of treatment in these cases of cerebral excitement. We have seen the happiest effects resulting from it, both in hospital and in private practice in Edinburgh and in London, and regret that the practice is not so generally followed as it deserves to be. There is not the slightest reference to it in Watson's Lectures.

Dr Graves enters at some length into the consideration of Dr Stokes' well-known memoir "on the Phenomena of the Heart's Action as an Index for the Administration of Wine ;" and although he does not concur in all the views contained in that essay, he fully

agrees with the author in his great practical conclusion, "that in the diminished impulse and in the feebleness or extinction of the first sound, we have a new, direct, and important indication for the use of wine in fever." He likewise adds some excellent practical remarks on certain symptoms commonly supposed to contra-indicate the exhibition of wine and opium.

1st, When, at an advanced period of fever, the tongue is dry, of the colour of old mahogany, or coated with a yellowish brown fur, and protruded with difficulty, wine and opium are often of the greatest advantage.

2dly, Suffusion of the eyes is often dependent on want of sleep; in such a case its presence does not contra-indicate the use of wine and opium, if other circumstances call for their exhibition.

3dly, A hot and dry skin does not necessarily contra-indicate the exhibition of wine and opium, particularly when there is at the same time a tendency to coldness of the extremities.

4thly, With regard to delirium, it is often a question, when this symptom is present, whether wine or opium should be given. According to Dr Graves, and we fully agree with him, these medicines are never applicable when the delirium is violent and continuous; but there are cases when the patient may mutter and speak to himself, when he may point to imaginary appearances, when it may be difficult to keep him quiet in bed, and when he may even rise occasionally—in short, when the disease assumes much the character of delirium tremens—where wine and opium are of the greatest value.

5thly, Heat of the head and face, redness of the cheeks, and strong pulsation of the carotids, are well known as contra-indicating wine or opium; but yet, in the advanced state of fever, the face may be suffused, flushed, and hot, and yet wine and opium may be our only resource.

6thly, When there is very severe headach, which cannot be relieved by local applications, by vascular depletion, or by purgatives, a full dose of opium will sometimes produce sleep, from which the patient awakes nearly free from headach. "Before having recourse to this remedy," says Dr Graves, "the effect of a blister to the nape of the neck ought to be tried. In the more advanced stages of fever, the headach, or rather the heaviness felt in the head, is something very different from the throbbing acute headach just spoken of, and constitutes no contra-indication to the use of wine and opium."

7thly, With regard to the pulse, Dr Graves considers that its frequency affords no test for or against the administration of wine and opium. "No one would ever think of exhibiting these remedies when the pulse is strong, and more particularly when it is strong and hard; but the case is otherwise when it possesses only a certain degree of *hardness*, and is at the same time small and thrilling, not resisting compression with the force the sensation of its hardness leads us to expect."

With this subject we must conclude our remarks on the lectures

devoted to the consideration of fever. If our readers suppose that we have abstracted all the practically useful matter contained in these lectures, we must assure them that we have only drawn lightly from a superabundance of rich stores; and that they cannot fail to derive much sound instruction from their careful perusal, provided it is borne in mind that the fever described by Dr Graves is for the most part maculated typhus,[1] and that the cerebral system seems usually somewhat more involved than in the ordinary London or even the Edinburgh fevers.

We shall resume the consideration of these volumes in our next Number. We cannot, however, conclude without expressing our sense of the obligations under which Dr Neligan has placed the profession, by his careful and judicious superintendence of this edition of Dr Graves' work, and particularly of the added matter; a care the more important in its results, because the last edition was not by any means well edited. G. E. D.

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1. *Reports on Asiatic Cholera in Regiments of the Madras Army, from* 1828 *to* 1844, *with Introductory Remarks on its Diffusion and Prevention, and Summary of the General Method of Treatment in India.* By SAMUEL ROGERS, F.R.C.S., and Surgeon of the Madras Army. London: 1848. 8vo. Pp. 267.

2. *Medical Report on the Causes, Character, and Treatment of Spasmodic Cholera in Her Majesty's* 86*th Regiment at Kurrachee, in June* 1846. By ALEXANDER THOM, Surgeon to H. M. 86th Regiment. (Printed by order of the House of Commons, 21st March 1848.) Folio. Pp. 50.

3. *Lectures on Asiatic Cholera.* By GEORGE ROSS, Surgeon to the Western City Dispensary. (Medical Times for October, November, and December 1848.)

4. *Account of the Practice in the Edinburgh Cholera Hospital.* By WILLIAM ROBERTSON, M.D., F.R.C.Ed. (Monthly Journal for December 1848, and January 1849.)

THE number of detached papers and memoirs on the subject of cholera which has emanated from the press within the last few

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[1] "The following," says Dr Graves, "are the most remarkable of the distinct varieties that have come under my notice:—

"1st, Simple continued fever, without maculæ, or any noticeable determination to particular organs. 2dly, Continued fever, without maculæ, with determination to some organ. 3dly, Continued fever, with maculæ. 4thly, Continued fever, accompanied *from the very beginning* by gastric derangement and epigastric tenderness. 5thly, The last-mentioned species, but in a more intense form, having black vomit and yellowness of the skin superadded. 6thly, Continued fever, with petechiæ.

"I have observed each of these varieties of fever constituting epidemics which lasted for longer or shorter periods; but with us, the dominant type of epidemics is the maculated fever."—Vol. I. p. 105.

months is so overwhelming, that we have found it impossible to put the whole, or even the majority of them, at the head of this article. We have therefore selected from these, for formal notice, a few which appear to us to have added more largely than others to our positive knowledge of this disease, or to have been founded on more extensive opportunities of observation than usual; and we shall endeavour, in discussing them, to do justice to whatever plausible speculations or well-recorded observations have fallen under our notice in other quarters. We are obliged, indeed, to confess, after a perusal of nearly all the publications on this subject which have passed through our hands, that the proportion of profitable matter is comparatively small. A great number of these documents must be considered as special pleadings in favour of some theory or mode of practice adopted with an exclusiveness and precipitation which augurs little good; and in not a few of them we find assertions so confident and unqualified, as, when taken in connexion with the opposite experience of others, to impress us with the strong conviction that they are utterly valueless. Nevertheless we believe that those who are best qualified to judge, are convinced that the state of medical opinion is improved since 1832, and that the experience of that epidemic has not been lost upon us; there being now widely prevalent a wise scepticism as to alluring theories and unqualified statements, which, however it may be carried to excess by some minds, is certainly in a great measure warranted by our experience of the former epidemic.

The volume of Reports edited by Mr Rogers is a well-timed production, and has all that peculiar value which belongs to military experience in medical matters. The individual documents of which it is composed appeared originally in the *Madras Medical Journal;* they refer to many different epidemics from 1828 to 1844, and are, as might be expected, of different degrees of merit and of minuteness of detail. This very variety of character, however, is valuable; and we have no hesitation in pronouncing this volume one of the most important contributions which we have yet seen to the history of this disease as it occurs in India.

The Report of Mr Thom, which was printed by order of the House of Commons during the present year, relates to the appalling epidemic which occurred in 1846 at the village of Kurrachee, in Scinde, and which carried off large numbers not only of the villagers, but of our troops, native and British, which were stationed there. This Report is of great length and importance; and although in some places rather arbitrary and hasty in its conclusions, is ably compiled, and, on the whole, excellently written.

The perusal of these important documents at the present time, when the disease to which they relate is under our own observation, is peculiarly instructive. We shall place before our readers a few of the reflections which the perusal of these volumes has suggested to us, without pretending, in doing so, to state any thing

entirely new, but in the hope that we may furnish to some minds a few of those guiding ideas by which the present juncture may be improved, and its data rendered valuable both for the purposes of science and art.

The spasmodic or pestilential type of cholera in India, although resembling, in most respects, the disease now among us so strongly as to leave no doubt of its identity, presents some peculiarities in the country of its birth which are not unworthy of notice. The most important symptomatic distinction is the greater rapidity of invasion and greater intensity of the attacks in India than in Europe; and, in particular, the rapidity with which the disorder involves large bodies of men. The most frightful instance of this fact was in the 86th regiment at Kurrachee, a force somewhat more than a thousand strong, where the epidemic, after dallying from the 11th to the 14th of June with small numbers, suddenly seized, within twenty-four hours, the alarming number of forty-seven of the troops. The next day, the 16th, brought the disease to its acme by the admission of 105 cases into the hospital; the 17th added an almost equal number —104;—a tenth of the whole troops being thus struck down in a single day. After this, however, the short-lived epidemic began to wane; the numbers next day sank to forty-nine, and during the three following days did not exceed twenty-five; after the 22d of June they subsided to small numbers. As it continued to occur till the 20th July—little more than a month after the first seizure—and in this space of time 410 individuals, two-fifths of the whole force became affected, and 238 died.

Although in India the rapidity of invasion and extension of the disease is so great, it does not appear to be in general more fatal to those attacked than in this country; the mortality being generally in troops from 30 to 40 per cent., and seldom exeeeding the half of the seizures. This is shown by a very large series of numerical returns relating to the Madras Presidency, and embracing all the regiments affected with cholera from 1821 to 1844.—(See Rogers' Reports, p. 6). This state of the mortality, notwithstanding the severity of the disease in the stage of collapse, may be not improbably owing to the remarkable fact, that the period of reaction, which is here so fraught with secondary dangers, is in India commonly unattended by any accident: the consecutive fever being slight and of short duration, and usually unaccompanied by any tendency to coma, which in this country is so common.

The most striking peculiarity, however, which is manifested by this disease in India, is its fixed and endemic character, particularly in certain localities. In this respect it seems to bear much the same relation to India that fever does to this country, especially to Ireland; lingering amid the population of particular towns and districts in all seasons, and from time to time breaking out in an epidemic

form over the country at large. This is well illustrated by Mr Rogers, in the introduction to the Madras Reports:—

"Since the first appearance of cholera in 1817, India has never been entirely free from it; occasional cases brought on by irregularity of diet, over-fatigue, and exposure, are every where met with; and an annual return of sick of a regiment without a notice of such an occurrence is an extraordinary event. Besides, we find that the disease may prevail amongst the inhabitants of some streets in a town to a great extent, whilst the neighbouring streets and the whole of the surrounding country are free from it. I have several times known such an occurrence in Madras. The Coom river winds very circuitously through Madras, and in its meanderings it nearly encircles the village of Chintandrepett. This river was made a necessary of by hundreds of natives daily throughout the year; and when the monsoon was heavy, and the bottom of this *Augean* stable was thoroughly cleansed, no ill resulted from it; but if the monsoon failed, and the river remained uncleansed, when the hot weather returned, the water became low, and the filth at the bottom was exposed to the heat of the sun, the smell was most offensive, and an attack of cholera was the certain result, its only victims being the inhabitants residing within a short distance of its banks.

"In some years, however, the disease assumes an epidemic form, and becomes universally spread in all directions. We hear of the towns throughout the whole country being affected, and it is found making its inroads amongst the troops in all our garrisons; at such a time, should any ill-starred regiment be obliged to move from one station to anothor, its ranks are certain to be considerably thinned."—Pp. 4, 5.

"In towns the first victims usually are the poorest of the inhabitants, living in dirty low situations, and on bad food, and I have known the natives themselves attribute an outbreak of cholera to a bad sort of rice, and an inferior species of fish, which frequently forms the diet of the poorest classes of natives. The disease, having once commenced amongst these people, spreads itself to portions of the town inhabited by the better classes."—P. 9.

The disease seems to have a preference for particular places, where it may be considered truly endemic or indigenous. It has also a peculiar disposition to attack large congregations of human beings, particularly when crowded together in imperfectly ventilated lodgings; thus it is said to be an annual accompaniment of the festival of Juggernaut, after the arrival of the pilgrims, who swell the town of Pooree from its 50,000 inhabitants to 150,000. It appears also to follow large masses of men from place to place; and regiments on the march are not only particularly apt to be affected by the disease in a severe form, but continue to be haunted by it even in unaffected localities, carrying about with them apparently the mysterious seeds of the disorder. This peculiar tendency of heavy marches, under the hot sun of India, to act as a predisposing cause of the disease, is illustrated by several instances in the reports before us, where regiments became severely affected in passing through districts where the cholera was a frequent scourge, even at times when it was absent.

Thus, in various parts of Mr Rogers' volume, we have instances of the breaking out of the epidemic on many different occasions, and in different regiments, in passing through the line of road from Chittoor to Masulipatam, in the Ghaut mountains.—(See p. 11 of the Introduction, and chapters i., vii., xii., and xiv). In at least four of

these cases, the disease, which is extremely frequent in this locality, appears to have been almost, if not entirely, absent from the population. The neighbourhood of Bellary, where the cholera is truly endemic, furnishes several similar instances.

It is not our intention, at present, to enter at any length into a consideration of the circumstances influencing the propagation of cholera. For so extended an inquiry, these reports, important as they are, would afford but unsatisfactory and meagre data. And we are the less anxious to enter upon a systematic view of this subject, seeing that a summary of the most important facts relating to it may be found in the reviews on cholera, in our January and February Numbers for 1848, as well as in an elaborate article in the *British and Foreign Medical Chirurgical Review* for June last. To those who wish to understand this subject in all its bearings, we would further recommend the work of Mr Parkes, and the excellent article "Cholera," in Dr Copland's Dictionary of Medicine.

We are desirous, however, of remarking in this place, on a few of the numerous passages in these reports which bear on the *questio vexata* of contagion in cholera; not only because the question is an important and interesting one in itself, but because we believe that much misapprehension exists, owing to the strong and unwarrantably exclusive statements which have gone forth upon this subject, from both contagionists and non-contagionists, as to the real nature of the evidence furnished by those who have extensively observed the disease.

Dr Copland, who, as most of our readers probably know, is a most uncompromising advocate of the doctrine of contagion in cholera, has taken great pains, in the article to which we have already alluded, to show the inaccuracy of the impression which is prevalent in this country, that the evidence of Indian practitioners is entirely opposed to the idea of contagion. Not only does he show that many of those who have had the largest opportunities of observing the disease admit, in full, its propagation by contagion; but that a still greater number who deny, in terms, this doctrine, adduce facts and opinions regarding the disease, which are strongly corroborative of contagion, in the sense in which *its evidence is understood by Dr Copland.*

On the other hand, many practitioners in India, and not a few recent authors in this country, while asserting, and even advocating strongly, the truth of many of the opinions, and accuracy of the principal facts, on which the doctrine of contagion is founded by its supporters, have drawn conclusions from these facts opposed to this doctrine. They admit, for instance, the spread of cholera among large numbers of congregated individuals, and even in some cases from person to person in close unventilated apartments; they admit the liability of a body of soldiers to receive the disease in an infected district, and to carry it, and communicate it to one which has hitherto been free; they admit also, that, in some instances, the

attendants of the sick have been attacked in a sufficiently striking proportion; but they see, in these facts, not the evidence of contagion, but of some principle of disease external to the human system, which, nevertheless, finds the conditions of its operation in the meeting and mingling of sick and healthy human beings. In other words, the same, or nearly the same facts, from which the contagionist adduces his strongest proofs, are believed to be insufficient to prove contagion in the sense in which *its evidence is understood by the non-contagionist.*

That the majority of Indian practitioners are of this last class, there is no doubt; but there is equally little doubt that the majority of them admit the existence of facts, which would in this country be considered as strong presumptive evidence of contagion, and of opinions which bear to our minds the closest resemblance to a modified admission of it. Of this there is abundant evidence in these reports. We have alluded to the spread of the disease among masses of population, and in marching regiments; a feature in which cholera precisely resembles the camp-fever of Europe. In reference to this subject, Mr Rogers himself says,—

"There remains to be mentioned one source from which cholera may be propagated—the assemblage of great bodies of people, for instance, the attendants at native festivals, and persons composing large camps. * * The disease when produced attaches itself to the mass, and is by them transported from one place to another, and in this way disseminated to persons who were previously free from it."—P. 17.

After attempting to show that "the contagion exists only in the mass," and that therefore "the disease was not contagious in the common acceptation of the term; that is, capable of being communicated from man to man, similar to other diseases possessing notoriously contagious properties," he adds:—

"It is, therefore, a fair inference that this power of transmitting the disease inherent in large bodies of men is only potent so long as the mass of the people is large, "the poison is rendered virulent by numbers," and is weakened and lost in a geometrical ratio according as the numbers are diminished, and the air around them becomes purified; so that in single cases it becomes so feeble, or diluted, as it were, that its power is at last lost. If such were not the case, we should hear of frequent instances of a single individual proceeding from an infected town or camp where the epidemic influence was rife amongst the inhabitants, to a place at a distance which was free from the disease, and thence communicating it to healthy persons."—P. 20.

The proofs of this mode of transmission are too numerous for us to quote; nor are the cases of separate transmission referred to in the last sentence at all wanting. The following case seems to have staggered Dr Henderson, staff-surgeon at Bellary, not a little. The disease broke out at Bellary in this manner:—

"It first made its appearance in the 46th native infantry, while they were on their march from Kolapoor to Madras, and moved along with them, infecting every village they passed through. On their approach they were directed to avoid this station, and precautionary measures were taken to prevent, as much

as possible, their communicating with our bazars. On the 1st of February they reached Courtney (a village about twelve miles from hence); and from what followed it would appear that the disease was brought from thence to Bellary, for during the succeeding day several cases were reported to have occurred in the neighbouring bazars among individuals who had communicated with the camp. * * The moment the disease was found to have reached Bellary, the only precautionary measures which could be pursued to prevent its gaining access to the barracks were instantly adopted. [The men were exhorted to temperance, and told to avoid the bazars in which the disease existed.] It is a singular fact that only one soldier of the corps has been attacked, although we have been surrounded by the disease for nearly two months.

"I shall not pretend to assert that cholera is of an infectious nature; still I must decidedly say we occasionally met with the disease under circumstances which rather favour the belief that it is so. In the present instance we find it breaking out in a corps on the line of march; it continues with them for several hundred miles; they are found sowing the seeds of the disease in every village they halt at, and even communicating it to others by intercourse, for instance, Bellary; in proof of which I can safely assert, that not a single case had occurred here until the 45th regiment was communicated with at Courtney, and further that the disease had not been met with at Courtney prior to the arrival of the corps at that place."—Pp. 142-144.

We could multiply instances of this kind from the reports of Dr Lorimer and others. We could also, were we desirous of straining the evidence in favour of contagion, give the tables of Assistant-Surgeon Lawrence (page 99), showing the spreading of the disease in individual families. But we are perfectly aware that such evidence when limited in quantity is open to objection, and that in a place where the disease was endemic, these circumstances might have been the result of other causes, or even of coincidence. It must also be admitted that the cases in which cholera appears to be communicated from one individual to another, without the intervention of masses, appear to be comparatively rare; from which it results either that the disease is not contagious, or that its contagion acts only in a concentrated form, and under otherwise favourable circumstances.

There is great discrepancy in the evidence as to the proportion in which the attendants on the sick are affected with the disease. "In the hospital of the Royal Regiment," says Mr Scot, "only one individual out of 101 attendants was attacked by the disease," p. 15. "In this epidemic," says surgeon J. L. Geddes, "among thirty or forty men employed about the sick, not one was seized." On the other hand, in Dr Lorimer's experience, a large proportion was seized; and, according to Mr Innes, assistant surgeon of the 27th regiment, many of the attendants were attacked, including four out of eight *writers*, whose duty it was to take charge of the property of the diseased men and to handle their clothing, three of the washermen, and several sepoys. The whole of these statements, however, are vitiated by the want of a proper numerical standard of comparison.

The argument drawn from the spreading of the disease among masses of men, is answered by the non-contagionists on the suppo-

sition, that it is the *mass* and not the *individual* which develops and gives virulence to the disease; and that large numbers of men in India, especially if ill-lodged or exhausted by marches, are placed in the worst possible situation for resisting an endemic, or any other agent of disease. It is argued, in short, that the poison does not spread by accumulation *within* the body, but by accumulation *out of* it; and involves the individuals of a multitude from their less power to resist, and their worse sanitary condition, than isolated individuals. It is also argued that the poison may be carried along with the mass to any distance, so long as susceptible individuals remain among them. So far this argument is barely tenable, though we think rather wire-drawn; but it will not explain the communication of the disease to isolated individuals, and from them to others, as appears to have happened at Bellary in the case cited, and also at other places. Besides, the *practical* question is the *communication of the disease;* and if the fact be that the disease may be carried by a multitude, and communicated by them to others in their neighbourhood, so as to cause a secondary epidemic, then we hold the question of contagion to be for all *practical purposes* settled, whether the accumulation of the poison in connexion with human bodies, which we must in this case suppose to take place, occur *without* or *within* the body;—subject to physical agencies alone, or subject to the laws of the living organism. In either case there can be no reasonable doubt that the same contagion which is developed in numbers of 50, 100, or 1000, would develop itself among two or three persons, provided there was a similar concentration of the virus, and similarly bad sanitary circumstances.

But it is held that the *evidence* of such cases of communication of cholera is insufficient. This we fully admit; we consider that the one element wanting to the proof of the contagion of cholera is a collection of facts, sufficiently large and numerically precise, bearing on the proportion of disease among the attendants of the sick, as contrasted with a similar proportion in those similarly circumstanced in all other respects, except in regard to contact with the sick. Such a return might easily be procured during the present epidemic, were care taken in the various cholera hospitals throughout the kingdom to record numerically the whole hospital staff, both of resident and non-resident officers, servants, and nurses. Till this, or something like it, is done upon a large scale, the contagiousness of cholera will remain, as now, an open question; for isolated facts of apparent contagion can always be explained as coincidences, till they are shown by large numbers and precise statements to be subject to rule. In the mean time, as the positive cannot be asserted, so neither can the negative. We have shown, even in this article, quite enough of evidence from non-contagionists themselves, to prove that the question cannot be prejudged; neither can the comparative rarity of cases of apparent contagion in cholera, as compared with typhus, be used as an argument in this case.

It is impossible for any one who considers attentively the history of cholera epidemics, however strong a contagionist, to believe that its contagious properties in any degree approach those of typhus fever, of which we have such incontestable proofs. The question is not, however, if its contagious properties are *as great* as typhus fever, but whether it has any such properties at all; and if further observation proved that those in contact with the sick were *uniformly* affected in a proportion not more than one per cent. greater than others similarly situated, we should still unhesitatingly pronounce it contagious, and measure its contagious properties by that scale. Our practical measures of prevention would of course not be regulated by the mere *name* of a contagious disease, but would be guided by expediency. Our views on this subject are so fully stated in the November Number for 1848, p. 350, that it is needless to enter upon them again.

Our readers will observe in Dr Robertson's excellent communication in the present Number, the remarkable statement that five of the nurses in the Edinburgh Cholera Hospital have been affected with cholera. We draw attention to this fact, not for the purpose of using it as an argument for contagion, but for the purpose of adverting to an explanation of it which we had occasion to hear, upon the principles, we presume, of the Board of Health. It was said that this accident was not owing to contagion, but to the crowded state of the hospital, and the consequent accumulation of an animal poison, which could not have taken effect had the number of persons been fewer. We shall merely remark that, *practically*, the fact of communication of the disease remains the same; and that, *theoretically*, a hypothesis like that stated, is directed equally against the existence of *all other* contagions which do not present themselves in the form of a specific and tangible virus; inasmuch as they all require a certain degree of concentration and crowding to ensure their action on a healthy individual.

We have only one more remark to make upon this subject, before we pass to the treatment of cholera. It is, that the contagion of cholera is a question to be solved by facts, independently of all other questions in relation to this disease; and in particular, that the fact of its transmission *by the human body*, if proved, would not involve the negation of all other modes of transmission. The difficulties in the way of believing in the diffusion of cholera over so wide a territory by means of human effluvia *alone*, and the positive objections which exist to such a theory, have at all times been a great barrier to the belief in its contagious properties *under any circumstances*. Could we take this view of the case, we should unquestionably admit it as a most serious objection to the contagion of cholera. The discussion of this subject would, however, carry us beyond our limits; and we are happy, therefore, to be able to refer to the lectures of Dr Watson for the arguments in favour of the view of the subject we here suggest. We hope the present epi-

demic will be fruitful in information as to the mode of transmission of cholera.

In considering the treatment of cholera, as illustrated by recent documents, we propose to avail ourselves chiefly of the data furnished by the practice of Dr Robertson, and the statistical inquiries of Mr George Ross. The former has made good use of by far the largest and best field of experience which has yet been presented by the present epidemic—the Cholera Hospital of Edinburgh. The results of Dr Robertson's treatment are before the public in more senses than one; for not only are the published records of them to be found in our pages, but the cases on which these statements were founded, and the whole details of their management, have been the subject of consideration and criticism by hundreds of medical men and students, who have been daily visitors to the hospital. We have heard but one opinion of the careful consideration and judgment displayed in these trials of remedies; and we do not believe, for our own part, that the difficult and slippery track, which, between the meteors of dogmatism and empiricism, leads towards a true practical result, could have been more steadily pursued than it has been by Dr Robertson.

Mr Ross's lectures, on the other hand, may be looked upon as the epitome—and an excellent one—of the *aggregate* of our *past* experience in regard to cholera. In this respect they stand aloof from all the publications, whether good, bad, or indifferent, which have issued from the English press during the present epidemic. The author has evidently begun in scepticism, and with the determination not to form or express an opinion upon imperfect evidence, but to accumulate facts in the most positive and numerical form possible, with the view of being able to warrant something like certainty of judgment. Whether he has succeeded in procuring evidence satisfactory to his own mind or not, he has accumulated a large body of data for the guidance of opinion; subject, no doubt, to many sources of fallacy, but which may be substituted, with much advantage, for the loose theories and generally partial statements of individuals, whose experience of the disease is small or imperfectly recorded. We hope Mr Ross will elaborate these results still further; we consider them as most important and valuable contributions to the history of treatment in this disease.

We laid before our readers in the last (December 1848) Number of our *Retrospect*, a summary of the Indian treatment of cholera, as contained in an appendix to the reports of Mr Rogers; but this summary must be considered as being, in a great measure, the result of loose individual impressions, and is certainly very imperfectly borne out by statements to which any degree of *positive* value can be attached. We consider it, therefore, as in this respect very much on a level with the numerous other publications of the same kind which are every day given to the world as the result of ex-

perience, but which in reality are so framed as to give prejudice and fancy abundant scope for the perturbation of the results.

Let us not, however, be misunderstood. It must not be supposed either, that we attach *no* importance to individual impressions on this subject, when borne out by extended experience; or that we suppose the eclectic and numerical method of Mr Ross free from the chances of being vitiated in its results by personal or collective prejudice. So far is this from being the case, that we are distinctly of opinion that in almost all numerical statements which have yet been published upon the mortality of this disease, the *relative* number of deaths to cases has been influenced much more by the *diagnosis* of the latter, than by the absolute number of the former. The extent of scepticism, which is sometimes necessary on this subject, is evident from the exceedingly different and contradictory accounts which are every day brought under the notice of the medical man, showing that the same remedy has been largely used by different practitioners, apparently in exactly the same circumstances and with exactly opposite results. We were told the other day by a gentleman (non-medical, however), who had seen cholera several times in India, that at a place where no medical man was accessible, he had taken the treatment upon himself, and had succeeded in passing above 100 cases through his hands without a single death!—at a time, too, when a neighbouring village was decimated by the disease. The treatment was—brandy and opium. We at once suggested to our friend that the cases were not all epidemic cholera;—and further, that in all probability the *number of them*, rather than the absence of deaths, was affected by the circumstance of a liberal allowance of brandy forming part of the treatment. That errors similar to this enter to a very large extent into *medical* records on this subject, we firmly believe; and, although they may be in part removed by the careful selection of trustworthy data, and in part merged in the accumulation of large numbers, as in Mr Ross's results, we are, nevertheless, convinced that no amount of care will prevent such variations in the essential data, as to render numerical statements *alone* a most unsafe guide in the treatment of cholera, or indeed of any other disease. We believe them to be a far *better* guide than fluctuating individual opinion, but this is all.

It is evident, therefore, that for a long time to come, the treatment of cholera cannot be fixed by an examination of records. Till it is so fixed, the practice in individual hands must be regulated in a considerable degree by theoretical or rational considerations; or rather by that peculiar combination of rational and empirical judgment which passes in the world under the name of *common sense;* the tribunal to which so much, besides cholera, affecting the interests of individuals and of the community, is in these days referred. Hence the importance to practical men of just ideas of the pathology of cholera, and minute knowledge of its phenomena; by which, and by

which alone, they can estimate the success of their own treatment, or that of others, in individual cases. We regret to find that the contributions to this department of our knowledge are so greatly overbalanced, as to weigh but like dust in the scale against the enormous mass of new suggestions in treatment, and crude theories, electrical, chemical, animalcular, fungous, &c., upon the nature and origin of cholera. Those who wish to learn upon this subject, cannot do better than go back to Cruveilhier.

In the mean time we shall allude shortly to a few of those remedies which have acquired, from longer or shorter experience of them, somewhat of the reputation of specifics in cholera; and conclude by inquiring if there be not a few principles of treatment so decidedly borne out by theory and experience, as to be fairly assumed as the basis of future inquiries.

The first and most controverted of cholera remedies to which we shall allude, is *venesection*. In reference to this, the evidence is of the most varied character. It has been employed with alleged benefit in all stages of the disease, and not less in this country than in India. The most satisfactory accounts are of its use in the early stage, before the collapse has occurred; and here it seems to be often most effectual in relieving the feeling of tightness and oppression about the stomach and region of the diaphragm, which are frequently most distressing to the patient. As to the effect on the mortality, it is difficult to form an opinion. It is usually only in the early stage that blood can be procured in quantity, and this is precisely the stage not only when mistakes of diagnosis are most apt to occur, but in which the disease is most manageable under any form of treatment. Notwithstanding this circumstance, the mortality where blood-letting formed a considerable part of the treatment, is rarely much less in any of Mr Ross's tables than 50 per cent. and sometimes more; few of these results, however, relate to bloodletting alone. Dr Robertson, whose observations on this subject in our present Number are well worthy of attention, thinks that he has in several instances prevented the collapse by this measure; and the favourable result of his cases presents the greatest encouragement to the practice. He employs it, however, only in the early stage.

Stimulants, especially alcoholic liquors, have been lauded in cholera no less than bloodletting; but there appear to be now grave reasons for doubting their efficacy, and even, we think, of rejecting them in a great measure in the treatment of this disease. Not only are they in many cases most disagreeable to the patient, whose perpetual thirst they do nothing to relieve; but it seems to be most probable that they are not absorbed, and that their action is therefore purely local. It is important also to observe, that the evidence against them in Mr Ross's tables is most unequivocal, and that, both alone and in combination with other kinds of practice, they seem invariably to have deteriorated the results wherever they were used. There

is not an instance in these tables of a mortality under 50 per cent. where stimulants have formed part of the treatment. That by stimulants alone, gives in the aggregate 58·8 per cent.

Opium has a very large amount of individual testimony in its favour, and is indeed apparently so directly indicated by the diarrhœa, that we cannot wonder at its having been extensively used. In conjunction with acetate of lead, it forms the celebrated pill of Dr Graves, which has had a most extensive reputation in the cholera both of this country and India. There seems no reason to doubt, that, in the premonitory diarrhœa, this remedy has the power ascribed to it; but, in the confirmed disease, Mr Ross's tables show that it has not diminished the aggregate mortality below 50 per cent. Those who continue to employ it should certainly do so in the fluid form, in order not to oppose any unnecessary obstacle to its absorption. In the stage of reaction, or where there is a tendency to coma, as is often the case in this country, there is every reason to reject opium as probably injurious.

Mercury, in the form of calomel, and usually in combination with opium, has been widely recommended in India. In this country it has been used to a large extent, but without remarkable success according to the returns, excepting in the hands of Dr Ayre of Leeds, and Dr Peacock, in whose cases the mortality was as low as 31 per cent, and who both used it *without stimulants*. Whether this success is due to the calomel, or to the simplicity of the treatment in other respects, is we think very doubtful, considering the negative results of mercury in other hands, and combined with other modes of treatment. If, however, mercury be employed, we think that the method recommended in our Medical News by Dr Fleming, deserves attention; that of administering it in the fluid form of corrosive sublimate. Absorption is known to take place with great difficulty in cholera; and it is difficult, therefore, to conceive a worse form for conveying mercury into the system than that of calomel.

Tartar Emetic is strongly recommended by Dr Billing,[1] on the ground that cholera is a fever, and must be treated by sedatives and fever medicines. He considers the collapse of cholera to be similar to the cold stage of ague, and strongly reprobates stimulants in every form. He allows cold water to be liberally given, and even pushes his theory of the disease so far, as to administer quinine from the beginning. The tartar emetic is given in small doses. Dry friction is the only external application. In Droitwich Lunatic Asylum, where tartar emetic was the staple of the treatment, there were only four deaths in twenty-one cases; but this number is too small to afford any thing more than a presumption in favour of the remedy.

[1] On the Treatment of Asiatic Cholera. By Archibald Billing, M.D., &c. London: 1848. P. 15.

We attach, however, more importance to Dr Billing's testimony than to his theory, and consider this method as worthy of further trial.

Injection of the veins was first introduced by Dr Mackintosh of Edinburgh; it has been so frequently tried in cholera, and so frequently found wanting, that, notwithstanding its extraordinary effects in the first instance, we should hesitate to recommend it in any case which presented a hope of recovery under any other treatment. Nevertheless, the high mortality ascribed to it in Mr Ross's tables (85·7 per cent.) is evidently to be ascribed to the trials of it having mostly been made upon moribund cases, in which alone it is by many practitioners thought justifiable. We refer our readers to Dr Robertson's remarks on this subject, in our December Number, as well as in the present one. Nevertheless, we think that, if this treatment is to have any chance of success, it must be by being tried earlier in the disease, and *repeated as often as the collapse recurs.*

Emetics and strong *Purgatives* (such as croton oil) have each had their supporters; but neither from theory nor experience can we gather much satisfactory testimony in their favour.

Chloroform inhalation has been used in thirty-seven cases in Peckham-house Asylum, all of which presented characteristic symptoms.—(See Med. Gazette, Nov. 24, 1848, p. 903). The number of cases is too small to enable us to form a decided opinion upon the practice, more especially as the details of symptoms and treatment are not given. In the mean time, the results are superior to most of the methods in Mr Ross's tables, but inferior to the tartar emetic practice in Droitwich Asylum, before referred to, and very decidedly inferior to the results of upwards of 700 cases treated by cold water and saline medicines internally. In the Edinburgh Hospital, chloroform was found to relieve the cramps while the patient was under its action; but with respect to the restoration of temperature, and amendment of the symptoms of collapse, it was believed to exert a negative, if indeed not an unfavourable, influence.

With regard to several other remedies which have been faithfully tried in Edinburgh, we must again refer to Dr Robertson's paper.

On the whole, we think it is clear enough that the specific for cholera remains yet to be discovered; and that none of the more vaunted cholera remedies present evidence in their favour so strong as to command an exclusive attention. On the other hand, it is much to be feared that the *routine heroic* practice of many practitioners, both in this country and in India, has aggravated to no small extent the mortality of this tremendous disease. This is especially the case, as we have already pointed out, with regard to stimulants.

On reviewing the evidence deduced from large numbers, we find that there are two modes of treatment which present so marked an

advantage in respect of mortality, as to arrest our attention very forcibly. The treatment by common salt, with cold water given in abundance internally, produced in 607 cases (in various hands) a mortality of 20 per cent.; and in Greville Street hospital, 107 cases treated by a saline mixture, with copious draughts of cold water, gave a mortality of only 14 per cent., the lowest which has yet been recorded from a similar number of unselected cases. Nearest to these stands the treatment by ice alone, given by the mouth; a continental practice, of which the results are 30 per cent. mortality. It is a remarkable fact, as pointed out by Mr Ross, that in all these the administration of cold water by the mouth plays a prominent part; and when we consider the success which this remedy alone appears to have had in the hands of many practitioners (especially Dr Shute of Gloucester), it is impossible not to think favourably of it in cholera. "Dr Shute states, that under this system the state of collapse is prolonged to two, three, or five days; and others have remarked, that during the reaction a paroxysm of raging delirium is apt to take place. It is not, therefore, an inoperative remedy." Add to this, that it is most grateful to the patient, whose burning thirst seems always to point to this as the most appropriate resource for his relief. If it be the case also, as seems every way probable, that the water so administered is either absorbed into the blood to replace the fluid lost, or tends to prevent the loss of fluid from the blood by the intestines, we can have no difficulty in understanding its beneficial effects.

We are most firmly persuaded, that cholera, like all other diseases dependent on a specific poison, has a spontaneous tendency to cure after the virus has exhausted itself; and that the treatment will be most securely and successfully accomplished by discarding, in the majority of cases, heroic remedies; by following out the indications afforded by the feelings and desires of the patient; and, as Cullen said of fever, by attending to those conditions and means calculated to "obviate the tendency to death." Now, all that we yet know of the pathology of this disease tends to ascribe the fatal result in the collapse to a slow asphyxia, induced by the imperfect fluidity of the blood. In proof of this assertion, we would refer to pathological facts (see our review on this subject in February 1848), as well as to the wonderful, though too transitory, effect of the injection into the veins. We would therefore endeavour by every means to supply fluid to the blood through the intestines, the skin, the lungs, or at least to prevent, in as far as possible, the fluids of the body from being thrown off by these channels.

This might be accomplished—1*st*, by supplying cold water in abundance by the mouth, as already mentioned, and as dictated by the thirst of the patient; 2*d*, by the use of baths of regulated temperature, at least at the *beginning* of the treatment; 3*d*, by maintaining the body of the patient *throughout* the treatment in contact with fluid media, or at least with fluid vapour, by means of soaked cloths placed around him, and covered by a sufficiency of blankets; 4*th*, by

surcharging the air of the apartment with vapour, particularly when the external air is dry or frosty.[1]

The third of these indications was put in force by Dr Robertson by means of the hot wet sheet, surrounded by several dry blankets, very soon after the commencement of treatment in the Cholera Hospital; and the advantages of it over the use of heated air, as in the first cases in the Infirmary, was soon apparent. The mortality under the latter practice, indeed, was so considerable as to cause it at once to be renounced in the Cholera Hospital. It was found, however (see p. 396 of the present Number), to have some disadvantages in the case of adults, from the disagreeable sensations sometimes caused by it, which gave rise to restlessness and struggling, and consequent exposure to the air. We are disposed to ascribe these effects in part to its having been used very warm, and exclusively with the view of producing reaction by artificial heat, and think that many of these inconveniences would vanish if the temperature were studiously regulated by the feelings of the patient.

The regulation of temperature is, indeed, a most important mean in the treatment of cholera. The extremes both of heat and cold appear to be ill borne. The momentary shock of the cold affusion (followed by warm wrappings) has indeed been useful in rousing patients from deep collapse; but nothing has shown its continued application to be beneficial. On the other hand, the exhausting influence of excessive heat, externally applied, has been noticed by many writers. The supervention of reaction appears to us to depend much more upon the reception of fluid into the blood than upon the application of external heat; and we have seen it take place, and follow its usual course, where no external heat has been applied. Should the relation of the reaction to the fluidity of the blood be established, it will appear still further how paramount is the indication of treatment by fluids, to which we have alluded. Meantime a moderate amount of heat, such as is agreeable to the patient, appears to us to have most evidence in its favour in the treatment of the collapse.

The relief of the spasms is the next important indication of treatment during the collapse. In relation to this symptom, further trials of chloroform appear to be requisite, unless it shall be found to affect unfavourably the progress of the case. The use of frictions to the affected parts, and of stimulation of the skin by liniments, or by mustard cataplasms, is universally admitted to be useful.

Finally, in the management of the reaction the most important

[1] We are happy to find a corroboration of these views, as to the importance of fluids in the treatment of cholera, in an able pamphlet by Dr Buchanan of Glasgow, which deserves special notice as one of the few recent contributions to the pathology and general history of the disease, bearing the traces of independent thought and observation, without being the mere exposition of a hypothesis.—(See *Observations on Malignant Cholera*, &c. By Andrew Buchanan, M.D., Professor of the Institutes of Medicine in the University of Glasgow.)

indication appears to be the restoration of the urine, and of its normal constituents, especially the urea and uric acid, which are often deficient, and appear by their deficiency to lead to coma. An excellent suggestion of Dr Robertson's in these cases is the administration of *colchicum;* and, we believe, this has been carried out by him in a few instances with good effect; though, of course, nothing can be yet said decidedly on this remedy.

We here conclude these remarks, being sensible of having devoted a larger space to this subject than we originally intended; but we hope that its intrinsic importance will excuse us in the eyes of our readers for having occupied their attention longer than is usual with us in a single review. W. T. G.

Chemical Reports and Memoirs. Edited by THOMAS GRAHAM, V.P.R.S., &c. London: Printed for the Cavendish Society.

THE Cavendish Society is an association lately formed for the publication of chemical works, on the plan which has been adopted by the Royal and other societies. The prospectus of the Society was published nearly two years ago, and at that time we called the attention of our readers to its merits and advantages. Various circumstances, however, appear to have delayed the actual commencement of the Society's operations to the present year; but we have now the satisfaction of recording the appearance of its first publication, and the consequent establishment of a society which is likely to be of great use, at a time when the public mind in this country is becoming awakened to the importance of the study of chemistry and its rapidly extending applications to medicine and the arts.

The work before us consists of a selection of detached memoirs on different chemical subjects, which appear to have been selected in part for the purpose of showing the extent of subjects embraced by the Society's operations. We have reports by Professor Otto of Brunswick, upon atomic volumes, and on isomorphism; a memoir on atomic volumes, by M. Filhol; a report on endosmose, by Professor Julius Vogel; a memoir, by M. Chevreul, on the simultaneous contrast of colours in dyeing; on the latent heat of steam, by M. Regnault; on the artificial formation of alkaloids, by M. Kop; and on the pseudo-volcanic phenomena of Iceland, by Professor Bunsen of Marburg.

It is manifestly impossible for us to attempt any analysis of these papers, many of which are incapable of abridgment; we shall content ourselves with stating, that their selection is judicious, that the translations are well executed, and the volume handsomely got up. We have only one fault to find, and that is the imperfect manner in which the press has been corrected. There are a number of errors both in the symbols and numbers, which, we trust, will be avoided in the future publications of the Society.

We find, by the report of the Society, that they have commenced the translation of Gmelin's Handbuch der Chemie, and that various other works will soon be taken up. It is of course obvious, that the Society's operations must be limited by the number of subscribers; and we strongly recommend all those who are interested in chemistry, to lose no time in becoming members of this most valuable Society.

Part Third.

MEDICAL NEWS.

CORONER'S INQUEST IN AN ALLEGED CASE OF ASIATIC CHOLERA—UNWARRANTABLE OPINION OF TWO MEDICAL MEN AS TO THE EFFECT OF THE TREATMENT PURSUED BY A BROTHER PRACTITIONER.

THOMAS RUSSELL, an inhabitant of Stanwix, Carlisle, after having been in Scotland at the Haugh of Urr in Galloway, thirteen miles from Dumfries, for ten or twelve days, returned to Carlisle on the 23d November, and next morning was attacked with diarrhœa. The purging continued during that and the four following days, and for the same period nothing would stay on his stomach. The vomiting does not appear to have been very severe, or at least did not draw much attention. The stools were watery throughout, free from blood. He had much thirst from the first. He complained of pains and stiffness, and latterly of a crampy feeling in the legs. Such is the imperfect view of the symptoms during the five first days of his illness to be gathered from the evidence of his sister at the inquest. At one o'clock in the morning of the sixth day, a medical practitioner, Dr Evans, was called to him for the first time. When Dr Evans arrived between one and two o'clock in the morning, he learned that he had been purging for several days; and that, for the last twenty-four hours, there had been frequent attacks of vomiting; the features were contracted and elongated; the face and forehead were extremely cold, and small drops of cold sweat stood on the latter; the upper and lower extremities were very cold; the chest and abdomen warmer. The abdomen was sunk and contracted. The colour of the surface was natural, but there was a marked deep blue colour of the larger veins. The patient complained of cramps in his legs, extending, as he said, "Up his thigh into the bowels." He had intense thirst, and expressed a great desire for cold water. He had no pain of the chest or head, and his breathing was then free. The pulse was small, quick, and contracted. He felt easy except when the cramps occurred, which he bore with much fortitude. Soon after Dr Evans arrived, he was sharply purged; the evacuation being very watery. Dr Evans did not leave him for several hours, remaining in the house till between five and six o'clock in the morning. About an hour before he went away, he examined the patient again carefully. The surface of the body, from the face to the lower extremities, was remarkably cold, communicating quite a chilly sensation to the hand, while the peculiar appearance of the countenance before noticed, had become more striking. Around the eyelids and the mouth there was a deep blue or livid colour. The lower extremities showed a considerable degree of lividity, and also the fingers; the hands and the nails in particular were very livid. He still desired cold drink, and distinctly asked for water. Dr Evans did not return till past eleven o'clock. During his absence he lay quiet; when spoken to, he muttered samething indistinctly, opening his eyes, but giving little attention. When the doctor returned, he could no longer speak; but, by the sister's account, he opened his eyes, and moved his body a little when his name was pronounced, but soon closed them again; this was but a short time before his death. The pulse at the wrist was imperceptible; the breathing was quick, as it had become before Dr Evans left him early in the morning. The lividness of the

surface was much increased. A short time before death, the fingers became bent and contracted. The doctor was present up to the time of his death, which must have taken place soon after mid-day, or in about twelve hours after the doctor was sent for.

The treatment employed consisted of occasional doses of laudanum, brandy, and water, a vapour bath, and the repeated application of sinapisms to the abdomen and lower extremities.

Owing to the excitement created by the report that a case of Asiatic cholera had occurred, a coroner's inquest was held. The principal witnesses were the sister of the deceased, and Dr Evans himself, from whose joint testimony the above particulars have been collected. Dr Evans considered the case to be one of Asiatic cholera. No distinct report of the post-mortem appearance is given in the account before us; but the following particulars are gathered from the evidence of Drs Elliott and Knott, who were appointed by the coroner to make the investigation. The body was not examined till three days after death. It was in tolerably good condition as to flesh, though the deceased was stated to be considerably thinner than some time before. Decomposition had in some degree commenced. There was greenness of the abdomen and incipient putrefaction. The countenance was placid; the eyeballs shrunk; the pupils of natural size; the toes not at all contracted; the fingers slightly flexed; the surface of the body of a dark colour. The bladder was contracted and empty; the surface of the serous membranes was moist.

Drs Knott and Elliott gave a report on the case, and then a desultory discussion arose between the medical men concerned as to the evidence of the case being a case of cholera. The coroner summoned up, and the jury returned a verdict, "That the death of Thomas Russell was occasioned by a severe attack of English cholera."

The following is the report presented to the coroner by Drs Knott and Elliot:—

"*Report of Drs Knott and Elliott.*—We, the undersigned, have attentively heard the evidence given by the several witnesses touching the general health, the last illness, and the death of Thomas Russell, aged 28 years, brickmaker: we have also carefully examined the body of the deceased, both externally and throughout; and respecting the question of death having resulted in this case from malignant or Asiatic cholera, we are distinctly of opinion that the deceased Thomas Russell did not labour under this disease.

"From the ample and satisfactory data just alluded to, our opinion is that the deceased came by his death through *a concurrence of circumstances*—not one of which simply, we think, would have produced a fatal result.

"The *concurrence of circumstances* to which we refer are as follow:—His general habits of intemperance, and especially his intemperance immediately preceding his fatal illness; the severe attack of English cholera under which he laboured for some days before his death; and the unquestionable proofs of serious bodily disease met with by us in the *post-mortem* examination, viz., partial softening, clearly of old standing, at the back part of the interior of the left half of the brain (known as the left lateral ventricle)—firm and very extensive adhesion of both lungs to the ribs, especially of the left lung—dilatation and flaccidity of the heart—and lastly, a dose or doses of laudanum in all above two drachms, as stated in evidence, which under any circumstances we would call a large quantity; but which is not larger than is, in peculiar cases, beneficially exhibited by medical practitioners, and falls far short of what has often been swallowed with impunity in cases of attempted suicide, but by which the exhausted state of Russell, and the extensive inroads of disease upon his brain, his heart, and lungs, rendered him peculiarly liable to be affected.

"In conclusion, whilst we cannot omit allusion to the exhibition of laudanum in this case, as tending, with various other and natural causes, to promote death, we yet beg to state our opinion to be, that much stress cannot be laid on the

exhibition of laudanum as stated—the peculiar circumstances we urgently request attention to. We must, however, express our regret that the responsibility of so administering laudanum under all the peculiarities of the case, has exclusively rested with the gentleman in attendance upon Russell at the time of his death.

"SAMUEL KNOTT, M.D.
"December 5, 1848. "ROBERT ELLIOTT, M.D."

This report needs no commentary. Our readers do not require to read it twice. At a single glance it may be comprehended—words would fail us to characterise it. We feel assured the profession will not ratify it. It sets all medical authority at defiance, and yet it applies to the reputation of a brother practitioner.

What are the circumstances? A man, of twenty-eight years of age, previously strong and vigorous, addicted somewhat to the use of ardent spirits, affected for five days already with purging, vomiting, and pains of the extremities, becomes at last so ill as to raise a tardy alarm in his family, who send for a medical man in the middle of the night. This medical man considers him in so great danger, that he remains beside him throughout the night employing such remedies, as his medical skill suggests, and the means within his reach at the time afford. Among other remedies he administers to him three doses of laudanum, in all about eighty drops, spread over between four and five hours. At the time when the first dose is administered, the man is still suffering from vomiting and purging; and it is on account of the continuance of the cramps that the other two doses are given. No loss of sensibility ensues, the man gradually sinks, being conscious, if we interpret the evidence aright, of his name being pronounced within half an hour or an hour of his death; and this event occurs at least ten hours after he took the first dose. There is not the shadow of evidence that the laudanum had any share in the fatal event. Call the quantity more than two drachms as stated in the report, though the evidence does not bear out this quantity; call the case English cholera, though we are not satisfied that it was not Asiatic cholera, and still we affirm there is no room for censure. Where shall we find a medical man, of a few years' standing in the profession, who has not administered, even to a delicate female, two teaspoonfuls of laudanum in as many hours? Is it in English cholera that opium is used with a sparing hand? Is it under cramps that this drug must be employed in the minimum dose? Does a man become more susceptible to the effects of another narcotic by having before accustomed himself to the narcotic influence of ardent spirits? Is it when the surface is cold, and the whole system passing into a state of collapse, that a dose of opium produces its maximum effect? But it was the concurrence of circumstances, the Report says, that destroyed the patient: which signifies that Dr Evans, not knowing the acute susceptibility to the influence of opium given by a firm and very extensive adhesion of both lungs to the ribs, was guilty of manslaughter. As to the alleged softening of the brain, we have not lost sight of the fact, that the inspection was not made till three days after death; and as to the flaccidity of the heart, that might well be present after six days' exhaustion by vomiting and purging. But let it be admitted that neither the brain nor the heart were free from chronic disease, the answer is, that the man was well and strong just before he was attacked by cholera, and that no part of the symptoms detailed can be referred to these organs. What Dr Evans had to treat was a case of cholera, English or Asiatic, in a state of collapse; and we affirm that he dealt with his task in a manner every way unexceptionable. Say the case was English cholera, what was surprising in the patient's death, after so many days of neglect, from which his friends were only roused when the death-struggle commenced, and then they sent for the doctor? But with what confidence can any man say that this was not a case of Asiatic cholera? It was not, indeed, a well-marked case; but those who happen unfortunately to be in a place where it prevails, know well that Asiatic cholera is not bound down to one fixed and invariable form any more than other diseases. All the

essential symptoms of Asiatic cholera were present — vomiting, purging, cramps, suppression of urine, collapse. The man had been living for ten or twelve days but a few miles from Dumfries, where the disease is raging with such fearful mortality; he had even been for some hours in the town of Dumfries the day before he was taken ill, and that after the time when cholera had set in there.

We think it worth while to quote what Dr Evans said in evidence, to support his idea that the disease was Asiatic cholera :—

"Dr Evans then described the appearance of the body, from which he concluded that cholera was the cause of death, viz., the absence of any disease which was in the least calculated to account for any of the prominent symptoms of his case. The distension of the gall-bladder with bile ; the considerable amount of fluid in the small and large intestines, which we three agreed was about a pint and a half ; the absence of fæces in the intestines ; the contracted and empty state of the bladder; the livid appearance of the body which existed after death, though it is frequently absent after death, notwithstanding it may be seen before death, according to the opinion and experience of medical men of cholera when it visited this country before; the great engorgement of the lungs, and the presence of fluid in the several cavities of the body, the chest,the lungs, the surface of the brain, and the surface of the pericardium; the entire absence of any marks of inflammation in the stomach and bowels, and also the remarkably sunken condition of the eyes, particularly one, the left, which corresponded with the side of the body that he complained of the cramp extending from the left thigh into the abdomen. I would also, in conclusion, just state that, from the observation of medical men who have had great experience in cholera, it has been found that those who have been addicted to drinking, whether they lived well, were clad well or not, were very bad subjects of cholera, the disease almost invariably terminating fatally with them."

Whether Dr Evans be right or not in pronouncing this a case of Asiatic cholera, may be a matter for question ; but there can be no question of his reasoning on the case being much more just, and in conformity with proper views of medical evidence, than those of his opponents—we had almost said of his persecutors. Dr Knott trusts to the absence of dryness in the serous membranes, in the *post mortem* examination, as a proof that the attack was not one of Asiatic cholera. Is Dr Knott sure that the dryness he refers to (which is sometimes well marked) continues so long as three days after death ? The post mortem examination is commonly early after cholera death. This dryness, at all events, is far from invariable. Effusion into the cerebral ventricles and the pericardium are not unfrequent.

We think the evidence of this case being Asiatic cholera strong ; but we do not quarrel with Drs Knott and Elliot on this point. They are entitled to hold their opinion that this was not a case of Asiatic cholera ; but they are not entitled, on their supposition, to account for death, by taking up gratuitously the suggestion of careless ignorance, like that of this man's family. It is plain from the evidence where the first suspicion of the effect of the laudanum originated. It is the old cry of poisoning by the doctor, when a disease runs its fatal course more quickly than is familiar to vulgar experience ; and God help the medical man who falls under this cry, when his brethren fail to stand his friends. Dr Evans is a stranger in Carlisle, a young man, settled there but a few months. We know nothing of him. We have no reason to distrust his worthiness ; but, were he less worthy, we should not alter one word of what we have said. He is entitled, in any event, to justice.

MEDICO-CHIRURGICAL SOCIETY.

The following gentlemen have been elected Office-Bearers for the ensuing year :—*President*, James Syme, Esq., Professor of Clinical Surgery in the University of Edinburgh. *Vice-Presidents*, Sir W. Newbigging ; John Good-

sir, Esq.; Dr Begbie. *Councillors*, John Coldstream, M.D.; James Dunsmure, M.D.; Richard Mackenzie, M.D.; Benjamin Bell, Esq.; Halliday Douglas, M.D.; Alexander Wood, M.D.; James Robertson, M.D.; William Cumming, M.D. *Treasurer*, Robert Omond, M.D. *Secretaries*, John Taylor, M.D.; William Robertson, M.D.

DEATH OF PROFESSOR SAMUEL COOPER, F.R.S.

IT is with regret that we announce the death of Professor Samuel Cooper—a man whose name has been well known to the profession for more than thirty years, and who deservedly enjoyed a high reputation, not only as a surgeon, but as a most successful contributor to medical literature.

Mr Cooper received his professional education at St Bartholomew's Hospital nearly fifty years ago. He was apprenticed to Mr Ransden, surgeon to that institution; and he received his diploma as a member of the Royal College of Surgeons in the year 1803. After having occupied himself for a short time in practice, he entered the medical department of the army, and in this, from his superior professional attainments, he very soon rose to the rank of staff-surgeon. In this capacity he served in Holland and in the Mediterranean. He was also in active service in the Netherlands after the battle of Waterloo.

Mr Cooper formerly held the office of surgeon to the Fleet and Queen's prisons. He is, however, better known to the profession, by his having held for the period of seventeen years the office of surgeon to University College Hospital, as well as the professorship of surgery in University College. Our readers are aware that he resigned these offices in April last. As a teacher, he was, we are informed, much beloved by his pupils; but ill health had compelled him of late years to resign a portion of the duties attached to the professorship of surgery.

The deceased, it has been stated, was admitted a member of the Royal College of Surgeons so long ago as October 7, 1803. In 1807, he obtained the Jacksonian prize, for an elaborate treatise on the joints; and on the 26th May 1827, he was elected a member of the Council of the Royal College of Surgeons. In 1832, on the anniversary of the birth of Hunter, he had the honour of delivering the annual oration, in memory of the founder of the Hunterian Museum. In 1835 he was appointed an examiner; and in 1845 he was elected to the highest honour he could obtain—viz. the office of President of the Royal College of Surgeons. He was also a Fellow of the Royal Society.

The works which have conferred on Mr Cooper a high reputation, and have made him a universal teacher of the present race of practitioners, are his "Surgical Dictionary, and his "First Lines of Surgery." The former work has been most widely circulated: it is, we need hardly say, a monument of industry and accurate research, which would alone suffice to confer upon the author a lasting reputation in medical literature. It has passed through many editions, has been translated into several languages, and is diffused as a standard work on surgery through all countries.

Mr Cooper died on the 2d instant, at his house at Shepperton, Middlesex. He was in the 68th year of his age. In him the profession may be considered to have lost a useful and worthy member.

DEATH OF DR CLENDINNING, F.R.S.

WE have also the painful duty to record the loss the profession has sustained in Dr Clendinning.

He was educated at Trinity College, Dublin, where he obtained the degrees of M.A. and M.D.; from thence he proceeded to Edinburgh, and graduated as M.D. He then made long sojourns at Göttingen and other continental Universities. On his return he obtained his *ad eundem* at Oxford.

On settling in London, he became physician of the Western Dispensary, and afterwards of the St Mary-le-bone Infirmary, which he held to the time of his death.

He had passed through various offices in the College of Physicians. Last year he was senior censor. He also served in several offices in the Royal Medical and Chirurgical Society. Under an ungainly exterior, and not very prepossessing

manners, he possessed a manly independence of character, great power of intellect, and an unswerving honesty of purpose, which secured the respect of all who knew him.

He died on Tuesday, December 5th, of malignant disease of the abdomen, under which he had lingered for several months.—*Med. Gazette*, December 1848.

CHOLERA IN BERLIN AND SMYRNA.

In *Berlin* the cholera commenced its ravages on the 28th of July, and continued till the 18th of November, when the last case occurred. Of a population of 400,000 inhabitants, Berlin has had, during this period, 2405 cases, of which, on the 22d November, 1573 were dead, 700 were cured, and 72 remained under treatment. Hamburg, which contains only one-third the population of Berlin, has had 1600 deaths.

During the late epidemic in *Smyrna* 2700 was attacked, and 1700 died. The mortality is thus divided among the different classes of the town.

Mussulmans,	1200 cases,	800 deaths.	
Jews, ...	580 ...	250	—
Greeks, ...	560 ...	450	—
Armenians,	100 ...	50	—
Catholics,	260 ...	150	—
	2700	1700	

TREATMENT OF CHOLERA IN LIBERTON PARISH (NEAR EDINBURGH).

In the *parish of Liberton* (exclusive of Niddry Mill), there have been in all 70 cases, of which 29 have died—53 of these cases occurred in the village of Gilmerton alone, and of these 19 died. Dr Fleming, who had the medical charge of the latter place, has found the following medicine useful in arresting quickly and effectually the vomiting and purging. To the success of the treatment in this respect, he attributes in part the small mortality.

Corrosive sublimate, 3 grains; muriate of ammonia, 30 grains; water, 3 ounces; and to this is added, solution of morphia, 1 ounce. Of this mixture, two or three teaspoonfuls are given at first, and one teaspoonful every four hours afterwards, until both vomiting and purging cease. The muriate of ammonia is added to cover the acridity of the sublimate. The efficacy of this treatment arises simply from the drugs being in a dissolved and *easily absorbable* form. The addition of syrups and mucilages, which obstruct absorption, is avoided.

MR SPENCER WELLS ON VARICOCELE.

To the Editors of the Monthly Journal of Medical Science.

Gentlemen,—In the November Number of your Journal, Dr L. R. Thomson has published a very able and interesting paper on the treatment of Varicocele by pressure. He introduces, however, an extract from a letter written by Mr Curling last August, which is the subject of my present communication. Dr Thomson says, Mr Curling, "as far as I am aware, is the only one who has published cases of varicocele cured by this plan;" and Mr Curling writes, "Nor am I aware of any one else having written on this subject." Now, sir, in the year 1845 I wrote an analysis of Dieffenbach's Operative Surgery, which was published in April 1846 in the *British and Foreign Medical Review;* and in commenting on the views of the great German surgeon with regard to the treatment of varix, I made the following remarks, which will be found in the 21st volume of the Review, p. 289 :—

"We believe, in stating the result of our observation of the result of simple compression in the treatment of varicocele, we impart a practical fact of great value to our readers. The unsatisfactory results of all the known methods of

treating this disease, with the exception of the ligature of the veins, which, though often successful, is often followed by dangerous phlebitis, led us to reflect on the effects of the ligature, and the means of safely fulfilling the same indications. What is the effect of the ligature when successfully applied? Simply obliteration of the vein, preventing the gravitation and pressure of the column of blood upon its dilated portion, and necessitating the return of the blood by some other channel. Now came the question, how could compression be applied to fulfil the same purpose? At first sight it appeared that any thing like compression between the varix and heart would inevitably increase the tumour by preventing the veins from returning their blood to the heart; but still a ligature does so, and, if it does not kill the patient, often cures the disease. We determined, then, to try the effects of compression upon the spermatic veins as they pass through the inguinal canal, and at first used linen compresses and bandages with some encouraging result; but a little experience proved that a common truss was by far the most convenient means of applying pressure. We have notes of twenty-four cases, of greater or less severity, in which wearing a truss from six to eighteen months has completely cured varicocele of long standing. The patient should be kept in bed, or on the sofa, with the scrotum elevated during the first week of its application, and the pad applied immediately above the pubes, so as to press upon the whole of the inguinal canal. The pad must not be too large and flat, or it will not answer the desired purpose; nor too conical, or it will not only cause inconvenience and pain, but also, by separating the fibrinous insertions of the oblique muscles, reduce the resistance to herniary protrusion. The common spring truss, well fitted, answers much better than the ball and socket pad, and has not the disadvantage of the pad behind. Immediate relief follows the application; the column of blood is prevented from bearing upon the dilated veins; they contract, and the portions subjected to pressure become either greatly thickened in their coats, as their hardness evinces to the finger, or perhaps altogether obliterated. We have not hitherto had an opportunity of examining the state of the veins after death, therefore do not enlarge upon the anatomical effects of this mode of treatment; but strongly recommend it as a simple, safe, and successful substitute for former difficult, hazardous, and doubtful proceedings."

I trust, sir, this will prove that the statements of Dr Thomson and Mr Curling require revision. I do not for a moment question Mr Curling's claims to originality of conception, or priority in publication; but at the same time I claim equal originality for the reflection which first led me to follow the plan in question; and I believed that I was adopting the most certain means of conveying the results of my experience to others, by publishing them in the pages of the first medical review of the day.

I would not ask you to publish this letter from mere personal motives; but as I believe that the utility of simple compression in the treatment, not only of varicocele, but of other forms of varix, will form an important chapter in the history of surgical science, I think I may safely beg for one or two of your valuable pages, and have the honour to be, Gentlemen, very respectfully yours,

London, November 12, 1848. T. Spencer Wells, R.N., F.R.C.S.

DR THOMSON'S ANSWER TO MR WELLS' LETTER.

To the Editors of the Monthly Journal of Medical Science.

Gentlemen,—I beg to thank you for sending me Mr Wells' letter in reference to my paper on Varicocele. In answer to it, I have to say that I cannot admit the justice of Mr Wells' remark, "that the statements of Dr Thomson and Mr Curling require revision," so far at least as it applies to myself. I still hold that Mr Curling "is the only one who has published cases of varicocele cured by this plan." It is not enough for Mr Wells to have mentioned in a review that he had notes of twenty-four cases. That was clearly not equivalent to the publication of them.

As regards Mr Curling's statement, it is evident to me that Mr C. did not consider an incidental notice in a review, of the treatment of varicocele by pressure, as entitling the author of it to say that he had written on the subject, according to the usual acceptation of that expression. If Mr Curling has made any omission in not alluding to this notice, written be it remarked anonymously, Mr Wells has certainly made a much greater in not alluding to Mr Curling's published work, treating of the subject. Mr Wells takes the credit of imparting a practical fact of great value to his readers, evidently not aware that the treatment of varicocele by simple compression had been communicated to the readers of the same Medical Review only two years before, by a reviewer of Mr Curling's book on the Testis, in which the treatment had been explained three years before the appearance of the notice in question.

Mr Wells claims equal originality with Mr Curling for the reflection which first led him to follow the plan. Now, Mr Curling does not claim originality for the reflection ! I may now take the liberty of stating the claims of different parties to the pressure plan of treating varicocele. Sir Charles Bell was the first to announce in his lectures the principle of treatment in regard to varicose veins in general. The application of the principle to varicocele was a suggestion of Mr Key's. To Mr Curling the medical world is indebted for first carrying out the practice, explaining the principle, and publishing the results. As for the reflection, it will be seen at p. 297 of your Journal, that I can, as well as Mr Wells, lay claim to originality; for I had been led to apply pressure by a truss before seeing Mr Curling's work on the Testis. But to this I attach no importance, as, like Mr Wells, I was discovering only what had been already discovered. I can lay claim, however, with some propriety, to having been the first to put the matter of *diagnosis* in a correct light, and of having contrived a truss, which I am satisfied will be found superior to any that have yet been used in the cure of varicocele.[1]

I am gratified to learn that Mr Wells has been so successful in curing varicocele, as his experience is another strong and convincing proof of the efficacy of pressure in the treatment of this disease. Mr Curling and myself differ from Mr Wells to a certain extent, in our view of the *methodus curandi.* Mr Wells evidently regards pressure by a truss as only a safe mode of producing the effects of a ligature ; *i. e.*, his treatment aims at obliterative pressure, while the other, as explained in my paper, " seeks only to restore the diseased vessels to their natural tone and calibre." Which may be the correct view, morbid anatomy has not yet informed us. But this I know, that the view Mr Wells takes is apt to lead the surgeon to a needlessly severe measure. Instead of confining his patient to bed for the first week with the scrotum elevated, Mr Wells, I am confident, will meet with equal success if he rest satisfied simply with applying a truss such as I have recommended, and allow the patient forthwith to pursue his ordinary avocations.

In concluding these remarks, I beg to state that I am perfectly convinced of Mr Wells' originality; but that I cannot admit the correctness of his remark, quoted at the beginning of this letter, as regards at least myself. Trusting that what I have written will be taken in good part by Mr Wells, for whose good opinion I entertain a proper respect, I have the honour to remain, gentlemen, yours truly,

Dalkeith, December 16, 1848. L. Ramsay Thomson.

NATIONAL CHARACTER AS DISPLAYED IN THE STUDY OF MEDICINE.

To contrast the American mind and its prototype, the English, with that of some other nations, it may be said that the Frenchman considers most in disease what there is to see about it; the German, what there is to think about it; and the Anglo-American, what there is to do about it.—*Trans. Amer. Med. Assoc.*, Vol. I.

[1] This instrument may be seen at Fortune's, Princes Street, Edinburgh.

ON THE POTATO AS AN ARTICLE OF DIET. BY PROFESSOR MULDER.

IN a work recently published, Mulder, the learned Professor of Utrecht, has put forth a counterblast against the potato. As an article of diet, he regards it as innutritious, and contends that it is the cause of the moral and physical degeneration of those nations which use it as food! He admits that life may be supported on potatoes alone; but it is not an elastic or healthy life! In fact, the potato fills the stomach with a mass of provender, from which but little healthy nutriment can be extracted. He contends that we shall never see the abuse of spirituous liquors got rid of until potatoes are abolished as a common article of food, on the principle that a certain amount of stimulus is indispensable, and that therefore the ignorant will have recourse to one that is destructive to them, so long as a salubrious excitement is denied.

All this is, chemically speaking, very ingenious; but we cannot agree in the inference. Potatoes, as an adjunct to animal food, are, we consider, highly serviceable to the support of the system; and we think a person would thrive upon such food much better than upon a concentrated essence of Mulder's favourite *protein*, or its compounds. It does not at all follow that substances holding but little nitrogen and sulphur are unadapted to the support of the system; nor is it to be admitted, that bodies which abound in these elements, are thereby fitted to sustain life in its maximum of integrity. Such wild inferences are the result of the ultra-chemical views which Liebig and Mulder are disposed to force on physiologists. Experience is against the first assumption; and if the second were true, *gutta percha*, *caoutchouc*, and *urea*, might take the place of beef, mutton, and pork, when these sources of food were deficient!—*Med. Gazette*, September 15.

AMERICAN GRADUATES.

BEFORE a recent Board, one gentleman defined a lotion to be "a kind of application," and an evaporating lotion "one which does not evaporate." Another confessed his ignorance of the freezing and boiling points of water, and contended that knowledge upon such subjects was useless. One candidate determined castor oil to be the "oil of castor, an animal." Another located the solar plexus in the sole of the foot. All these were graduates.—*Trans. Amer. Med. Assoc.*, Vol. I. p. 303.

ERRATUM IN OUR NUMBER FOR JUNE 1847.

AN error of our reporter in the above Number, at p. 934, in giving an account of a short communication read by Professor Simpson to the Medico-Chirurgical Society, has unfortunately escaped notice till now, when it has been raised into importance by a late letter of Dr Collins in the Obstetric Record. It is stated in our report (*of which we claim the entire responsibility*), that "*one* out of every *twenty-one* mothers died." In the same report, the total number of cases is stated at 1400, which is also erroneous. The true numbers are 11 deaths in 1475 cases, or 1 in 134. The careful and elaborate report *by Dr Simpson*, in which these numbers are given, appeared in our November Number (1843), and is quoted by Dr Collins in the letter to which we refer, in order to contrast it with *our* statement, the inaccuracy of which is but too obvious to every unbiassed person. We beg that, for the sake of our peace of mind, if not of his own, Dr Collins will, for the future, abstain from using the errors of our printers and reporters as a ground for throwing doubts upon the authentic and detailed statements of Professor Simpson, or any one else with whom he may happen to be engaged in professional controversy.—EDS. M. J.

Dr Davidson's article is unavoidably delayed till next month. Several important Reviews are also in type, which were intended for insertion this month.

The Reports of the Medico-Chirurgical Society, and the List of Books received, will also be given next Number.

THE

MONTHLY JOURNAL

OF

MEDICAL SCIENCE.

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*Anæmia and its Consequences—Enlargement of the Thyroid Gland and Eyeballs. Anæmia and Goitre, are they related?* By James Begbie, M.D., F.R.S.E., Fellow of the Royal College of Physicians, and Vice-President of the Medico-Chirurgical Society of Edinburgh.

[*Read to the Medico-Chirurgical Society of Edinburgh, 3d January* 1849.]

I propose to attempt an answer to this question by a short review of some cases which have come under my own observation during the last few years, in which *impoverishment of the blood*, originating in a manifest source, and existing as a primary and chief symptom, has been associated with enlargement of the thyroid gland, and with a remarkable enlargement also of the eyeballs; and where, under treatment adapted to the cure of the constitutional affection, the local diseases have been arrested, reduced, or ultimately removed—affording evidence sufficiently satisfactory that these affections stood to each other in the relation of cause and effect.

Anæmia may spring from different causes, and acknowledge various forms and degrees. It may originate in defect or failure of the functions of assimilation and sanguification, and as a *cachexia* manifest itself in the different shades of *bloodlessness*, as represented in the pale chlorotic girl, the exsanguined coal-miner of Anzain,[1] or in the remarkable case of Haynes, as described by my friend Dr Combe, in a valuable communication made at an early period of the proceedings of this Society, and subsequently published in their Transactions;[2]—or it may occur as the result of changes in the

[1] Journal de Medicine, tom. ix. p. 3.
[2] Edin. Med. Chir. Trans. vol. i. p. 194.

relative proportions of the constituent elements of the blood, and as a *hæmorrhagy* present the features of impoverishment of blood, in the many forms of disorder, the consequence of artificial and spontaneous drains, which we are accustomed to witness from day to day.

In all its forms, and from whatever cause it may originate, anæmia presents the same general and physical signs, and these will detect it in the endless variety of disordered functions pertaining to every organ of the body, and in those local diseases with which it is complicated, and to which it gives rise.

It is the *vascular* form of goitre which I have found in connexion with the condition referred to, and I am inclined to believe that the coincidence of the glandular enlargement with a deteriorated state of the blood is by no means rare; and that those who have opportunities of observing the local affection, especially in its early stages, will find on inquiry that it has coexisted with some functional derangement, or perhaps with some organic disease, which in its nature has a tendency to vitiate or impoverish the circulating fluid, though the influence of such disorder may have fallen short of the production of what is more strictly speaking termed *anæmia*.

That an anæmic condition of the system is the true cause of bronchocele in those districts of the country where it prevails in an endemic form, I am not prepared to assert; but the wretched condition of the vast number of its unhappy victims, and the physical causes which have generally been assigned for its prevalence, together with the pale exsanguined complexion, the protuberant eyeball, and other signs of *anæmia* manifested by its subjects, render the conjecture not improbable.[1]

In all countries, and equally in the endemic and sporadic forms, bronchocele has been observed to make its appearance and to develope itself rapidly during confinement in childbed, and to undergo a temporary augmentation at the period of menstruation. Dr Copland has seldom met with an instance in the female unconnected with some kind of irregularity in the catamenial discharge, or disorder in the uterine function; but I have not been able to find any reference to the condition of the blood, as influenced by such disorders, in explanation of the connexion of the two diseases.

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[1] It is a curious coincidence, and worthy of remark, that the blood-impoverished coal-miner of Anzain,[2] and the goitrous peasant of the Alps, both attribute their apparently dissimilar maladies to the quality of the water they are accustomed to use for their daily sustenance; and that the late researches of M'Clelland, Bally, and Inglis, in different and far distant parts of the world, have gone far to establish the fact, that the endemic prevalence of goitre is connected with the use of water impregnated with calcareous salts. May not the habitual employment of such water so influence digestion, assimilation, and sanguification, as to lead to the anæmic condition, which, in its turn again, is the cause of the development of goitre?

[2] Mackintosh's Elements of Pathology, vol. i. p. 549.

Dr Parry had often seen goitre follow diseases of the heart and other maladies, especially those called nervous, and other writers have noticed palpitations and affections of the lungs in connexion with it; but the former suspected the gland itself to be intended as a diverticulum for blood disposed to flow with too great force to the brain, and the latter have generally considered the chest disorder as the consequence of the local disease, and the derangement it occasioned in the circulation of the blood, instead of regarding it as an indication of a deranged condition of the fluid itself—a condition which, I apprehend, in many of such cases, is the true cause both of the cardiac and pulmonary as well as the thyroidal disease. A just appreciation and skilful application of the means of diagnosis *now* within our reach, must lead to the discrimination of these affections, by discovering those simple and all but infallible signs which tell of the *quality* of the blood in the heart and its unfilled vessels.

It is a form of the *hydrophthalmia* of systematic writers—perhaps that form of dropsy denominated *buphthalmos*, or *ox-eye*, which has accompanied the enlarged thyroid gland as a concomitant symptom of anæmia in the cases now to be related. The etiology of this affection of the eye is acknowledged to be obscure, and our best English writers have done little to elucidate the subject. It is sometimes observed as a congenital affection, and in conjunction with hydrocephalus or other disease of the brain. Its connexion with anæmia, strictly so called, is not referred to, though its dependence on a morbid condition of the system has been admitted by some writers, and scrofula, cachexia, and chlorosis specified as causes. Beer has noticed it as appearing in chlorotic girls as a symptom of that cachexia. Juengken regards it as resulting, among other causes, from serious abdominal disturbances, particularly those connected with menstruation, and from hemorrhoids; but Mr Lawrence, in his learned and classical work on diseases of the eye, remarks, that "the facts which have come under his observation have not afforded the slightest support to these pathological views, nor to the measures of treatment founded on them." I cannot, however, reject them, having found this enlargement of the globe of the eye the result of congestion and effusion, intimately connected with that condition of the system in which the blood is deficient in fibrin and colouring matter, and yielding to a plan of treatment founded on this principle. How the eyeball and thyroid gland are specially called on to manifest this state of disordered blood remains to be determined.

In all the three cases which have come under my own observation, the sight has been little if at all affected, neither has there been any complaint of pain; the cornea has been healthy, the iris natural, and the pupil fitted to discharge its functions. The congestion or effusion appeared to be in the vitreous humour, the chambers of the aqueous maintaining, at least in two cases, their normal size; the eyeball was enlarged and firm, conveying to the fingers, when

pressed, the sensation of great distension. In one case the globe was so full and prominent, that when the patient slept her friends remarked that the eyelids did not cover the ball. Distrusting my own powers of diagnosis in a matter relating to the minute pathology of the eye, I requested my friend, Mr Walker, well known as an experienced oculist, to examine the subject of the second case, now in progress of recovery; he has kindly done so, and favoured me with the following statement regarding it:—

"Both eyes are considerably more prominent than natural; the eyelids, however, close completely over the globes, which Mrs M. says they did not do for some time. Vision is not at all impaired, nor is there any pain in the eyes, but often, she says, there is a feeling of distension. The corneæ are of their natural size and shape; the anterior chambers are not larger than usual; the irides are lively and active; and the lenses are quite transparent. The sclerotic of both eyes is evidently distended from an increased secretion within; they are larger than usual, have somewhat of a bluish colour a little anterior to the insertions of the recti muscles, although they do not seem to be thinned at that point; have a slightly flattened appearance, and do not present the usual globular shape that they do in the healthy eye. Both eyes are much harder than natural. There is no appearance of any morbid growth in the orbits—the globes can be pressed backwards as usual without causing any pain, and their movements are natural."

I.—In the autumn of 1839 my attention was called, for the first time, to the connexion of goitre and anæmia in the case of a lady, aged thirty-two, one of a numerous and healthy family, which, with one exception, had grown up to manhood. She had married at the age of twenty-four, and continued to enjoy for many years a full share of health. She had no family, nor had ever been pregnant. For some years before I saw her she had suffered from continued mental anxieties and distress, and had laboured under profuse leucorrhœa, by which her general health and appearance were much affected. For the last four or five months she had complained of inordinate pulsation of the heart, which was greatly increased by excitement, by going up stairs, by walking fast, by every thing, in fact, which hurried the circulation. At these times her face, which was otherwise pallid, flushed, and she became confused. The eyes presented an unusual appearance, being prominent and staring, giving a wild and startled expression to the countenance; a much larger share of the albuginea was seen than in the normal condition; the membrane looked dark at a distance, and when examined near was found to be pervaded by vessels—many of these were also seen apparently in the sclerotic coat itself when examined through a lens; there was no dimness of vision, but a painful sense of distension of the eyeballs; all this was increased on the occasion of palpitation, or when the face was flushed. Simultaneously with the appearance of these symptoms, an enlargement of the thyroid gland manifested itself. It was soft, smooth, and elastic, and of equal character throughout, presenting the form of the hypertrophied gland, and had rapidly developed itself to its present size—that of three or four times the magnitude of the gland in health; but it was subject to remarkable variations in this respect, according to the state of mind, rest, or palpitation. It appeared to be highly vascular, and conveyed to the touch the sensation of an erectile tumour. It had suffered no diminution from the continued application of iodine. The pulse at this time

generally ranged from 100 to 130 ; it was small and jerking, and, on the occasions of excitement, accompanied by a thrill. The inordinate action of the heart was felt beating in the head, so as to produce a state of almost constant watchfulness. There was much breathlessness, and frequent faintness, severe headach, vertigo, and tinnitus aurium. On attentively listening to the heart's action, the contraction of the ventricles seemed somewhat prolonged, and it was attended, especially under palpitation, by a soft bellows-murmur at the aortic orifice, and a corresponding *bruit* was heard in the carotid and other large arterial trunks ; there was no extended præcordial dulness, neither was the impulse of the heart very remarkable. Along with these symptoms there was a high degree of nervousness, and much derangement of the digestive functions. The catamenial discharge was imperfect and irregular. A variety of treatment had been for some time pursued for the relief of these symptoms. Blood-letting had been resorted to, with that measure of benefit which Dr Marshall Hall has so well described and accounted for in such circumstances,[1] digitalis, mercury, and many others, but all in vain, and the condition of the patient was considered hazardous. The appearances and signs, however, were manifestly those of impoverished blood, and with this view a change of remedies was adopted, which, after perseverance in for many months, has issued in restoration to health. This consisted of large doses of the carbonate of iron, with gentle aloetic purges, soothing doses of hyoscyamus, a full diet of animal food, a change of air to the seaside, and regular passive exercise in the open air. Under this plan the general system gradually became renovated, the leucorrhœa subsided, the goitrous swelling diminished and disappeared, the eyes regained their wonted character, the complexion its accustomed hue, and the heart and arteries their normal beat. This lady travelled on the continent for many months after I first saw her, and I had no opportunity of ascertaining the progress of her restoration ; but I saw her two years ago, and she was then quite well.

II.—Some years passed away before I had an opportunity of again tracing the connexion between bronchocele and anæmia, and verifying the symptoms and appearances just described, when I was consulted by my friend Dr Beilby, in the winter of 1846, in the case of a lady aged thirty, married, and the mother of two children. The commencement of her illness was marked by long-continued and frequent hemorrhage from internal piles, often for weeks together, to the amount of 5 or 6 oz. daily, and extending over a period of between three and four years, during a long period of which she had been subjected to much anxiety and distress of mind. The hemorrhage at last ceased on the occurrence of an abscess near the anus, and never returned ; but it left her reduced in strength and exsanguine. She had too long disregarded the repeated loss of blood, and only became alive to her situation when the violent palpitation of the heart, with throbbing and giddiness of the head, arrested her attention, which occurred almost immediately on the cessation of the hemorrhage. The catamenia continued regular. On my first visit, twelve months after the appearance of these symptoms, the large and prominent eyeball, which I cannot well describe, but which once seen cannot be readily forgotten, the dark discoloured eyelid, and the pallid complexion which bespeaks poverty of blood, induced me to examine the thyroid gland before listening to the heart's action, or feeling the pulse at the wrist. It was tumid and hypertrophied, especially the lateral lobes, which were enlarged to four or five times their normal bulk, and had long resisted the use of iodine, which had been administered in large measure, and, according to the experience of the patient, with manifest disadvantage. The tumour, as in the former case, was subject to remarkable variations, according to the state of the circulation, augmenting and decreasing as excitement or rest prevailed. She felt very sensitively the marked change

[1] "On Loss of Blood." Med. Chir. Trans. vol. xiii. p. 140.

in her expression, from what she termed the protruding eyes, and experienced at same time great discomfort from the sense of pressure, as if the eyeball were too large for the orbit. I need not repeat a description of the symptoms—the throbbing of the head, the ceaseless impulse of the heart, the anæmic murmur of the ventricle, the continuous rushing sounds in the veins of the neck and in the thyroid swelling, the whiff of the carotids, or the jerking of the pulse at the wrist. One set of symptoms I may notice in the words of the patient herself,—"A very painful sensation was experienced on leaving the horizontal posture, violent cough came on, causing retching and an agonizing pain in the head, seemingly caused by an extreme rush of blood to the brain, with a sense of bursting. This was relieved on resuming the horizontal posture, and taking food. As a preventive, I was forced for some months to breakfast immediately on awaking from sleep, lying perfectly still in the posture in which I awoke, as even a slight raising of the head would occasion distressing pain. After eating I could rise with impunity." These are manifestations of the enfeebled brain, and part of the train of symptoms induced by impoverished blood. Every symptom was referable to this state; the murmur of the heart, and arteries, and veins, proclaimed its presence, and nothing remained but to recommend the simple, safe, and satisfactory treatment of the former case. It was begun, and persisted in for some time, and attended by beneficial results; it was interrupted, however, from various causes, so that the success has not been equal to its known efficacy. Still, however, at this time the patient is manifestly improved, the thyroidal swelling is greatly reduced, the protruding eyeballs much less conspicuous, and all the signs which accompany the anæmic condition gradually disappearing. She can walk many miles without breathlessness, and lie down and rise up without pain or confusion.

III.—A third sufferer from anæmia and goitre I have at this time under my care, in the person of an unmarried lady, aged fifty, who had for a long series of years enjoyed perfect health till about four years ago, when she became the subject of dyspeptic and uterine derangement. For many months she laboured under profuse and wasting leucorrhœa, when the protracted drain was followed by distressing palpitation, hurried breathing, rapid, feeble, and jerking pulse, pallid complexion, varied by flushing on the least excitement or exertion, great nervousness, disordered secretions, the characteristic eye, and the hypertrophied thyroid of this form of anæmia. The isthmus of the gland was more prominent than in the former cases, but the bulk of the tumour was less than in either. She had undergone a variety of treatment with varying success for some years before I saw her for the first time, two years ago. At that time she was chiefly anxious regarding the protuberant eyeballs, and little concerned about the goitre or her general health, which she had begun to think did not admit of remedy. The general and physical signs were those of anæmia; and, believing that the local affection would yield to constitutional treatment, she was immediately placed under full doses of iron, the citrate was the form employed, full diet of animal food, and a moderate share of Bass's ale. She improved rapidly, but was seized in the autumn of 1847 with severe dysentery, then epidemic in the city. Through this ordeal she struggled, and after a tedious convalescence she regained her former position; the goitrous swelling and the projecting eyeballs appeared to suffer no change during this long illness, neither was the anæmic murmur of the heart and arteries affected by it. She resumed her former treatment, which had been interrupted by the attack of dysentery; and now, at the close of another twelvemonth, she is greatly improved in health, the thyroidal swelling is scarcely perceptible, the protuberant eyeball much reduced, the pulse, though rapid and feeble, and still easily affected by exciting causes, greatly moderated, and the breathing much more natural. There are no contra-indications to the continued use of iron and animal food; and a confident expectation may be entertained, that, under the persistent employment of them, the functions of assimilation and sanguification will be restored to

health, and the local disorders now dependent on their morbid condition entirely removed.

I might add to these histories of anæmia and goitre, thus rapidly gone over, other cases communicated to me by professional friends in illustration of the connexion; but I shall not detain the Society longer than merely to state, that in one case the anæmic condition was the consequence of long-continued hemorrhage from the bowels, and complicated with enlargement of the thyroid gland and eyeballs; in another, these were associated with amenorrhœa; and in a third, the goitrous swelling, without the enlarged eyeballs, was observed in a young woman labouring under chlorosis. It deserves remark, that in all the cases now referred to, the subjects were females.

I had proceeded thus far with my narrative, when my attention was called to a paper in the *Dublin Journal of Medical Science*,[1] entitled, "Observations on a *peculiar* form of disease of the heart, attended by enlargement of the thyroid gland and eyeballs," by Dr R. L. Macdonnell of Dublin, and at the same time to Mr Hill's Remarks on that communication,[2] and through them, to the proceedings of the Dublin Pathological Society,[3] where Sir Henry Marsh had more than once introduced the subject of this form of heart disease. These, as well as the notice of other cases by Dr Graves,[4] I am somewhat ashamed to acknowledge, had previously escaped my observation. I have now perused them with great interest, and beg to call the attention of the members of this Society to them, as affording additional value to the cases now submitted; and I hope to show a satisfactory confirmation of the views I have been led to take of the subject, though it is not without considerable hesitation that I venture to claim them as illustrations of anæmia, seeing that some of the most eminent physicians of the sister island have regarded them as examples of a *peculiar* form of cardiac disease, and neither indicated by their observations, nor by the mode of treatment adopted, that they consider them cases of that description.

In each of the three cases which I have just related, it will be observed that there existed an adequate cause of *impoverishment of blood;* in two, in the form of lasting leucorrhœa; in the other, in that of continued hemorrhoidal discharge;—that these drains first affected the heart, producing the anæmic palpitation; subsequently symptoms referred to the brain and the lungs; in fact, the well-known symptoms of *poverty of blood*, to which I have taken leave to add, as part of the train of sequences, enlargement of the thyroid gland and eyeballs. I remark a considerable variety in the cases now on record, in regard to the appearance of the prominent symptoms. In some, the enlargement of the thyroid exists without

[1] Vol. xxvii. p. 200.
[2] Ibid. p. 399.
[3] Dublin Journal of Medical Science, vol. xx. p. 471.
[4] Clinical Lectures, vol. ii. p. 193.

the dropsy of the eye; in others, the enlarged eyes without the goitrous swelling; while in those in which they coexist, a considerable interval of time has elapsed before the one has followed the other;—but in all, the manifestations of disordered circulation have been first observed.

The Dublin cases, in all essential characters, bear a striking resemblance to the three histories now read; and though the fact of the subjects of them being sufferers from *impoverishment of blood* is not adverted to by any of the authors, I apprehend that sufficient evidence is afforded by such of the cases as are detailed at length, to show that such was the condition of the patient. For instance, John M'Keon, the subject of the first case related by Dr Macdonnell, long before the first symptoms of cardiac affection presented themselves, is stated to have suffered from indigestion, so that his food was passed in an almost unaltered state; to have been subject to prolapsus ani, and occasionally attacked with bleeding from the protruded gut. It is mentioned also in regard to him, that he was subject to diarrhœa, by which he was greatly exhausted; that at one time it lasted for four months, obliging him to go to stool fifteen or sixteen times a-day; that towards the close of this attack, the stools acquired a black colour, and highly offensive odour, and that there were at times profuse discharges of blackish flakes like tea-leaves. It was about this period, says Dr Macdonnell, that he first began to suffer from palpitations; his pulse was never under 120, and was at times as high as 200 (?). Then follow in succession the enlargement of the thyroid, excruciating headachs, throbbing and sleeplessness, enlargement of the eyeballs, and all the group of symptoms already enumerated.

The second case detailed by Dr Macdonnell, appears to me to bear evidence also of the anæmic condition. The subject of it, a woman aged twenty-two, of leucophlegmatic habit, began to menstruate at fifteen, up till which time she enjoyed good health, and ever afterwards she suffered from leucorrhœa, and latterly from amenorrhœa. She then became affected with violent headach, palpitations, great lightness of head, enlargement of the eyeballs, "with a peculiar staring appearance, which attracted the attention of all her friends, and on more than one occasion induced them to think that she had become maniacal," together with a numerous train of symptoms the detail of which is unnecessary.

Dr Graves, in his admirable clinical lectures, refers to three cases of violent and long-continued palpitation in females which he had lately seen, in each of which the same peculiarity presented itself, viz., enlargement of the thyroid gland; but he has not detailed the history of these cases, nor indicated his views of their origin, but he has given, at some length, the case of a lady aged twenty (communicated to him by a friend) who became affected with some symptoms which were supposed to be hysterical. After she had been in this nervous state about three months, it was observed that her pulse had become singularly rapid. The rapidity existed without any apparent

cause, and was constant, the pulse being never under 120, and often much higher. She next complained of weakness on exertion, and began to look *pale* and *thin*—the rapidity of the heart's action continued, and then the enlargement of the eyes took place, and afterwards that of the thyroid gland. In about fourteen months the heart presented all the signs of Laennec's passive aneurism. It would be highly instructive could we ascertain the character of those symptoms "supposed to be hysterical," and the general state of health during the three months immediately preceding the period when the pulse became singularly rapid, and not less so could we ascertain that they were connected with some leucorrhœal or other drain.

The investigations of Sir Henry Marsh, who was the first, I believe, to describe the symptoms during life, and as yet the only one the appearances after death in this interesting affection, are very valuable. At a meeting of the Pathological Society of Dublin in January 1841, this eminent physician, after adverting to the fact of his having, the year before, described to the Society a *singular variety of disease of the heart*, now wished to exhibit another preparation illustrating this "*very curious and interesting affection.*" The disease of which he then spoke presented the following striking characters—remarkable engorgement of the veins, particularly of those of the neck, rapid, violent, and irregular action of the heart, and these in every instance coexisting with enlargement and swelling of the thyroid gland. He also mentioned that, in the majority of these cases, there was a remarkable prominence and protrusion of the eyeballs, so as to give to the group of symptoms by which this disease was characterised a very striking feature. After describing the leading symptoms more particularly, he goes on to exhibit the appearances on dissection, to which I shall afterwards allude, and then read the notes of another case which had lately come under his notice, and in which he observed, "the disease was very well marked." The subject of it was a woman, aged forty, who had been originally very plump. She had been married, had borne ten children, and menstruated regularly. She was a person of lively and energetic manner, of a highly nervous temperament, and evinced much activity both of body and mind. She had been subject to *epistaxis*, but exhibited no signs of disease either of the heart or chest. She had been for a considerable time in attendance on an epileptic relative, and this had kept her in a constant state of nervous excitement and apprehension. Sir Henry said he mentioned this particularly, because the disease he had been describing seemed to begin with nervous palpitations. Her face was *pale* and somewhat tumid, eyes prominent, her lips purplish, the veins of the neck considerably distended, and the thyroid body much enlarged. The impulse of the heart gave motion to the integuments over a space exceeding far the ordinary limits of the cardiac region; and there was a considerable extent of dulness

on percussion. The first sound of the heart was short, quick, and loud; the second faint and scarcely audible, in consequence of being masked by the first, but there was no *bruit de soufflet* or any other abnormal sound. She complained of attacks of dyspnœa in the morning, accompanied by a sensation of fluttering in the heart. Any unusual exertion or sudden mental emotion was sufficient to bring on distressing palpitations. Her pulse was quick and jerking, never below 90; her respiration clear and puerile. The bruit in the carotid artery, where it was pressed on by the thyroid gland, was actually perceived by the patient, and caused a great deal of annoyance. She felt a whizzing sound in her neck, of which she never could get rid, and it was one of her most distressing sensations. Sir Henry considers this a good description of the disease. He thought there was no valvular disease; but he believed that if the affection had been unchecked, morbid alteration of the valves would have been the probable result. In the treatment the strictest quiet and rest were enjoined, and the exhibition of the carbonate of iron with extract of hyoscyamus was very beneficial.[1] In all this, I think, we cannot fail to recognise a faithful picture of anæmia—the history, the progress, the development, the means of relief, all testify to it; and if any irregularity in the physical signs is calculated to raise a doubt, that doubt should be removed when we call to mind, that long-continued functional derangement of the heart, such as that produced by anæmia, may sooner or later pass into dilatation of the chambers of that organ, and so modify considerably the signs emitted by auscultation and percussion.

On reviewing the history of bronchocele and its association with other disorders, I am willing to admit the force of Professor Hasse's observation, that the different varieties occur so often independently of, or only accidentally complicated with other diseases, that it is by no means an easy matter to establish any constant relation between them; still I cannot but hope that an approach has been made towards this end by the views indicated, and the cases recorded in the preceding remarks. We have seen the thyroidal swelling in connexion with leucorrhœal and hæmorrhoidal discharges, commencing when the drain has been established, obtaining its full development when that drain impoverished the blood, and subsiding under the use of means which we know are calculated to restore the red particles. And what are the other known conditions and disorders which have been observed in conjunction with goitre, but just such as are capable, under particular circumstances, of producing the same deterioration, or are themselves the signs and evidence of that deterioration? The influence or presence of child-bearing and menstruation, or of menorrhagia and amenorrhœa, can readily be understood to operate in this way.

The attempt to connect bronchocele with excitement of the gene-

[1] Dublin Journal of Medical Science, Vol. xx. p. 472.

rative system, and to establish a relation between the diseases of the thyroid gland and those of the genital organs, has entirely failed. If some of the cases now related appear to countenance such an opinion, in consequence of the illustration they afford of the glandular swelling being associated with the uterine affection, the others afford no such support; while they all equally confirm the simple view already taken, and prove that the same train of events follows the continued hæmorrhoidal discharge and the leucorrhœal drain.

And here I cannot help adverting to the misconception which I apprehend prevails, at least in some cases, in regard to the effects which the thyroidal tumour is supposed to produce by means of its interference with the circulation and respiration. The pressure of the enlarged gland is every where acknowledged to be the cause of obstruction to the free descent of blood from the head, and in this way the occasion of headach, giddiness, confusion of thought, and other symptoms already enumerated. These views were entertained in regard to that class of symptoms in some of the cases just related, and led to the adoption of a line of practice not likely to be beneficial; but an attentive observation of the origin and development of the whole phenomena, will, I apprehend, lead to sounder views, and ascribe to the want of healthy and sufficient circulation through the brain, the effects supposed to arise from an opposite condition—the impoverishment of blood being the cause, at once of the disorder of the brain, the embarrassment of the lungs, the labouring of the heart, and the congestive turgescence of the eyeball and the thyroid gland. It cannot have escaped observation, that sufficient evidence of that impoverishment, and an adequate cause for its presence, existed previously to any manifestation of disordered cerebral or pulmonary circulation; while I may here mention that in all the cases the bronchocele was of a soft and elastic character, and not fitted by its size, position, or relation, to occasion such embarrassment as was observable, particularly in the two cases first related, and that all these disorders subsided under a plan of treatment calculated to effect a change on the constituent elements of the blood; we may therefore conclude, that to its abnormal condition the general disturbance, as well as the local disease, is to be ascribed.

But it may be replied that these views, if correct, can only apply to that form of goitre denominated *vascular*, in which the blood-vessels are chiefly implicated, and cannot explain the melicerous degeneration or cystic formation in the thyroid gland. All authors, however, agree that the vascular passes into the other forms of bronchocele, and though sometimes temporary and marked by periodical augmentation and decrease, through causes acting on the general circulation, it becomes permanent and assumes the character of the other forms of degeneration. I have under my care at this time a lady aged sixty-three, who in early life was subject to leucorrhœa. She married when a girl of fifteen, having then a small, it may be a graceful goitre. She became the mother of a numerous family, and

at each of her confinements could remark the augmentation of the thyroid gland. She remarked it more conspicuously on the occasion of a most painful and tragical event in her family, when, in the course of one night, she assures me it nearly doubled in size. It has made progress ever since, and now, at the period of her grand climacteric, the unsightly mass rests upon her chest, and requires the aid of a suitable support. It is hard, unyielding, and unequal, and presents the character of hypertrophied and disorganized structure. It is curious to observe in this lady, even now, many of the features of the anæmic condition—the great nervousness—the morbid appetite —the dislike of animal and the craving for vegetable food—the pale waxy complexion—the palpitating heart—the throbbing head, and the thrilling pulse of unfilled arteries.

But the best proof that this form of thyroidal enlargement passes into structural disease of a permanent kind, is afforded by the dissections which Sir Henry Marsh has been privileged to make after death. After relating the more prominent symptoms of a well-marked case of the disease now under consideration, Sir Henry exhibited to the Pathological Society a preparation illustrating the appearance after death. The first point to which he directed attention was the condition of the thyroid gland. It was altered in appearance since it had been first examined; it was now considerably shrunken; in its recent state it was much larger. When first examined, its surface was irregularly lobulated, and the lobes or cysts contained a considerable quantity of clear fluid. During life, it projected so as to form a very large and prominent tumour; whenever there was any inordinate action of the heart, it appeared to swell and increase in size, whilst at the same time the veins of the neck were so greatly distended as to give the patient a very extraordinary appearance. I must refer to the proceedings of the Society for a full account of the morbid appearances in other parts, and content myself now by stating, that the internal jugular vein of the right side was found so very much dilated, that when emptied by puncture it measured an inch and a half across; it was filled by dark fluid blood: that the left auricle was enormously enlarged; the walls were not hypertrophied, but the capacity was far beyond the normal standard. The dimensions of the right auricle were also considerably increased, though not to such an extent as those of the left; the muscular fibres of its walls were slightly hypertrophied. The left ventricle was dilated and hypertrophied, but not to a considerable degree. Along the margin of the mitral valve were depositions resembling granular fat. The same disease existed, and to a greater extent, in the valves of the right ventricle. The symptoms during life had existed a long time; the disease terminated in general anasarca, followed by erysipelas and gangrene.

It is worthy of remark, that while some cases of this affection, like that now related, pass into organic disease, others appear to undergo a spontaneous cure, such as that detailed by Dr Macdonnell,

when, after a continuance of many years, it is reported that the strength is daily increasing, the eyeballs have much diminished, though they still retain some of the peculiar staring; the enlarged thyroid has become more solid, and the heart's action is now tranquil, and unaccompanied by any abnormal sound. At an earlier period it is mentioned, that "the two lower thirds of the neck was occupied by a soft flabby tumour corresponding to the lobes of the thyroid body, the left portion of the tumour being much larger than the other. The whole tumour measured *sixteen* inches and a half around its most prominent part to the sixth cervical vertebra."

If anæmia is capable of such varied and extended effects, how important is its diagnosis! Fortunately in most of its forms it is not difficult of detection; it proclaims its own presence; and in the pallid countenance and rapid pulse, the breathlessness, and faintness, and palpitations on slight bodily or mental exertion, it is readily recognised; but even when masked and unsuspected, there are physical signs of no uncertain character which betray its lurkings. The soft systolic bellows murmur heard in the præcordial region, and followed by the ear into the large arterial trunks, is present when the impoverishment is great, and almost always under palpitation from excitement or other causes; and the peculiar sighing sound caused by the descent of the attenuated blood in the veins of the neck, and recognised by the stethoscope, even when the poverty is inconsiderable, lead to a sure and certain diagnosis, and suggest a plan of treatment which, in a large majority of cases, will be found equally sure and certain.

"These murmurs," says Dr Latham, "whether appertaining to the heart and arteries, or to the veins, which have their origin in the quality of the blood that circulates within them, furnish an eminent example of the highest degree of comprehensiveness, both for knowledge and for use, which can belong to the idea of a symptom.

"Where these murmurs are, there a countless variety of other symptoms is found in company with them, pointing to all organs of the body, and giving notice that the functions of all are going wrong; the surface pale and cold, palpitation and dyspnœa, appetite perverted, digestion imperfect, nutrition insufficient, secretion scanty and unhealthy, pain every where, and a shattered nervous system and an enfeebled brain. Such a portentous crowd of symptoms strikes the observer at once. But what they all mean we cannot tell until we take one single symptom for their sole and sufficient interpreter. The murmur, which is at the same time endocardial and arterial and venous, is comprehensive of them all, and includes the knowledge of them all, inasmuch as it points directly to their one common source, even the impoverished blood. And further, this same murmur not only contains the knowledge of all the rest, but it is the single representative of them all as an indication of treatment. Standing, as it does, for the sign of impoverished blood, we treat

what it denotes and nothing else. But in doing so we treat inclusively every error of function throughout the body which proceeds from it." [1]

How often have cerebral affections, severe, lasting, and intractable, been completely removed by discovering an anæmic cause in a long-continued or oft-repeated hæmorrhage from piles! How often have cardiac disorders, too readily pronounced organic, been remedied by detecting the same cause in a long neglected menorrhagia or wasting leucorrhœa! And so will we find, in some cases of less pressing moment, that a cure has been obtained when means considered specific had failed, by discovering that the quality of the blood was the real cause of the local affection.

Our first object in the treatment of anæmia and its secondary disorders is to ascertain the exciting cause, and having arrested or removed this, to improve the condition of the blood by the free use of the preparations of iron, and a liberal allowance of animal food, unless there are complications of such a kind as to contra-indicate their use. Wine is seldom necessary, and generally hurtful; but porter or ale may be taken with advantage. A change of air, travelling, and passive exercise, also do good. Persistence, however, is absolutely necessary both on the part of the patient and his attendant. *Iron*, the chief means of cure, in order to be effectual, must be continued for months, and perhaps, after an interval, again and again renewed. It will not, however, disappoint our expectations; for whether it act by invigorating and improving the digestive and assimilative functions, or by restoring and augmenting the red particles of impoverished blood, there is really no remedy in the hands of the physician, whose virtues have been so thoroughly tested and so deservedly recommended to our confidence and trust.

ARTICLE II.—*Case of Spontaneous Cure of Ovarian Dropsy, by means of an Ulcerative Opening of the Cyst into the Bladder.* By JOHN HUGHES BENNETT, M.D., F.R.S.E. Professor of the Institutes of Medicine, and one of the Professors of Clinical Medicine, in the University of Edinburgh.

(*Read to the Medico-Chirurgical Society of Edinburgh, January* 3, 1849.)

IN a paper which I had the honour of reading to this Society in December 1845,[2] I described the general anatomy and mode of growth of encysted tumours of the ovary. I pointed out that, while in many cases ulceration took place in the external sac, and permitted fluid secreted within the tumour to flow through the openings, and collect in the cavity of the peritoneum; in other cases the sac

[1] Clinical Lectures on Diseases of the Heart, vol. i. p. 72.

[2] Pathological and Clinical Observations on Encysted Tumours of the Ovary. —*Ed. Med. and Surg. Journal*, January 1846.

expanded more rapidly, and the included cysts broke into each other. Both external and internal ulcerations may occur together; but when the former do not take place, the latter are more rapid, gradually produce a less number and larger size in the secondary cysts, and at length, in the last stage of the growth, they all break down, and occasion one large cyst only. I further stated, that if at this period in the progress of an ovarian encysted tumour, a puncture was made, or an ulcerative opening formed, whereby the contents could be discharged, and the walls of the cyst brought into contact, a permanent cure might be accomplished; and I alluded to the occasional occurrence of rupture of such cysts, and their subsequent disappearance as corroborating this statement.

The following case seems to me an interesting proof of the correctness of these pathological views:—

Case.—Anne Pyper, a servant, aged 25, was admitted into the *female* clinical ward of the Royal Infirmary, Nov. 8, 1848. She had been delivered fourteen days previously of a male child in the Maternity Hospital; and, on inquiry, I find that the labour was a natural one, and presented nothing unusual. On the birth of the child, however, the abdomen still continued enlarged, and at first led to the suspicion that another fœtus remained in the uterus. After a time the true nature of the case was rendered manifest, and a large swelling was detected, which was moveable to a certain extent, and presented all the characters of an encysted tumour of the left ovary.

When I first examined her in the Infirmary, I found the abdomen swollen to about the size of a woman's during the sixth or seventh month of pregnancy. The tumour extended from the epigastrium to the pubis, but bulged considerably towards the left side. Its surface was irregular; and two large nodules, each the size of a cocoa-nut, existed about its centre. It was tense and firm to the feel, somewhat elastic, but no fluctuation could be detected. The tumour was firmly fixed, and the seat of constant pain, especially in the left lumbar region, which was increased by pressure, by lying on the right side, or on assuming the erect posture. The urine was of a slight yellow colour, and presented its normal characters. The digestive, respiratory, circulatory, nervous, and integumentary organs appeared to be healthy. She had observed the tumour seven months before her delivery; and it has gone on gradually increasing, and been somewhat painful from the first. *Eight leeches were ordered to the most painful part of the abdomen.*

For four days the patient remained in the same condition, the local pain, however, having been relieved by the leeches. On Nov. 12, my attention was directed to the urine, which now presented a copious white deposit, occupying two-fifths of the jar, while the supernatant portion was of a light amber colour, and unusually viscid. The deposit was determined by the microscope to consist of pus, mingled with a few compound granular corpuscles. The clear portion was strongly coagulable by heat and nitric acid.

At first I imagined that the cyst had burst into the vagina, but the patient and nurse assured me that there was no discharge between the intervals of micturition, and that all the fluid came from the bladder.

The urine presented the same characters during the next three days; the amount discharged during the twenty-four hours being about three pints. On the 15th, I observed that the tumour had somewhat diminished in size, its hardness and tensity had disappeared, and distinct fluctuation was perceptible in it. *A broad flannel roller was ordered to be applied firmly round the abdomen, and compression made by means of pasteboard, previously soaked and modelled to the abdominal surface.*

From this time, the abdomen rapidly diminished in volume, while the amount

of purulent viscous fluid discharged from the bladder varied from three to five pints in the twenty-four hours. The appetite and general health continued good; and she was ordered nutritious diet, with four ounces of wine daily. On the 23d, the amount of pus contained in the urine was greatly lessened, and the clear portion presented only a slight haziness on the addition of nitric acid. On the 27th the abdomen had regained its natural size, although a dense mass, evidently the collapsed ovarian sac, could readily be distinguished, occupying the left iliac and hypocondriac regions. The urine now also was natural in quantity, and presented only a slight sediment, consisting, as shown by the microscope, of some crystals of oxalate of lime, and a few pus globules.

From this period she may be said to have recovered. She suffered occasionally from uneasy feelings on the left side, sometimes amounting to pain, which were relieved by the application of four leeches, followed by a small blister. One of the leech bites ulcerated superficially, but soon healed up. She was dismissed on the 18th of December, expressing herself as being well in every respect, having been sitting up and running about the ward for the fortnight previous. The indurated mass in the left iliac region was greatly diminished in size, but still very perceptible to the feel, though not to the eye.

Remarks.—The history of this case can, I think, only lead to one conclusion, namely, that an ovarian encysted tumour was present on the left side; that the individual cysts had, if not altogether, at all events for the most part, broken down to form one large cavity; that the contents of this cavity had suppurated, and a fistulous opening, formed either into the ureter or bladder (most probably the latter), through which the contained fluid was evacuated, permitting collapse of the sac and cure of the disease. The permanency of this cure will depend upon, whether all the secondary cysts had been ruptured and were broken down before the fistulous opening took place. This is a point which it is impossible to ascertain with certainty; but a careful examination of the woman before she left the Infirmary, convinced me that no rounded nodules or cysts could any where be felt.

The only instance I am aware of, in which an opportunity presented itself of dissecting an ovarian encysted tumour some time after its spontaneous rupture, was in an individual I saw examined with Dr Simpson, by the late Dr Makellar.—(*Monthly Journal*, Jan. 1847, p. 558.) In that case the cavity of the cyst was almost obliterated, and its walls were thickened and of cartilaginous hardness. A fistulous opening, however, was kept up between the tumour and the abdominal walls, below the umbilicus, where it had burst, and the patient sank from the continued discharge. How far a communication with the external atmosphere in this instance, and the presence of chronic peritonitis, may have operated unfavourably, I do not know; but the total cessation of all discharge, and absence of these circumstances in the case related, augurs well for her permanent recovery.

Whether a fistulous communication between the ovarian sac and urinary passages be favourable or not, is uncertain; for I have been unable to discover any recorded case in which this has ever happened. Many instances are to be met with where similar cysts have burst into the peritoneum, the fluid been absorbed, and

excreted in large quantities by the kidney as urine. Other cases are to be met with, where the contents of the tumour have burst externally by ulceration through the abdominal walls, or into the vagina, or into the intestines; but in none, so far as I am aware, previous to the one now related, have the contents of the tumour been evacuated directly as a purulent viscous fluid from the bladder, proving a direct communication with that organ.

The occasional occurrence of such spontaneous cures, has led to the proposition of producing permanent artificial openings, with a view of imitating a natural cure. Mr Bainbrigge of Liverpool—(*Prov. Med. and Surg. Journal*, vol. iii. p. 593)—suggests making an incision into the sac, and uniting its edges with the external wound; and Dr Tilt of London—(*Lancet*, vol. ii. 1848)—has lately proposed making a minute aperture by means of Vienna paste, so as to cause a permanent opening. Such practice can only be useful at a particular period in the growth of ovarian tumours—that is, when all the internal cysts have broken down into one; indeed, it is only in these cases that Dr Tilt proposes making the aperture. But such cases are exceedingly rare, and the practice recommended can be of no real advantage until these gentlemen instruct us how to distinguish in the living subject unilocular from multilocular cysts. Numerous dissections of ovarian tumours have convinced me, that in the present state of the art this knowledge is not to be arrived at with any degree of certainty; and that consequently any proposal, however valuable in itself, which is founded upon the assumption of our possessing that knowledge, is not likely to be practically beneficial.

Another proposition, however, has been made, which deserves consideration. In the paper formerly referred to, I remarked—"One practical rule to be followed in the treatment of these cases is, not hastily to have recourse to tapping, but by all possible means of delay to further the natural disposition, which the internal cysts exhibit under pressure, of forming one large sac." "There is every reason to suppose, that artificial pressure is capable of facilitating the absorption of the walls of the secondary cysts, and their opening into each other; but we possess no means of ascertaining when only one sac is produced. That it has succeeded in obliterating and ultimately curing the disease, however, has been proved by Mr Isaac Brown—(see cases recorded in the *Lancet*)—whatever other opinions may be held respecting the propriety of his treatment."

Now, the case I have read seems to me illustrative of the effects of pressure. It must be acknowledged that the seven months which had elapsed between the time the tumour was first perceptible, and the period when it spontaneously burst and collapsed, was a remarkably short one. In the most favourable cases this result takes about two years to accomplish by itself; but in the instance of Pyper, the tumour was subjected to the gradually increasing and equable pres-

sure of the pregnant uterus, and to its influence must, I think, be attributed the fortunate result and rapid breaking down of the secondary cysts. The ulceration into the bladder was probably determined by the direction the pressure had assumed in this case, and, of course, could not be imitated artificially.

There still only remain two methods of curing an ovarian dropsy by art—viz., by excision, and by pressure followed by puncture. The case I have narrated confirms the views suggested by pathology with regard to the *modus operandi* of the latter treatment; and if, in cases which do not admit of extirpation, pressure be so gradually and equably applied as to obliterate the internal or secondary cysts, an artificial opening then made would cure the disease. The difficulty is to ascertain when the moment for making the puncture has arrived—in other words, when a multilocular is converted into a unilocular cyst. In the present state of the art, this, as I have said, is impossible; but, as an exact indication of the difficulty is often the best preliminary to its removal, I do not despair of some day seeing it completely conquered by the cultivators of rational medicine.

ARTICLE III.—*Observations on the Primary Influence of Personal and Domestic Cleanliness as a Sanitary Measure in the Prevention of Typhus, Cholera, &c., compared with that of the Removal of External Nuisances, such as Sewers, Cesspools, &c.* By WILLIAM DAVIDSON, M.D., formerly Physician to the Glasgow Royal Infirmary.

FILTHY streets and lanes, cesspools, sewers, drains, and accumulated heaps of decomposing animal and vegetable matter, together with their concomitants, personal uncleanness and deficient ventilation, have long been associated in our contemplations with the spread of plague, fever, cholera, &c., and practical measures founded on these have frequently been inculcated on the public by our civic authorities; but more particularly of late, when visitations of this kind have been so frequent and formidable, and when the habits and habitats of our working and pauper population threaten to contaminate every thing around them. The public are therefore sufficiently alive to the general magnitude of the danger, and the government of our country has wisely interposed its authority for the purpose of stemming this overwhelming torrent, and much good will undoubtedly result from such enactments. At the same time, we fear that the evil will only be partially diminished so long as impurity of skin, and a strict regard to personal and domestic cleanliness, are so little attended to by the lower orders of society. It will obviously be a matter of some difficulty to ascertain the precise or most operative agencies in the propagation of these diseases, when epidemical, from their almost universal association with personal impurity, filthy

streets, sewers, and foul accumulations; but, in order to render the analysis more conclusive, the following points will become the subject of our investigation.

1. Plague, typhus, cholera, &c., spread most extensively in filthy, ill-ventilated localities, while the inhabitants are similarly characterized in their personal and domestic habits; and these various circumstances are almost universally associated together.

2. Narrow and ill-ventilated streets and lanes, associated with public sewers, cesspools, dunghills, &c., being the nuisances always before the eye, on general inspection, have obtained a character of more influence and power than those internal, and consequently more hidden, sources of disease; and that probably filthiness of personal and domestic habits is the most potent agent in the propagation of the evil.

3. That though, in large cities, the above characteristics are almost all uniformly present, yet in many country towns where there is an average amount of external ventilation, and fewer accumulations of *active* composts, the personal and domestic impurity is found to be abundantly efficacious in fixing on the skin the contagious or miasmatic matter, and thus propagating their corresponding maladies.

4. Sanitary suggestions deduced from the above.

I. It is a remarkable fact, that although the nature and laws of the propagation of plague, typhus, and cholera, three of the most fatal epidemics, differ in many important particulars, they establish their focus in the same localities, and among a similar population, showing most conclusively that there is some invincible attraction in this overwhelming vortex. Few persons acquainted with the history and progress of epidemics, will consider it necessary that we should here substantiate this point by evidence, as the facts are now so generally admitted as to be considered established maxims by the profession; but it may be remarked, that though the existence of plague in modern times has happily not furnished the inhabitants of Britain with an opportunity of examining this point, yet all history records the filthy state of the habitations of London during the existence of this epidemic; and in eastern countries, where it is often in active operation, the same peculiar characteristics are observed. Can this extraordinary coincidence be accounted for according to the ascertained laws of these several epidemics?

In the examination of this subject, it must be assumed that some material substance, however impalpable, refined, or imponderable, is the cause, in all cases, of the phenomena exhibited in these various diseases; for they, being precise and uniform in their laws, must be produced by an antecedent possessing the same qualities of stability and regularity; and it is not possible to conceive that the infinite varieties and combinations of heat, electricity, and moisture, or the ordinary fetid effluvia of large towns, could alone be productive of such uniform results.

We have besides, in smallpox, a disease analogous in its law of propagation to those under discussion, an example where the malady can be generated in two ways; viz. by the inoculation of a fluid cognizable by the senses, or by a visit to the chamber of a patient labouring under the affection. The deduction from this amounts to a demonstration, and proves that, if matter produced the disease in the one case, so in the other the agent must have been similar, although different in form, and only cognizable by its effects. This special quality in the contagion of smallpox warrants us to draw the conclusion, that the liquid matter of the pustules contains, as an element of its constitution, a volatile principle, or one which is capable of being exhaled. According to some chemical authorities, the matter of smallpox cannot be considered a truly volatile or vaporous body, because it can be dried without losing its qualities. It may be all very true that we cannot condense it into the same form after evaporation, as is done with water, alcohol, or ether, or even condense at all any of the peculiar matter in question; but we have nevertheless many instances where substances may be thoroughly dried, and yet contain a large portion of volatile matter, such as the various vegetables that contain volatile oils, as those belonging to the genus Mentha, Cassia, Lavandula, &c. There is, besides, the well-known analogy, that vaccine lymph soon loses its properties when exposed to the air, even though properly dried. It is not necessary in our argument to discuss the *questio vexata*, whether plague and typhus be contagious or not, since we have a material although imponderable matter generated, which is capable of being attracted as well as carried from one place to another. It may be proper, however, to state, that the word contagion, as in plague or typhus, implies that the virus is generated in the body of the sick; and that miasma, as in cholera, indicates a peculiar body supposed to exist in the atmosphere, generated by external or unknown causes, being stationary, or travelling by laws peculiarly its own. In what form this imponderable matter exists, whether infinitesimally divided and suspended in the atmosphere, like the pollen of flowers, or in some exceedingly refined gaseous condition, it is impossible to conceive. Hence many of its laws are very imperfectly understood; but analogy may guide us, to a certain extent, in explaining some of the phenomena. It is well known that gases are attracted and fixed in certain bodies, while other substances exert little or no influence; thus atmospheric air, carbonic acid gas, and ammoniacal gas, are soluble in various proportions in water, and the vapour of essential oils is soluble in alcohol. Odours of flowers and fetid effluvia are known to fix themselves in certain solids or fluids with more intensity than others; and various states of the atmosphere promote the development and perception of perfumes disengaged from plants. We have no experimental means of proving that odoriferous and fetid effluvia are regulated by the same laws of dif-

fusion and attraction as those of a contagious and miasmatic quality; but this may with safety be asserted, that they agree most materially in one remarkable point, in having affinities of definite kinds for certain substances. Having shown the high probability, both from fact and analogy, of an attraction existing for contagious and malarious effluvia, their mode of operation requires now to be examined. If, in a certain given space, such as the whole or a certain given quantity of a large city, there exists in the atmosphere the miasmatic effluvia of Asiatic cholera, and if this be at first equally diffused through the atmosphere, which we are warranted to assume, how does it generally select its victims from narrow streets, filthy lanes, and from small and ill-ventilated houses? Does the atmosphere of the district, contaminated by various species of emanations, attract a superabundant portion of it, or does it remain undilated from deficient ventilation? Either of these hypotheses will account for the retention of at least all its original strength; but the former alone, if it exist, can account for any particular concentration of its force in a particular locality. For it is quite obvious, that unless there was some peculiar attraction, each volume of atmospheric air which displaced and consequently succeeded its predecessor, would have precisely the same amount of impregnation. There must, therefore, exist attraction somewhere; for without its operation the whole population of the district would have to contend with the same measure of miasmatic influence. The same law of attraction applies to the contagion of typhus and plague; for though dilution may more readily render it innocuous, owing to its local generation, and not general existence in the atmosphere in large masses, as is now a rational belief regarding cholera; yet, if this attraction did not occur, its peculiar localization would not be so uniform, or be accounted for in a satisfactory manner. The remarkable similarity in the localization of these diseases, confers also on the arguments a conclusive probability, that if there be attraction in the one case, so must the same exist in the other.

II. Does this attraction, then, exist in the atmosphere of the district, or in the person or clothes of the person who resides in it? This is a question of some intricacy, and involves considerations of momentous magnitude. It is within the range of possibility, that an atmosphere impregnated with fetid exhalations may attract with more power than a purer air, what may be considered of a kindred nature, such as malarious or infectious effluvia, although we are aware of no facts which warrant such a supposition. With more apparent probability it may be imagined that stagnant pools, open sewers, dunghills, and cesspools, with other accumulated filth, may possess properties of this kind. Admitting this hypothesis to be correct, how can we explain the transmission of this absorbed effluvium to the bodies of the inhabitants—for ready attraction and spontaneous volatility are manifestly incompatible in the same sub-

stance—unless some special cause for the latter be in existence? It may be true, that a certain portion of what was originally fixed by the attraction may gradually be exhaled; but, according to analogy, this would be small unless some considerable decomposition was going on in the materials. Allowing, then, a certain influence to the attractive power of an atmosphere impregnated with fetid exhalations, and making all necessary concessions to cesspools and all varieties of accumulated composts, the difficulty is only partially accounted for. We cannot rationally explain the magnitude of the disturbance upon this theory alone. There are other circumstances, however, which may avail us in the solution of the problem. Reasoning *a priori*, and many speculative physicians have so reasoned on this subject, the respiratory organs would be considered the most natural receptacles for the impregnation of the system with the contagious or miasmatic effluvia; and though there can be no doubt that many gaseous, alcoholic, or ethereal agents act very rapidly through this channel, yet it ought to be conceded, that the morbid poisons which bear the strongest analogy to those under consideration act through the skin, such as smallpox, syphilis, yaws, glanders, &c., to which may be added the poison of serpents. Smallpox, however, is the only disease perfectly analagous that we can bring forward to bear upon the question, and being similar in too many points to be fairly disputed, may be adduced in illustration. This disease is communicable only in one way to an individual totally removed from the sphere of infection, and that is by inoculation; and the reception of variolous matter into the stomach will not produce the disease. If the skin, therefore, be the channel of its introduction by means of inoculation, it would be contrary to all sound reasoning not to assign the same route to it in the state of vapour or effluvia. Having then, the skin, with its extensive surface and innumerable pores, constantly in contact with atmospheric air, its great powers of absorption, and its extraordinary influence over the whole machinery of the system, with the numerous analogies of diseases and poisonous effects being communicable in this manner; why take the lungs into consideration as a principal actor, when the difficulty can be solved satisfactorily without their material intervention? In addition, however, to arguments such as have been adduced, we have the fact palpable and undisputed, that assailable individuals are in the large majority of instances unclean in their personal habits; or, to speak more plainly, that their skins are loaded with impurities, condensed from the perspiration and other sources.

If this state be the most constant in a locality where the disease is concentrated, it is plain that its operation in fixing the contagion or miasma must be great and important.

III. It is not alone, however, found in the narrow lanes and closes of a large city, but may be often encountered in country towns, where

the general ventilation of the thoroughfares cannot be considered bad; and even there the results are not greatly dissimilar. How, then, does an accumulation of impurity on the skin tend to attract and fix the contagion or miasma? The law of attraction may explain it. It has already been stated that various species of fetid emanations fix themselves in various substances, when brought within the sphere of their operation. Thus, according to M. Parent-Duchatelet, beef, wood, soup, water, are readily tainted with them. He also found that camphor, valerian, and tar, communicate their odour to water when it is exposed to their effluvia.[1] The tainting of several culinary substances in a similar way, is also well known. Analogy therefore warrants the belief, that accumulations on the skin from condensed perspiration, &c., may attract effluvia; of a contagious or miasmatic kind, and in this way present it in a form perfectly analogous to the ordinary process of inoculation, both as respects the material and the mode of its introduction. Wearing apparel, also, seems to be capable of absorbing the effluvia; and thus the miasmata of cholera, though not generally believed to be contagious, may be attracted both by the impurities on the skin and by the clothes, and in this manner be carried from one locality to another; although it is obvious, in this latter case, that unless the atmosphere of cholera be present in the locality, its range of operation, if such exist, must be extremely limited, and must speedily be exhausted.

The following conclusions appear to be warranted:—

1. That the poisons of plague, typhus, and cholera, are analagous to those of smallpox in their law of propagation.

2. That smallpox matter is composed of an effluvial substance condensed in an animal fluid, and that this affection is communicated in two ways; that it is propagated through the skin like many other poisons; and that it as well as analagous diseases spread most extensively amongst persons who are filthy in their habits.

3. That this can only be accounted for on the principles of attraction.

4. That the attraction of cesspools, sewers, *et hoc genus omne*, although highly probable, will rather tend at first to fix the effluvia, until disturbed by some internal operation or extraneous cause.

5. That all analogy and facts, derived from the spread of such diseases, in localities where no cesspools, dunghills, or decomposing composts of marked description exist, would lead to the conclusion, that the skin, clothing, and paraphernalia of the household are, in all probability, the most operative in the propagation of these epidemics; being the circumstances the most constantly present in the propagation of cholera, plague, typhus, &c.

The causes assigned in the foregoing remarks for the progression of epidemics among the lower classes, are in some respects different

[1] Annales d'Hygiene Publique, tom. v.

from those generally entertained at present on this subject; in as much as the various accumulations of organic impurities are here considered inferior in their influences to those which generally accompany them in the persons and houses of the inhabitants. It is far, however, either from our wish or intention to throw doubt or discredit upon any of our present sanitary practices or enactments, or difficulties in the way of any which may in future be projected. On the contrary, we wish the public to be persuaded, that the demolition of all the exterior defences of pollution will not effect enough, but that these sanitary measures must be carried into the interior of every household in the locality. It is true that great results must necessarily ensue from the removal of nuisances, the widening of streets, the more thorough construction and ventilation of houses, and the more regular supply of water; for in this way many dens of corruption will be swept away, others exposed to the criticising and correcting gaze of the public; while the example of all around them will either tend to amend their nestling habits, or scare them away to some other retreat, more suited to their own squalor and immorality. Can the legislature achieve much more than this? or would any interference with the domestic arrangements of private families be tolerated by the public, except in extreme cases? Let us, however, glance at the question for a moment. According to the law of nations, every one that trespasses upon or injures his neighbour, becomes by this act amenable to justice; and hence nuisances injurious to the health are prohibited, and decayed or poisonous food is interdicted from sale. Do the inhabitants of a district who concentrate and disseminate epidemic influences come under this category, and do they not inflict on their neighbours (unwittingly at present no doubt) an evil, which with the due appliances of cleanliness might have been warded off? Does it not carry along with it the similitude of an active volcano, that scatters ruin over its whole neighbourhood? and would it be an infringement on rational liberty to put a check to this, by the legal enforcement of domestic cleanliness? In solving these questions theoretically, the claim of the laws might appear equitable; but the social sufferings which would ensue would give it the reality of a cruel despotism. How could the law reach the squalid pauper clothed in rags; the sturdy labourer nestling in a pigsty, with a single coverlid for his body, and his half-naked children huddled into various corners of his apartment; or the heartless mechanic who squanders his ample wages in drunken revelries, and consigns his family to the precincts of pauperism? Laws similar to those of Lycurgus, or a government grant, could alone provide adequately for this portion of the gigantic evil, a scheme too chimerical for experimental solution. If, then, legislative interference would be considered an intolerable grievance, and a surveillance of domestic arrangement despotic in its character, as undoubtedly it would; and if, in addition to all this, adequate

habitations and clothing for the pauper, and those nearly overwhelmed in the vortex, must be provided, it may be asked what sanitary provisions can meet calamities of this magnitude? It would be vain, nay dishonest, to deny the difficulty of the position; but assuredly this great nation has resources within itself, which, if rightly expended, might tend greatly to ameliorate the evil. Two methods may be employed for effecting more or less improvement. 1. By example. 2. By private benevolence and philanthropy. It is well known that the regular use of the bath daily is by no means common in this country, and that those who use it regularly constitute the minimum proportion of the community. Were the question put to those who neglect this operation, why they bestow so much ablution on their face and hands, and systematically omit other parts of the body, few sensible persons could give any satisfactory answer. In truth, if the matter be reasoned upon at all, the conclusion is inevitable, that if it be useful and pleasant to have clean hands and a pure face, it must also be advantageous and agreeable to have a clean person. Frivolous excuses are the only arguments generally brought forward by those in comfortable circumstances; and how impotent these ought to be considered, when contrasted with the advantages of the practice to the general health and comfort of the individual, and in the prevention of disease! If it be true that the infinitesimal amount of cutaneous pores have been established for the performance of a vital and essential function, surely their obstruction, with sordes and condensed perspiration, must be productive of bad consequences; and no particular demonstration is required for the statement, that if there be no accumulation on the skin, there will be no nidus for the deposition of contagious or malarious matter. This salubrious practice, being once established among the respectable and middle classes, will speedily descend into the lower walks of life, and thus, like a stream, will produce a vivifying irrigation. But the question may be asked, how is water to be obtained by those verging on pauperism, and how is the clothing necessary for a regular change to be supplied? There seems to be only one method of supplying the first in sufficient quantity, viz., that the landlord, by legislative enactment, should be obliged to supply it to every house or tenement, and that the interest for this outlay should be included in the rent. Much also might be done by the proprietors of our now extensive and numerous factories, at comparatively small expense, in establishing baths for their workers; and thus they might find an indirect compensation for the outlay in their improved health, and by the induction of habits more regular and domestic. The efforts of private philanthropy can alone supply the deficiencies of the other. And why should not a portion of the means and appliances, which are scattered on all sides with such benevolent profusion for the real or supposed benefit of mankind, be bestowed liberally and sys-

tematically upon this important undertaking? There is generally no want of labourers in a field so full of promising results, for Europe is teeming with enthusiasm and outlets of this description, for which such a current is surely desirable. Such a scheme would prove not only great as a sanitary measure, but in moral power it would be equally conspicuous; filth and vice are twin brothers, being linked together like cause and effect; and no moral or religious crusade can ever have great influence in ameliorating the hearts of men who are clothed in rags, and overwhelmed with physical impurities.

ARTICLE IV.—*Case of Idiopathic Inflammation of the Tongue, with Remarks.* By A. C. SELKIRK, M.D., Carluke.

INFLAMMATION of the tongue is of rare occurrence in private practice, and rarer still as an idiopathic affection. It is met with frequently, in a slight form, as a sequela of the use of mercurials; occasionally, in an exalted degree, from the same cause. We find, in systematic writers, very little notice taken of the affection. The subject, however, is of importance in a practical view, and requires, in some instances, energetic treatment for its extinction. The rapidity of its development—the inflamed and congested organ occupying completely the mouth in a few hours—the difficulty of deglutition, and the alarming nature of the symptoms, call for active treatment on the part of the medical attendant. The indications to be fulfilled are the same as in the other phegmasiæ—viz., unloading the distended capillaries, and relieving the congested cellular tissue. The relief obtained from deep scarifications (apparently so only) on each side, and parallel with the median line of the tongue, and from back to front, is more marked than even in the case of inflamed tonsils, the bulk of the organ subsiding under the eye so soon as the scarifications are effected. The depth of the incision must be guided in some measure by the degree and intensity of the existing inflammation, and the relative size of the organ. The strangulated and engorged structures must be liberated, and the incisions which fulfil this indication look formidable when first performed; but in a few hours, on the yielding of the tumefaction, dwindle into mere scratches.

In the first case of this affection presented to my notice, in the year 1843, the patient, Mrs. H., a farmer's wife, was in excellent health at twelve o'clock on the morning of 2d July, and by nine o'clock in the evening the tongue had become tumefied so rapidly as to occupy nearly the whole cavity of the mouth. The symptoms present were, deglutition impossible, complete suppression of voice, slight dyspnœa, pungent pain indicated on inquiry by an inclination of the head. Two long incisions were practised on each side of the raphe of the tongue, and apparently to some depth; blood flowed freely from the wounds. In one hour the bulk of the organ was perceptibly lessened, and the patient enabled to utter monosyllables in a whisper. In twenty-four hours the disease had entirely subsided; and the lines of the incisions seemed as if effected by the gentlest touch.

Sir Benjamin Brodie, in the valuable practical lectures published in the *Lancet* in 1844-5, takes no notice of this formidable affection. Robert Liston, Esq., the late celebrated surgeon, refers to it as a sequela of mercurialization, and advocates the use of incisions in the treatment of the disease. I cannot remember one single instance on record in which the disease appeared as an idiopathic affection; nor do I recollect of having heard one sentence in regard to it during whole courses of systematic and clinical instruction, with the exception of a hint thrown out in the *Lancet*, so far back as 1828, and which I accidentally perused a short period previous to being called upon to treat the case above referred to. That the case in question was strictly of an idiopathic nature, the previous excellent health of the individual amply demonstrated. Mrs H. had been going about her usual avocations until one o'clock P.M., at which period she first complained of an "unusual stiffness" in the motions of the tongue. Pain and tumefaction followed so rapidly as to present the symptoms, elsewhere enumerated, by nine o'clock in the evening. Dyspnœa was not a prominent symptom, but had occurred to such an extent as to evidence the implication in the inflammatory action of the parts surrounding the superior portion of the larynx; and hence one danger of allowing the disease to proceed, even for a limited period, unchecked. That the disease had its origin primarily in the tongue, and radiated to the larynx and parts adjacent, is evident from the history of the case. There was no inflammation of tonsils or fauces previous to the accession of the attack. The dyspnœa was not remarked until the disease had existed four hours; but after that period the inflammatory action, extending to the larynx, had created difficulty of breathing, and had been increasing in intensity up to the moment the incisions were performed.

The benefit derived from the treatment was instantaneous—the pain, difficulty of breathing, and size of the organ, subsiding from the moment the incisions were effected; and in twenty-four hours the tongue had returned to its normal dimensions.

ART. V.—*On the Pathology and Treatment of the Deafness attendant upon Old Age; illustrated by Dissections and Cases.* By JOSEPH TOYNBEE, F.R.S., Fellow of the Royal College of Surgeons in England, and Surgeon to the St George's and St James' Dispensary, London.

THE majority of medical men seem to have arrived at the conclusion, that the distressing affection to which attention is about to be directed, depends upon a gradual and natural decay of the powers of the organ of hearing; and that it must in consequence be endured as a disease entirely beyond human control. The grounds usually assigned for arriving at this conclusion are, that as other organs of

the body are found to lose their power as life advances, it is only natural to anticipate that the ear should form no exception to that rule. Not content, however, with this view, it appeared to me desirable to ascertain what was the peculiar pathological condition attendant upon this diminished power of hearing, in order to arrive at some definite information on the subject. With this object in view, and with the persuasion that an accurate knowledge of disease must not only precede its successful treatment, but would also be of service in relieving the minds of sufferers from the continued discomfort resulting upon unsuccessful trials after relief; I have neglected no opportunity of investigating the pathology of this special form of deafness, and I now offer to the profession the results of my dissections and cases.

Before entering upon the pathological appearances discovered, it may be well to draw attention to the fact, that when we consider the great number of elderly persons who hear well, in comparison with those whose hearing is materially affected, the frequency of this disease in old age is not so great as may perhaps generally be supposed. The results of my experience tend to show, that this decline of the power of hearing in old age, is dependent upon the influences to which aged persons are frequently subjected; namely, the prolonged stay in warm and close rooms, the avoidance of the open air, the cessation from bodily exertion, the want of attention to diet, and to the healthy performance of the functions of the skin; and that it does not depend upon the decline of nervous power, or upon an atrophy of the tissues which compose the organ of hearing. On the contrary, an extensive field of *post mortem* investigation has demonstrated, that the *most frequent* pathological condition found in cases of senile deafness, is a considerable increase in the substance of the mucous membrane lining the tympanic cavities; and that the evidences of atrophy of the tissues are very rare. The pathological condition *second* in frequency in these cases, is a thickening of the *membrana tympani;* and the *third* consists in the presence of bands of adhesions, which connect together various parts contained in the tympanic cavity and these contents to the walls of the tympanum. The examination during life of elderly patients suffering from deafness, quite agrees with the results of the pathological researches. Thus, while the external surface of the membrana tympani remains smooth and shining, its substance is seen to be whiter than natural; upon attempting a forcible expiration with closed nostrils, air is heard by the otoscope[1] to enter the tympanic cavity, but it produces an unnatural sound: the hearing is generally worse during an attack of cold, and in dull weather.

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[1] An elastic tube twenty inches in length, each extremity having fixed upon it a piece of ivory or ebony: one orifice is introduced into the ear of the surgeon, and the other into that of the patient, while the latter attempts to make a forcible expiration with closed mouth and nostrils.

CASE I.—*A Lady, æt. eighty-seven, Deaf for Six Years previous to her Death; Bands of Adhesion in the Cavity of the Tympanum.*

Mrs L. died in Dec. 1848, at the age of eighty-seven, from a gradual decline of the vital powers. Until the age of fifty, had no symptoms of derangement of the ears, but about that period she was in the habit of taking "cephalic snuff," which she thought produced a buzzing sensation in the ears; the latter symptoms disappeared immediately that its use was relinquished. At that period there was no sign of deafness, but about the age of eighty-five she first perceived that the sense of hearing was becoming blunted. From that time, deafness gradually but very slowly made progress, attended now and then by noises in the ears. The deafness was temporarily increased by a cold, but when she recovered the usual amount of hearing remained. During the last year of her life the disease did not make much progress, but the patient was obliged to use a speaking-trumpet in conversation; it was observed that the sense of hearing was more acute during the last few days of her life. For the last nine or ten years this lady took very little exercise; was in the house, and sat in a warm room the greater part of the day. She suffered also from a chronic inflammation of the mucous membrane of the eyelids, lacrymal sac, and nasal ducts. It is important to observe, that this lady had two sisters, one of whom died at the age of ninety, and was nearly, to the time of her death, a very active person, she was slightly deaf; the other sister, still living, aged eighty-nine, who has been confined to her room for some years, is extremely deaf.

DISSECTION I.—Right Ear. *The External Meatus* was dry, and did not contain any cerumen.

Membrana Tympani. The fibrous layer is white and slightly thickened; it is more concave and tense than natural.

Tympanic Cavity. The mucous membrane appears to be healthy and not thicker than natural, but there are bands of adhesion which connect together the ossicula, and the ossicula to the wall of the tympanum. These adhesions may be divided into two distinct sets; one is placed between the incus and the inner wall of the tympanum and the stapes; the other connects the head of the malleus and the body of the incus to the external wall of the tympanum. The first-named set consists of two portions, one of these measuring half a line from above downwards, and three-quarters of a line from without inwards; connects the anterior part of the long process of the incus to that part of the inner wall of the tympanum which is posterior to the fenestra ovalis (see Fig. 1).

Fig. 1.

Fig. 2.

Fig. 1. *Dissection of an ear taken from a deaf lady, who died at the age of eighty-seven.* a, *A branch of adhesion connecting the posterior part of the long process of the incus to the inner wall of the tympanum.* b, *A second band connecting the anterior part of the same process of the incus to the stapes and tympanum.* c, *Two bands connecting the head of the malleus and the body of the incus to the outer wall of the tympanum.—Fig.* 2. *A magnified view of the bands* a *and* b, *which connect together the incus, stapes, and inner wall of membrana tympanum.*

This band is connected by several small ones to the superior surface of the stapes, and also to a fine membrane which extends between the two crura of the latter bone. These small bands are seen in Fig. 2, and they are so firm and tightly stretched between the stapes and the larger band, and between the incus and the inner wall of the tympanum before mentioned, as to keep the stapes more fixed than natural. There are also adhesions between the upper surface of the crura of the stapes, and the margin of the *fossa fenestræ ovalis*. The other part of this band extends from the posterior part of the long process of the incus to the inner wall of the tympanum, posterior to that just described; this band is also firm and tense. The second set of adhesions passes from the head of the malleus and body of the incus directly outwards, connecting them to the osseous walls of the tympanum superior, and posterior to the attachment of the membrana tympani. It is interesting to consider what effect these bands must have had upon the ossicula and membrana tympani during life. Those which surround the stapes, and connect it to the *fossa fenestræ ovalis*, must have had the effect of impeding the movements of the stapes; and those which connect the long process of the incus to the inner wall of the tympanum, by pulling that process inwards, would probably have been the means of pressing the stapes (attached to its lower extremity) towards the vestibular cavity. The effect of the adhesions between the bodies of the malleus and incus and the outer wall of the tympanum, seems to have been to draw those extremities outward, and in consequence their inferior extremities inwards; this action upon the malleus is apparent, and may account for the greater concavity of the membrana tympani, which is carried inwards with the long process of the malleus, and is very tense. The base of the stapes is apparently fixed more firmly than natural to the margin of the fenestra ovalis. The membrane of the fenestra rotunda is in a natural state. The membranous labyrinth is healthy; there is rather more black pigment than usual in the cochlea of the left ear; in other respects it is like the right ear, and presents adhesions connecting the ossicula.

DISSECTION II.—*The Mucous Membrane of the Tympanum very thick; Mucus in Tympanic Cavity.*

Mr S. T. died at the age of eighty-seven from the effects of an accident; he had been deaf, especially in the left ear, for a considerable period previous to his death.

Right Ear. The meatus externus is distended with cerumen; the other parts of the ear are healthy.

Left Ear. The meatus externus is full of cerumen. The membrana tympani is smooth on its surface, but very white. The mucous membrane of the tympanum is very thick, and the cavity contains a large quantity of dense and tenacious white mucus, which is also found in the mastoid cells. The presence of this fluid, and the thickened state of the internal coat of the membrana tympani, explain the white appearance of the latter. The stapes is entirely concealed by the thick mucous membrane.

DISSECTION III.—*Bands of Adhesion in the Tympanum.*

Mr B. C. died of asthma at the age of eighty. He had been very deaf for a considerable period previous to his death. Both ears presented numerous bands of adhesion connecting the membrana tympani with the inner wall of the tympanum, and by completely surrounding the ossicula the membrana tympani is nearly concealed. The Eustachian tube is healthy throughout.

In this case only a small quantity of air could have entered the tympanic cavity, and upon its ingress only a slight puff or crack would have been produced.

DISSECTION IV.—*Mucous Membrane of Tympanum very thick.*

Mr J. R. at seventy-three died of consumption, and had been deaf for a considerable period previous to his death.

In each ear the mucous membrane lining the tympanic cavity was so thick as to conceal the stapes, and to form so thick an investment for the other ossicula as to prevent their natural form being recognized.

Dissection V.—*Disease of the Membrana Tympani, Mucous Membrane of Tympanum and Bone.*

Mrs L. E. died of apoplexy at the age of ninety-four; she had been deaf for several years in both ears.

Right Ear. The membrana tympani is entirely gone, but a thick and soft portion of it lies in contact with, and adheres to the promontory. The malleus has been removed by disease; but the incus remains attached to the stapes, which latter bone is entirely concealed by the extremely thick mucous membrane. The lining membrane of the mastoid cells is also very thick; and the upper osseous wall of the tympanum is diseased and soft.

Left Ear. The membrana tympani is thick and opaque, especially at its posterior part. Its upper surface is firmly adherent to the inner wall of the tympanum and the stapes; the latter being entirely concealed by adhesions. The upper osseous wall of the tympanum is in several places carious, presenting large orifices, which allow the dura mater to be in contact with the thick mucous membrane of the tympanum and mastoid cells. One of these orifices measures three lines in length and two in breadth. The posterior part of the pars petrosa presents a large exostosis. The labyrinthine humours are very small in quantity, and the membranous labyrinth is much atrophied. The base of the stapes projects into the cavity of the vestibule.

Part Second.

REVIEWS.

On Femoral Rupture; its Anatomy, Pathology, and Surgery. With a new mode of Operating applicable to Cases of Strangulated Hernia generally; with Plates. By John Gay, F.R.C.S.E., &c. 4to. Pp. 97. London: 1848.

This treatise, as its title implies, consists of three several departments. 1st, The descriptive relative anatomy of the parts concerned in femoral hernia. 2d, The pathology of the disease, its surgical treatment, the various plans of operation usually followed for its relief, and the statistics of operations for hernia. 3d, The author's proposal for a new method of operating. We shall therefore notice these different departments *seriatim*.

The numerous investigations and descriptions of the anatomy of the parts of femoral hernia, which, about the beginning of the present century, emanated from the press both in this country and on the continent, led, in many cases, to what may be termed an excess of minuteness in subdividing and giving different names to parts which naturally, and for all practical purposes, formed one continuous structure, thus greatly complicating their description; whilst the habit which then existed, of prefixing the name of the

discoverer to the structure he had described (a very doubtful honour), gave rise to a most useless and absurd style of nomenclature; such as the fascia transversalis of Cooper, the crescentic margin of Burns, the ligament of Hey, and a host of other names which, in themselves, indicated no anatomical relation or position, and which afterwards not unfrequently led to discussions as to which was the structure really meant. The results, therefore, of these investigations and minute descriptions of the parts of hernia, whilst they conferred great benefit on surgery by directing attention to the subject, and elucidating some most important facts, left, in our opinion, one desideratum unfulfilled; namely, that some person well acquainted with the literature of the subject, and accustomed to the dissection of the parts, should, dismissing all preconceived notions, write a simple, straight-forward description of his own dissections; preserving in these dissections the various structures, as far as possible, in their natural connexions, instead of artificially subdividing them; and eschewing all nomenclature which had not a strictly anatomical value. By this plan, we believe, the subject would be simplified, the essential practical points brought prominently forward, and rendered easy of comprehension to the student, instead of being, as at present, a bugbear to him, owing to the numerous, complicated, and often conflicting descriptions which give rise to the difficulties that confuse him.

Such being our views, we confess ourselves disappointed with the anatomical department of this work; for though no one who reads the present elaborate treatise, can doubt that the author was well fitted, from his knowledge of the subject, for carrying the plan we have suggested into execution, yet he has not done so; and, whilst his anatomical descriptions evince great care as well as laborious research into the descriptions of others, they are not given in that simple demonstrative style which is desirable; and by the number and extent of the foot-notes, and references to these from the text, the latter is rendered confused and difficult to follow. Instead of aiming at simple description, our author allows himself to be led into a sort of critical analysis of apparently conflicting opinions regarding certain structures; of this we have an instance in his discussion as to what is meant by the femoral ligament of Hey, whether it be the falciform edge of the fascia lata, or a distinct structure lying deeper? This might readily have been spared, by reading the preface to the edition of Mr Hey's Surgery, published in 1810; for we there find Mr Hey's own explanation of the term femoral ligament, as follows,—"Having cleaned the parts, I introduced my finger into the sheath; and carrying it downwards, on the inner side of the vein, till it appeared below what is now called the lunated or falciform process of the fascia lata, I took notice where the stricture upon my finger was the greatest. In doing this, I found the anterior edge of the thin projection of the aponeurosis of the external oblique muscle, to coincide or be continued with the falci-

form edge of the fascia lata. This part I called the femoral ligament." From what he there says, it seems evident that the structure he referred to as causing constriction, is formed by the deep layer of the falciform process, which, ascending behind Poupart's ligament, connects itself with the reflected edge of that ligament, shelves obliquely backwards, and, curving on itself, passes downwards and inwards, to become the crescentic margin of the falciform process; thus forming the deep or inferior crural arch which is in immediate contact with the neck of the hernial protrusion. Accordingly, when Hey's ligament is described by some as identical with the falciform process, the mere superficial falciform *edge* is not meant, but the deeper lamina and twisted prolongation of the same structure. But we are falling into the same fault as that we complain of in our author; and therefore stop short, merely remarking in passing, that the simple and comprehensive statement of the disposition of the lower crural arch given by Mr Liston, which our author quotes, and which we have verified in its main points in a very large number of dissections, might serve as a model of anatomical description. He explains that the iliac portion of the fascia lata, together with its falciform process, is formed by the union of the deep layer of the abdominal superficial fascia with the fascia transversalis, below Poupart's ligament; which ligament lies enclosed between these two laminæ, above their angle of junction, so that the combined fasciæ referred to, form in fact the resisting portion of the arch which is in contact with the neck of the hernial sac. The only point of description in which Mr Liston's views are defective in regard to these structures is, that he did not keep sufficiently in view the strong attachment of the deep layer of the structure to the posterior reflected edge of Poupart's ligament, and the tension consequently exerted upon that ligament by the fascia lata in certain positions of the limb.

In describing the cribriform fascia, Mr Gay says,—

"The cribriform fascia is oval shaped, partly reticular and partly membranous, situated within the saphenous opening, and stretched upon that portion of the walls of the sheath of the femoral vessels which remains unprotected by the fascia lata. It has, by some anatomists, been considered as a perforated portion of the fascia lata, which is obviously an error; for this fascia, as we have seen, after forming the edges of the saphenous opening, is reflected outwards upon itself;—by others it has been regarded as a 'deep lamella of the superficial fascia;'—whilst, by a third party, it has been confounded with the femoral sheath itself, and termed its cribriform. portion. I have been led to dissent from these views, and to concur with the opinion of those who look upon the cribriform as a distinct lamina, having peculiarities of structure and function which entitle it to be so considered."

We must confess, that of all the points in the description of the anatomy of femoral hernia, nothing has puzzled us so much as what can be the reason for such diversity of description and views as to the cribriform fascia; for, after many years' experience in dissect-

ing and demonstrating these parts, together with careful examination of them for our own special satisfaction, we have not been able to see any reason for describing this cribriform fascia otherwise than as a continuous portion of the fascia lata, always presuming, of course, that no artificial dissection is made to separate the natural connexions of the parts. If the subcutaneous superficial fascia be carefully raised, commencing over the saphena vein, and the fat be cleaned away with the handle of the scalpel, so as to expose the saphenic edge, the falciform process, and the fascia lata generally, we see between the falciform edge above, and the saphenic edge inferiorly, a continuous layer of fascia stretching from the iliac towards the pubic portion of the fascia lata; thinner, perhaps, and reticulated by foramina for vessels, but still perfectly continuous with the two great portions of the fascia. If the handle of the knife or a probe be passed under the saphenic edge, and pressed downwards, it is resisted in its progress, showing that here also, though the edge is abruptly reflected, the membrane is continuous, and above, at the falciform process, where the reflexion is less sharp, the continuity can be seen. It is only by artificial dissection that we can destroy this continuity, and make a distinct saphenic opening as it is called. As to the description of its being a portion of the superficial fascia of the thigh, that is evidently incorrect; for the saphena vein, the superficial branches of the common femoral artery, and their accompanying veins, together with the superficial set of femoral glands, form a natural separation between that fascia and the cribriform. As little do we see any reason for considering or describing the cribriform as a distinct structure, for any such distinction must be purely artificial, and we confess we cannot appreciate our author's reasons for so describing it; namely, "that after forming the edges of the saphenic opening, it is reflected outwards on itself." This statement appears to us not exactly correct. The iliac portion of the fascia lata forms reflected edges only at two points of the boundary of what is termed the saphenic opening, namely, at the saphenic edge inferiorly, and the falciform edge superiorly; in the intermediate space, the fascia passes directly across from the iliac toward the pubic portion of the fascia lata, applied upon the femoral vessels, and we have already stated how the continuity of the saphenic and falciform edges with the cribriform portion may be traced; but, even admitting that the iliac portion of the fascia lata was reflected, as Mr Gay describes, that would not prove its non-continuity with the cribriform. Is not the whole surface of the serous pericardium continuous, and yet its reflexions at the points of exit and entrance of the great vessels, form margins as abrupt and distinct as the saphenic or falciform edges of the fascia lata?

The second department of the work, or that which treats of the pathology and surgery of femoral hernia, is deserving of all praise. Here the author's research and anxiety to furnish information by

reference to every source—which, according to our views, rather interfered with the simplicity of his anatomical descriptions in the former part of the work—contribute essentially to render this portion highly interesting and valuable. It contains, within very moderate compass, a large amount of exact and useful information; and although we cannot agree with Mr Gay in all the conclusions which he draws from the facts established, still we have derived much interesting information from the perusal of this department of the work, and especially from the statistical tables and the remarks upon them. We shall take an early opportunity of laying before our readers some of these details in the *Retrospect;* meanwhile, as we have yet to examine the third and most original portion of the work, we can at present only afford space to quote our author's conclusions as to the general causes of death in cases of strangulated hernia.

"I am led to conclude that death, in cases of strangulated hernia, is caused either *directly* or *indirectly*.

"1st. Directly; by, (*a*.) The shock of the accident, in the ratio of about 1 to 5. (*b*.) The lesions, physical or organic, which the strangulated parts sustain, in the ratio of about 1 to $2\frac{1}{2}$.

"2d. Indirectly; by (*c*.) Trusses or other compression, which have the effect of thickening the neck of the sac, without obliterating its orifice. (*d*.) Delay in affording relief by operation. (*e*.) The injudicious use of the taxis. (*f*.) The injudicious use of bloodletting. (*g*.) The injudicious administration of tobacco. (*h*.) The simultaneous existence of diseases in remote organs. (*i*.) The operation.

"The indications of treatment which these results afford, are briefly as follows:—

"(*a*.) Stimuli, and immediate relief; the latter by operation, if the remedies should not succeed in rousing the exhausted powers. (*b*. and *h*.) The early relief of the strangulated parts by operation, as soon as the judicious use of the preliminary means shall have proved unavailing. (*c*.) The avoidance of all unnecessary pressure on the hernial sac. (*d*.) The judicious choice of a *few* preliminary measures, their moderate employment, and non-repetition. (*e*.) The most gentle handling of the parts on all occasions. (*f*.) The avoidance of bloodletting, in all but those cases in which it is manifestly demanded. (*g*.) The abandonment of this remedy in every case where the operation will be submitted to. (*h*.) A more simple operation."

We now come to consider, in the last place, the new plan of operation proposed by our author as less dangerous than those at present practised; and with this view, we shall in the first instance quote his own words descriptive of the principle and steps of the operation. After referring to the dangers of the ordinary methods of operating, he proceeds:—

"The principle of that which I now propose to substitute for it, is non-interference with the hernial tumour. The incision, by which the seat of stricture is to be reached, is made at the farthest possible distance from the protrusion, and through tissues free from the results of morbid change. For the first step of the operation, two imaginary lines should be drawn *from the point directly over the* inner side of the neck of the hernial tumour; one to the tuberosity of the pubis; the other down the thigh, along the inner side of the tumour. These two supposed lines will include an angular space, in which, and about mid-

distance from either, the *first incision* is to be made. This incision should commence just within the angle formed by the meeting of these lines, and be carried inwards and rather downwards, for the extent of an inch or little more; and should divide the integuments and superficial fascia over the *femoral fossa*.

"For the second part of the operation, the forefinger of the left hand (if the hernial tumour is on the right thigh, and *vice versa*) is to be passed through this wound, upwards and outwards, along and close to the side of the hernial tumour, to its neck. Should any of the deep laminæ of the superficial fascia which occupy the femoral fossa obstruct its passage, these must be divided. The blunt point of a bistoury caché, guided by the tip of the finger still in the wound, is to be passed *through* the cribriform fascia (one of the sheath-like apertures in which it will readily enter), and the delicate *vaginal sheath*, which alone intervenes between it and the crural canal. It is now to be directed upward, through the *canal* to the *ring*, where it usually meets with resistance from the seat of stricture. By the least amount of force, and with the aid of a little gentle compression of the inner side of the tumour by the finger, the point of the bistoury may be insinuated between the *sac* and the pubic margin of the *ring;* the edge of the knife is then to be turned towards the pubes, and by projecting the blade, the seat of stricture in that direction may be effectively divided. Should the surgeon, however, be disposed to direct his incision forwards, it may be done either before or after the incision of Gimbernat's ligament with equal facility. It will be obvious, from a reference to the anatomical relations of the parts, that in its course from the femoral fossa through the cribriform fascia, crural canal and ring, the knife will pass successively behind the falciform process, the edge of the lower orifice of the canal, Hey's ligament, and the fibres of the inguinal ligament of Hesselbach, and thus be made to command which ever may happen to be the particular seat of stricture. By taking care not to keep the knife too close to the side of the hernial tumour, after its point has passed the cribriform fascia, its point would glide between the sheath and the deep layer of the iliac fascia lata; and in this way it would pass *between* the *ring* and the *last portion of Hey's* and *Gimbernat's ligaments*, and thus a division of these structures might be effected from the *outside* of the sheath or canal.

"If, after the division of the ring and parts on its pubic and front sides, the hernial tumour is not to be replaced without the employment of force, the stricture cannot have been dilated; and the surgeon would then have to contemplate the possibility of its being situated in the fibres of the septum crurale. The still constricted neck of the sac would in that case be felt, and it would be for the surgeon to get his knife cautiously behind the constricting fibres. This could be most easily done by such a knife as that I am in the habit of using, which has a beak flattened in the direction tranverse to the edge—by gently pressing the side of the tumour outwards immediately below the seat of constriction, so as to gain room for its introduction.

"Should the seat of stricture be in the margin of one of the apertures of the cribriform fascia, which would in all probability be discovered at an early period of the operation, the means for dilating it would be analogous to those which have just been detailed. If, after the division of these various textures external to the sac, the hernial tumour should still remain constricted, the external wound would have to be enlarged, and the seat of stricture looked for either in the neck of the sac, or within the sac and amongst its contents.

"The operation just detailed is most simple and easy of execution; requires but few, and those small incisions; leaves the hernial tumour and its diseased coverings untouched; permits, if necessary, the immediate use of a truss; and, in an otherwise healthy subject, requires but a few days for the perfect recovery of the patient. A single attentive examination of the parts in the dead subject, is sufficient to ensure competence in the performance of this operation."

If we comprehend this description, and the position of the incision as indicated in Plate I. and Plate III. Fig. 2, it is intended

that no part of the hernial protrusion, nor of the parts over its neck, shall be exposed to view during the operation; but that the finger, introduced through a wound in the integuments, made at some little distance from these parts, shall serve to feel the resisting membranous structures, and to guide the knife or bistoury caché for their division. Now this really appears to us a very doubtful and haphazard mode of operating, and, in our opinion, exposed to greater risks than any plan we have ever heard of, with the exception of that by subcutaneous incision, which it very closely approaches. Every surgeon accustomed to hernial operations, must have often encountered difficulties arising from alteration of the superimposed structures, and other causes, and felt the necessity for careful dissection of the parts exposed to view by his incisions. And though, in our author's description, the bistoury caché seems to glide with great ease, and almost intuitively, first, into one of the larger openings of the cribriform fascia, and then between the neck of the sac and the constriction; yet we know from experience, that in operations for strangulated hernia, it is generally no easy matter to get the point even of a very small bistoury insinuated between the stricture and the neck of the sac, and that ere this can be done, the margin of the constricting fibres often requires to be raised with the forceps, and carefully divided or notched with the edge of the knife.

If such difficulties occur, and such caution be necessary, even when the parts are fairly exposed, we leave our readers to judge how far the operation will be facilitated, or its risks diminished, by the plan proposed by Mr Gay, where the surgeon must depend entirely on the sense of touch to guide him in dividing the stricture. As regards the evil our author proposes to avoid by this plan, namely, the bad effects of dividing what he terms the diseased coverings of the tumour, we can only say, we have never met with any trouble from this cause in our own cases; and though the coverings of herniæ are often so far altered in structure as to be matted together or condensed, they are not often in a state which would render their division peculiarly hazardous. In cases where we have known erysipelas to have appeared in the wound, it has been when that disease was prevailing generally; so that it would just as likely have attacked the incision made by Mr Gay's plan, as the other. Besides, in ordinary cases, it is never necessary that the incision be extensive: if the wound in the integuments be merely sufficient to expose fairly the upper part of the protrusion, and the structures over its neck, so that the surgeon may see what he is doing, there can seldom be any necessity for interfering much with its deeper coverings, except at the constricted part; and those cases in which the superimposed tissues are so far altered as to deserve the term "diseased," are the very cases which would necessitate freer incisions, that the operator might make sure of the actual condition of the parts, and so be enabled to decide as to what plan

for dividing the stricture was best suited to the particular case, and whether or not he might require to open the sac; for all admit, that in cases of hernia it is impossible to predict from mere external examination what may be the actual condition of the protrusion, or exact seat of stricture. Again, though Mr Gay says "that his plan is adapted to all cases where the seat of stricture is external to the sac," we think, that from its uncertainty it might even bring that mode of operating into undeserved disrepute; for in some of the cases of division of the stricture external to the sac, the operators have found, after the more distinct resisting fibres were divided, a tense fibre or two surrounding the neck of the sac, so deeply indented as to appear as if the constriction were actually in the sac itself, which required to be divided ere the bowel could be returned into the abdomen. Three instances of this condition of parts will be found recorded in the numbers of this *Journal* for March and April 1848; two among Dr Duncan's cases of hernia, and one in Mr Spence's. Now, in such cases, there would be risk in operating by Mr Gay's plan, that, after the resisting fibres of the crural arch had been divided, and the femoral canal thereby enlarged, a small femoral hernia might be pushed back "en bloc," with these fibres still constricting its neck. We have therefore come to the conclusion, that the risks far counterbalance the supposed advantages of our author's plan of operating, and consequently cannot look upon it as an improvement on the present methods; and we believe the majority of the profession will agree with us in thinking, that in operations like that for hernia, it is better that the surgeon should see, as well as feel, what he is doing.

Although we have been obliged, after a careful perusal of Mr Gay's treatise, to dissent from his views as to the operation in femoral hernia; we feel bound at the same time to say, that the work, considered as a whole, is most creditable to him, as exhibiting great perseverance and elaborate research, and conveying much interesting and useful information. J. S.

School Chemistry, or Practical Rudiments of the Science. By ROBERT DUNDAS THOMSON, M.D., Lecturer on Chemistry in the University of Glasgow, &c. &c. London: 1848.

An Introduction to Practical Chemistry, including Analysis. By JOHN E. BOWMAN, Demonstrator of Chemistry in King's College, London. London: 1848.

Rudimentary Chemistry, for the use of Beginners. By GEORGE FOWNES, F.R.S., Professor of Practical Chemistry in University College, London. London: 1848.

Chemistry, as Exemplifying the Wisdom and Beneficence of God. By GEORGE FOWNES, F.R.S., Professor of Practical Chemistry in University College, London. Second edition. London: 1849.

As many of our readers have occasion to provide themselves with works on chemistry at this season of the year, we take this oppor-

tunity of recommending to their notice the treatises named at the head of this article, all of which are very moderate in price, and well worth perusal.

Dr R. D. Thomson's School Chemistry will be the most interesting of the whole to practitioners and students of medicine; although its title might readily lead them to consider it the one least concerning them. The book, in truth, has a twofold aim; viz., to furnish a text-book suitable for the instruction of the youngest persons at all competent to take an interest in chemistry; and likewise to supply a manual, such as shall enable the professional student, and especially the student of medicine, to follow University lectures. At only half the cost of the excellent treatises of Professors Gregory and Fownes (*Manual of Elementary Chemistry*), it discusses sufficiently and fully all the important departments of chemistry. The style is simple and perspicuous; the experiments suggested are well devised; and the mode in which they should be performed is made manifest, by a plentiful use of woodcuts illustrating apparatus and manipulation.

In addition to its substantial merits as a text-book, its pages are lightened by a variety of curious, and frequently amusing details, to be found in few other treatises on chemistry. These curious notices, however, do not occupy the room of solid information, but are given in addition to it, and are therefore to be considered as decidedly enhancing the value of the volume.

In one respect, Dr Thomson's work is peculiar, and we owe it to our readers to warn them of the peculiarity. The "School Chemistry being intended," as the writer himself informs us, "as an Introduction, in the College of Glasgow, to the System of Chemistry by Dr Thomas Thomson," the equivalents, or atomic weights of the elements, are represented according to the scale of numbers preferred by Professor Thomson, which makes oxygen 1. This materially limits the value of the work, as all the other teachers of the country employ the scale which makes hydrogen 1, and oxygen 8. One series of numbers is no doubt as good as another for representing the combining proportions of chemical substances; but it throws a formidable obstacle in the way of acquiring and retaining knowledge when different scales of numbers are employed by different teachers. Confusion is constantly occurring in the statement of temperatures, from the employment of the dissimilar Centigrade and Fahrenheit graduation of the foreign and British thermometers; and blunders are made in the translation of works from French or German into English, and *vice versa*, where weights are expressed in the one case in grains, and in the other in grammes. On matters like this, the majority plainly give the rule; and we observe that, in the last edition of his "Elements of Chemistry," Professor Graham has abandoned the scale which makes oxygen 100, and adopted that which makes hydrogen 1. Should Dr Thomson's work reach a second edition, as we hope it will, he might at least compromise matters by printing at each substance its equivalent, according to the hydrogen as well as

the oxygen scale. We have had occasion to know that the absence of this, has prevented his work from being purchased by those who were otherwise satisfied of its excellence. The concluding portion of the work, which is devoted to animal chemistry, forms one of the best brief digests of the subject we have yet seen.

The scope of Mr Bowman's work is different from that of Dr Thomson's. It treats of chemistry as an art, not as a science; and aims at instructing the student in the methods of chemical manipulation, and the modes of analysis. It is the best introductory work on the subject with which we are acquainted.

It is a very pretty specimen of typography, moreover; and we are perplexed to understand how so great a number of beautiful woodcuts can be furnished at the small price charged for the volume. A glossary of chemical terms is added as an appendix. The definitions contained in it are unusually happy.

Dr Thomson and Mr Bowman make large use of chemical symbols in illustrating decompositions. For this they have both been called sharply to account, by able journals which have reviewed their works. The accusation is to the effect, that chemical symbols are a kind of short-hand, very suitable for the initiated, but not requisite for the beginner, whom they will only scare by their unattractive character. The charge preferred, implies a total misapprehension, on the part of the critics, of the nature and value of the chemical symbols.

They are a chemical short-hand, no doubt; but they are something more. One part of their value consists in their enabling us to substitute single letters for whole words, as the arithmetician substitutes ciphers for written numerals. The composition of a body can thus be expressed much more distinctly to the eye, and decompositions can be followed more clearly, than if the names of substances were written out at length.

This, however, is the smaller part of their value. They are not mere abbreviated substitutes for words. They supply information which the word does not give. Thus the name 'Nitric Acid,' tells us only that the compound contains nitrogen and oxygen; but the symbol or formula NO^5, not only tells us this, but also that the acid contains one equivalent of nitrogen, and five equivalents of oxygen. The word 'Ammonia' tells nothing; but NH^3 shows us both the nature and the proportion of the elements of that compound.

We consider it accordingly, not a fault, but a great recommendation of the volumes under notice, that they employ the chemical symbols, and insist on teaching them to the youngest student from the very first. Mr Bowman states chemical changes in the form of equations; a method which only gives the results of a reaction, but does not exhibit the steps by which the result is reached. Dr Thomson illustrates the stages of each reaction, in addition to giving the results as equations. His diagrams, however, are far from being so

distinct as they might be. The fault is a good deal the printer's; nor can it altogether be remedied by any concise method of printing. Dr Thomson's diagrams, we have no doubt, are quite intelligible on the black board or slate where the symbols are first put down; then the lines indicating decomposition and combination; and finally, a second set of symbols showing the result. But, in the printed page, both sets of symbols and the connecting lines appear together, and the diagram becomes confused.

We must charge against both the writers, as a defect which should be remedied in succeeding editions, that they do not explain the laws of combining proportion, so that the student can learn what is signified by the term chemical equivalent or atomic weight. Treatises which employ symbols, are self-contradictory if they do not teach what chemical equivalents are; for the symbol is a sign, not of the chemical substance merely, but of an equivalent or equivalents of it.

Mr Fownes' little work, "Rudimentary Chemistry," is a still more elementary treatise than those just noticed. It is intended for the youngest class of students of chemistry, and is admirably suited for its object.

The Actonian Prize Essay, our readers will observe, has reached, as it deserved, a second edition. There may be differences of opinion as to the mode in which Professor Fownes has disposed of the difficulties in the theological part of his argument; but there can be but one judgment as to the merits of the work, considered as an illustration of the way in which chemistry furnishes an argument in proof of the existence of an allwise and benevolent Creator.

Transactions of the American Medical Association. Instituted 1847. Vol. I. 8vo. Pp. 403. Philadelphia: 1848.

THIS Association has laid its plans on a grand scale, and is evidently determined to achieve a high position in the United States. It seems to us desirous of commanding in that country the influence enjoyed by the Royal Academy of Medicine in France, whose constitution and mode of conducting business it has to a certain extent adopted.

There are several obstacles to the attainment of this influence in America, in the absence of that close connexion with the government which gives an official character to many of the acts of the French Academy; and more especially in the want of a large central capital, where the meetings would be always held, and in which, as in Paris, nearly all the medical talent of the kingdom congregates. As in general politics, so in medicine, Paris represents France, and the discussions of the Parisian Academy are referred to as the index of French medical opinion.

The American Association migrates annually from one town to

another; but although on this account it cannot acquire the power of a central stationary body, it may yet be of the highest utility in improving the members of the profession, and in advancing medical science. In our own country, the Provincial Medical and Surgical Association has contributed powerfully to these important ends.

The Transactions begin with the "Report of the Committee on Medical Sciences," under which title we are presented with a very imperfect *resumé* for the year, in the preparation of which the authors have evidently been much indebted to the published retrospects. Surely the compilation of an abstract is not the object contemplated by the Association in nominating this committee, or what advantage is to be gained by referring to it the endless task of reporting on the medical sciences?

There are many subjects which may be very advantageously referred to committees, but they must be of such a character that their details may be completely investigated within a limited time; and we refer with pleasure to the excellent report of the Chloroform Committee of the Edinburgh Medico-Chirurgical Society, in illustration of what may be effected by a committee when entrusted with a suitable subject.—(See *Monthly Journal* for December 1847.)

In the second report, that on Practical Medicine, the authors, feeling the difficulty of doing justice to so extensive a theme, have wisely confined their attention to the subject of fever; and here the reader will find a considerable amount of useful information.

The next paper on the treatment of Œdema glottidis by scarification, is an excellent contribution to practical medicine. We have given an analysis of it in last month's *Retrospect*.

The best report is that of the committee on Surgery, which is indeed a very creditable performance. We quote the following returns of the results of lithotomy practice in America, as they show an almost unprecedented success:—

"For the cure of stone, the cutting operation still continues to be that mostly resorted to, and the lateral operation with the gorget is believed to be the procedure most commonly employed.

"In the last account of the practice of Dr Dudley which has reached us, it is stated that, up to the beginning of 1846, he had operated upon 185 cases of stone—of which number 180 are reported as successful. This remarkable result, according to Dr Bush, cannot be attributed to any selection of cases on the part of the operator, since out of 188 subjects presented to him 185 were cut. Dr B., who furnishes this report, ascribes these results to the thorough preparation of the general system made by Dr Dudley preparatory to the operation, an account of which was detailed some years since, in a paper published by him in the Transylvania Journal, and which we can only here refer to.

"From communications that have been made to the committee, it appears that Dr Marsh of Albany has operated by the lateral method seven times, all of which were successful.

"Dr Mettauer of Virginia states thathe has operated by lithotomy on seventy-three cases of calculus, two of which proved fatal. One from prostatic hemorrhage, and the other from the occurrence of spasm of the ileum.

"Dr Jno. C. Warren has operated upon thirty cases, of whom two died;

one of these lost his life by an error in diet, the other had a purulent effusion, owing to the great size of the stone, and the force required to extract it. The mode of operating in his fifteen first cases was by the lateral incision and the gorget; in the thirteen following by the knife; and in the three last by the bi-lateral method.

"Dr Eve of Georgia has operated eight times, including one female, all of which were successful. Dr Mussey of Cincinnati informs us, that he has cut thirty-two patients for stone, all of which cases have been successful but two."—P. 161.

From a table furnished by the Pennsylvania Hospital, it appears that of 83 cases of lithotomy, which it is believed was invariably performed by the lateral method, and except in a few instances of very young children, by means of the gorget, 72 were cured, 10 died, and 1 is set down as relieved.

The chapter on anæsthetic agents is very complete, and includes several returns from public institutions, displaying the results of operations with ether and chloroform. They confirm the conclusions of Dr Simpson in his paper on *Etherization in Surgery*.—(*Monthly Journal*, 1848.)

We have quoted largely from the report on OBSTETRICS in the monthly *Retrospect* for December 1848.

The report on *Medical Literature* is very ably executed, but it is open to the same objection that we have already urged against the report on the *Medical Sciences*. We fully agree with the committee in awarding to the "American Journal of Medical Sciences" the first place in their *journalistik*. The long standing of this publication, the support which it has received from many of the best writers in different parts of the country, and the elevated literary character and strict impartiality which have distinguished it, have rendered it the favourite organ of the profession.

In the long list of its contributors, we observe the names of many whose reputation is well established on this side of the Atlantic. Its pages record many of those daring operations which are dwelt on with so much pride by the American surgeon, and contain numerous highly valuable memoirs in all the departments of medical science.

The comparative dearth of original medical works in America has long been matter of surprise; the more so when we consider the number of its medical men, and the very superior opportunities for education which they enjoy. A much larger proportion of American than of British students obtain the advantage of one or two years' residence at the continental schools.

"It cannot be denied that the great *forte* of American medical scholarship has hitherto consisted in 'editing' the works of British authors. The committee are not disposed to disguise the fact, that this business has been carried on in a very cheap and labour-saving fashion. A tacit alliance between writers and publishers has infused the spirit of trade into the very heart of our native literature. The gilt letters of the bookbinder play no inconsiderable part in the creation of our literary celebrities. Sometimes the additions by the 'American editor' have been real and important, oftener

nominal and insignificant. The following calculation of the proportion added to different recently published works, taken at random, will show the average amount of material so contributed. The editor's proportion was in two instances one-fourth; in two more, one-eighth; in one, one-ninth; in another, one-tenth; in others, one-fifteenth, one-seventeenth, one nineteenth, one-twentieth, one-twenty-eighth, one-fifty-ninth, one-sixty-fifth, one-ninetieth, one-hundred-and-seventh, and in one instance such a sprinkling as a single penful of ink might furnish, and leave enough to spare for a flourishing autograph. The fairest fruits of British genius and research are shaken into the lap of the American student, and the great danger seems to be that, in place of the genuine culture of our own fields, the creative energy of the country shall manifest itself in generating a race of *curculios* to revel in voracious indolence upon the products of a foreign soil!"—P. 286.

The report on the *Indigenous Medical Plants* is very imperfectly executed; but this is owing more to the difficulty of the subject, and the short time allowed for its investigation, than to any want of ability on the part of the members of the committee. The inquiry is a most fertile one, and if pursued in a proper spirit, and the report prepared so as to embody the independent experience of each member in relation to the physiological and therapeutic action of the medicines, is, of all the subjects discussed in the volume, the best adapted for successful investigation by a committee. They must confine their attention, however, to at most three or four plants, and make their examination into their properties as accurate and complete as possible. We trust that the Association will earnestly pursue this inquiry. The reader will find the substance of the present report in the *Retrospect* which accompanies last Number.

A short paper on "*A new feature in the Anatomical structure of the Genito-urinary Organs*," by Dr Buck, and another on "*Ophthalmitis postfebrilis*," by Dr Dubois, conclude the volume.

We do not approve of all the plans of the Association, more especially we object to the nature of some of the subjects referred to committees for report; but we can scarcely doubt that these errors are now quite apparent to the members themselves, and will be avoided for the future. On the other hand, in this the first volume of its Transactions, we have found much to admire, both in the solid usefulness of its matter, and the superior style of the writing. The Association aims at a high standard of excellence, and we predict for it an honourable and useful career.

On Cancerous and Cancroid Growths. By JOHN HUGHES BENNETT, M.D., F.R.S.E., &c., Professor of the Institutes of Medicine, and one of the Professors of Clinical Medicine, in the University of Edinburgh.

THE readers of the *Monthly Journal* for the last two years do not require to be made aware of the claims of Dr Bennett on their attention as an original inquirer. His long-continued devotion to the

subject of this volume is already well known to the public through the medium of our pages; many of the facts and opinions on which the present work is founded having been published in our Journal during the period when it was under Dr Bennett's able superintendence. But we cannot allow the opportunity to pass without bearing our testimony to the merits of this inquiry in its present complete form; and of gleaning from it, for the benefit of our readers, a few of its more important and novel deductions. Since the publication of the work of Müller, we are aware of no contribution from original investigation to the pathological anatomy of cancer, which is at all comparable to the present in the importance of its results to science and practice. The author has taken account not only of his own laborious observations, but of the investigations of all the recent writers on this subject, and particularly of those inquiries, founded on minute structural examination, which have lately occupied so great a space in the journals of Heidelberg, Vienna, and Prague.

The first part of this volume is entirely occupied by the detail of Dr Bennett's own observations. As the general character of these is so well known to our readers, we advance to the second part, which treats of cancerous growths, and those which may be mistaken for them, under the heads of Histology, Chemistry, General Anatomy, General Pathology, Statistics, Diagnosis, Prognosis, and Rational Treatment.

In the first three of these chapters the whole of the growths to which the term cancer has been, or may readily be, attached, are described with reference to their external characters and minute structure and composition. Dr Bennett finds, like other observers, that the name *malignant* has been assigned, on practical or anatomical grounds, to a great variety of structures possessing different characters and elementary composition. He applies to the fibrous, epithelial, cartilaginous growths, &c., the name cancroid, first used by Lebert, and anticipates great additional accuracy in the recording of future observations and facts from attention to this distinction. He reserves the name cancer for the three varieties of scirrhus, encephaloma, and colloid, which present somewhat similar anatomical elements, and which have in common the character of spreading from a centre, involving and destroying all the normal tissues around them.

The external characters ascribed by Dr Bennett to these three descriptions of cancer are not very different from those of his predecessors in the inquiry, particularly Müller and Walshe. As regards the minute structure, they possess a great variety of histological elements, most of them individually identical with those of pathological or even normal structures; a fact which has induced Müller and other pathologists to maintain, that cancer contained no characteristic elements at all. According to Dr Bennett, however, while no *individual* element can be said to be characteristic in all circumstances of a cancerous growth, a particular *combination* of elements *is* characteristic. These essential elements are a fibrous stroma, a cell possess-

ing certain definitive characters, and a viscous fluid. "It is the relative amount of each of these which determines its peculiar form. If the fibrous element be in excess, it constitutes scirrhus or hard cancer; if the cells be numerous, encephaloma or soft cancer; and if the fluid abound and be collected into loculi or little cysts, it is called colloid cancer. All these forms of cancer may frequently be observed in the same tumour."—(P. 172.)

This leads us to the consideration of the so-called *cancer cell.* Although Müller declined to pronounce any cell characteristic of cancer, he described those which were found in it, and, among others, fusiform and caudate cells, which were long supposed by those who did not examine for themselves to be the characteristic cancer cell. It is now known that such cells occur in many structures, and that cancer may, or may not, present them according to circumstances. Lebert, to whose investigations a great deal of our knowledge on this subject is undoubtedly due, described as characteristic of cancer a large cell, having a nucleus with nucleoli, the outer wall of which became faint on being treated with acetic acid. That Lebert's description of the cells generally found in cancer is correct, is now generally admitted; but in imagining such cells to be never found but in cancer, he was in error. In fact, the tendency of pathologists to discover every where *specific* cells, has produced more of those hasty generalizations, and unaccountable contradictions, for which the microscope has generally been blamed, than any other cause of error; and we are happy to find a histologist of the experience of Dr Bennett expressing the following opinion, with which we entirely concur:—

"There is no kind of cell formation which, at all times and under all circumstances, is capable of being distinguished from every other form of cell growth. Whilst this statement may be received as the expression of a fact of great importance, it is no less true that very characteristic differences may exist among cells, the study of which is of the utmost service in distinguishing one tissue from another. These differences principally depend upon the age or state of development, the situation in which the cells are formed, and a variety of concomitant circumstances, all of which are necessary to take into account before an accurate opinion as to their nature can be arrived at. It is the more necessary to point out this, because the inexperienced are very apt to suppose that a glance through the microscope, at any particular form of cell, is sufficient at once to determine the existence or absence of cancer."

According to Dr Bennett, the cancer cell, though not absolutely in itself characteristic, may occur under circumstances where it becomes so. The cells most liable to be confounded with it are those of epithelium and cartilage in particular stages of growth. The characters of each of these cells are most instructively given in the work before us.

In the chapter on General Pathology, various questions as to the origin, growth, and degeneration or decline of cancer are discussed. Dr Bennett considers cancer to be produced by an exudation from the bloodvessels of the *liquor sanguinis*, which, as in simple inflam-

mations or exudations, is afterwards transformed into cells, fibres, &c. The cause of the difference between simple and cancerous exudations is believed by Dr Bennett to lie in the constitution of the blood itself,—a view, we think, much more exclusive than can be successfully defended. The views upon the decay of cancerous growths, and their cure by conversion into cicatrices and concretions, which were explained by the author at a meeting of the Medico-Chirurgical Society of Edinburgh (*Monthly Journal*, March 1847), are here reproduced and somewhat matured; but, curious and interesting as they are, we cannot enter into the discussion of them at present.

In opposition to Lebert, Dr Bennett believes that cancroid growths may be originally simple, and may assume afterwards the real cancerous character.

In the diagnosis of cancer during life, the author thinks that the use of the microscope, superadded to the other means of diagnosis at present in use, will effect the most beneficial changes. The microscope *alone*, however, cannot do much. On this subject, we extract the following remarks from the preface :—

"The author has endeavoured to inculcate, that the microscope *alone*—that is, independently of all other kind of observation—can seldom determine in the living subject the presence or absence of cancer. At the same time he feels himself bound emphatically to declare, that he thinks it capable of being as serviceable to the surgeon in cases of morbid growth, as the stethoscope is to the physician in cases of diseased heart or lungs. Neither instrument is infallible; both require to be studied in an especial manner; both demand long practical experience, and judicious reasoning power; and both require to be conjoined with all the aids to be derived from other modes of observation. With the stethoscope, it is not that the crepitating râle in pneumonia, or the mucous râle in bronchitis, differ from similar râles which accompany tubercular disease; but that these signs, *conjoined with other symptoms*, clearly establish the diagnosis. So, likewise, it is not the recognition, by means of the microscope, of certain cells and fibres, which will enable us to assert with certainty the existence of cancer; but that their detection in particular places, and accompanying peculiar forms of growth, permits us to do so."

Dr Bennett recommends in some cases of tumour the employment of the exploring needle, recommended by M. Küss of Strasbourg, with a view of removing small portions of the tissue to allow of microscopic examination. He remarks, however, that "those who are not habituated to the use of the microscope should be very cautious of arriving at conclusions by its means."

The treatment of cancer is discussed in the last chapter of this work; and the unsatisfactory results of empirical practice have only the effect of inducing Dr Bennett to look for a treatment based upon pathological views. He is not without hope, that by making ourselves better acquainted than hitherto with the origin and cause of the disease, and the methods by which a natural cure has been in a few instances effected, we may be able to arrive at some more successful principle of treatment. Whether the hints which he throws out in this chapter will conduce to such a result, we know not;

but we recommend them to the earnest consideration of our readers.

The local remedies which the author thinks applicable to cancer are such as retard the growth of cells in general, especially cold, dryness, and pressure. The last he thinks warranted by experience as well as theory, judging from the cases of Recamier, Bayle, and Walshe. He decidedly objects to all lotions or poultices, on the ground of their favouring cell growth, and therefore the progress of the disease.

The alleged constitutional remedies for cancer are, in Dr Bennett's opinion, all equally ineffective; nor does he appear disposed to suggest any new specific. He thinks that cancer and tubercle are opposed to one another, inasmuch as the latter is essentially deficient in fat, and is generally ameliorated by the injection of fatty substances; while the former is a growth abounding in oily matters, and is probably to be counteracted by the removal of fatty matters from the blood. But this is offered by the author as a purely hypothetical suggestion; and we doubt very much the correctness of the reasonings on which it is founded.

The remarks upon the extirpation of cancerous or apparently cancerous tumours, are undoubtedly the best and most impressive of all the practical considerations in this chapter. Dr Bennett here appears as the decided champion of excision, not only in doubtful, but in decidedly cancerous cases; and we think the arguments he employs ought to induce all surgeons who profess an extreme unwillingness to operate in cases of malignant growth, carefully to re-consider their opinions. Many surgeons believe that the truly cancerous tumours are incurable by operation; and that the operation may even hasten the fatal result. Even admitting the first of these positions to be true, Dr Bennett has satisfactorily shown that epithelial and enchondromatous growths will run the same destructive course as cancer; and we are afraid that there is little doubt of the correctness of Dr Bennett's idea, that the timidity or caution of surgeons as to operating, has allowed many of these pseudo-malignant growths to run a fatal course when their progress might readily have been arrested. This should suggest to surgeons who hold the above views as to the incurability of cancer, the absolute necessity of employing every means of diagnosis before refusing to operate, and not merely trusting to external appearances. But the principle of the incurability of cancer by operation appears open to most grave objection; there being cases which prove that operations, even after repetition several times, have not been destitute of ultimate success; and this principle is still more applicable to cancroid growths.

"The practical rule, then, which pathology and experience unite in causing us to adopt, seems to be this, *that so long as a cancer remains fixed in a part which is capable of being removed, and the strength of the patient is not too much reduced, so long is the surgeon warranted to interfere.*"

We recommend the above "practical rule" to the attention of sur-

geons, and also the following paragraph, which appears to contain a most important suggestion, calculated in our opinion to effect most important and useful changes in the practice of surgery :—

"Do any rational means, therefore, remain to us whereby it is possible to discover why cancerous growths return more frequently than cancroid ones, and if so, should they be put in practice? To both these questions I have no hesitation in answering in the affirmative. When a fibrous, fatty, or encysted growth returns after excision, it is universally attributed to the whole of the diseased parts not having been removed. May not many of the cases of cancerous growth which return, be attributed to the same circumstance? May not the success which follows amputation of cancerous testicles depend on the completeness with which, from their position, they may be separated from other textures? Germs, altogether invisible to naked sight, may be infiltrated among a tissue, which, instead of being excised, is only divided by the knife. This happened in Obs. XLVI., in which I found the muscles in the neighbourhood of the tumour so loaded with cells, that at the time I prognosed the return of the disease. Surgeons, however, up to the present time, have acted on the principle, that their sense of touch and naked sight, are means sufficient for determining whether the whole of a growth has been removed. That this is a complete fallacy must be acknowledged by every histologist. New researches, therefore, are necessary. Not only should the tumours themselves be carefully investigated after extirpation to ascertain their nature, but the neighbouring tissues should be examined, to determine whether they are entirely free of cancer cells and nuclei. The microscope ought to be a necessary instrument in the operating theatre, and every suspected tissue examined on the spot, before the lips of the wound are closed. It may be stated that this is impracticable; but when it is known that, proper precautions being taken, an expert histologist might make such examination in the short space of thirty seconds, and that several demonstrations could be accomplished in a few minutes, or while ligatures are placed on the vessels, it will I trust be acknowledged, that there is nothing very embarrassing in the proposition."

We have rather given an account of this work than criticised it. It contains many most valuable observations, and doubtless is not free from errors, both of observation and reasoning. Some of these we think we can already discover; but they can be met only by renewed observations and facts, by which the subject may be advanced another step, and set in a yet clearer light. For this the time has not yet come, seeing that no one has appeared before the public in connexion with this subject who is in a position to judge this work from a point of view gained by superior observation; on the contrary, the author has carried his subject to a height to which few can follow him.

We cannot fail to remark, however, in perusing this work, the exclusiveness with which the author pursues the ideas suggested by his own method of inquiry, and the indifference which he displays towards the results of collateral methods. Admitting that the observations here recorded go far to establish new, and abolish old distinctions in the *pathological anatomy* of tumours, the bearings of which on science and practice cannot fail to be of the greatest importance; we nevertheless cannot allow these distinctions at once to form the basis of definitions so entirely remodelled as to make a

tabula rasa of our former experience, and oblige us, microscope in hand, to begin the world anew. The practical surgeon—who calls that tumour *cancer* which stretches out on every side (crab-like) its straggling limbs, and from gland involves and destroys skin, and cellular tissue, and muscle, and bone—who calls that tumour *malignant* which, wherever situated, is accompanied by diseased glands and sallow complexion, the manifest evidences of a tainted constitution—will not readily surrender these practical and important ideas to any considerations derived from minute structure. If an enchondromatous or epithelial growth fulfil these conditions, he will call it accordingly; and although it may be important to him to know that different structures are confounded under his comprehensive terms, yet he will do well not to give up his definitions until he is satisfied that the substitutes are, in actual practice, productive of more accurate results; he will rightly judge that time is required to determine how far his diagnosis, formed from glandular enlargement and cachexy, must yield to that from cells and fibres, and how far the contrary must be the case.

Dr Bennett seems to demand, in the chapter on statistics, that all past numerical details in regard to cancer shall be swept away, to make room for a series founded on the new pathology. We object to this proposition for several reasons. 1st, We think that the old statistics suggest *some* important considerations, which, though not very numerous, or in a very accurate form, are yet in the most accurate form attainable, and are far more easily corrected from new data than the vague results of opinion. 2dly, We think that the new pathology is not likely, for a long time at least, to furnish statistical data in any degree more positive than the old; and we decidedly object to the small specimen in the work before us, inasmuch as we find added together the cases of Lebert and Sédillot, who judge of cancer from the cell alone, and those of Dr Bennett, who denies the possibility of so forming a judgment; and, accordingly, the cancroids of the one observer may be the cancers of the other, and *vice versâ*. 3. We believe, that for some time at least, the diagnosis of cancer will remain, under the new pathology, as under the old, a matter of opinion as much as of observation; and, should it become general, we believe that the comparison and contrast of the old and new statistics will furnish far more important results than either separately.

It is not possible to close a review of any English work on cancer, and more especially to speak of the statistics of that affection, without reminding the reader, that in Dr Walshe's most admirable and complete monograph, he has every information that can be desired upon the literary and statistical inquiries connected with cancer. But the labours of so many observers have been directed to its pathological anatomy within the last few years, as to have entirely remodelled the subject; and we believe that those who would enter upon the observation of this subject with a good and trustworthy guide, cannot do better than study the work of Dr Bennett.

W. T. G.

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVIII.

MEETING I.—*Wednesday, November* 15, 1848.—Dr ROBERT HAMILTON, President, in the Chair.

ANÆSTHETIC MIDWIFERY.

A COMMUNICATION from *Dr Simpson* was read, containing a lengthened refutation of various arguments brought forward against anæsthetic midwifery by Dr Ashwell, Dr Meigs, &c. &c., on the ground that the pains of labour were physiological, and hence the anæsthetic practice unnatural. Dr Simpson showed, that, if these arguments against the employment of anæsthesia in the function of parturition were really founded in truth, they would, if applied to all the other functions of the body, alter all the present practices of civilized society, and virtually send us back to the customs of savage life. If applied, for instance, to the function of locomotion, they would necessarily lead to the abrogation of travelling by horses, coaches, railways—in fact, by all modes of progression except nature's primitive method, viz., walking; and so on with respect to all other functions. If the pains of labour should be endured, and not averted because it is "physiological," then, on the same grounds, we should keep ourselves naked, like the New Hollander or original American, because clothes are unnatural, and improperly (in this mode of reasoning) avert the endurance on our part of the physiological heat and physiological cold which nature has formed us to feel. He showed that almost all the innovations constituting the practices and luxuries of civilized life, had been at first opposed on the same grounds; such as the introduction of optical glasses, coaches, tea, &c. &c. &c.

As to the supposed medical dangers from chloroform, Dr S. stated that it had now been employed in Edinburgh for a year in all surgical operations, and in a large amount of midwifery cases;—many thousands in all had been chloroformed here;—and not any kind of accident or drawback had occurred. Probably as many doses of opium, calomel, antimony, &c., would not have been so innocuous.

THE CHOLERA.

A short verbal statement was made by *Dr Christison* on cholera, relating particularly to the epidemic at Loanhead. The disease broke out in that village very suddenly, and with great virulence; the houses affected being clean, dry, and in airy situations, the inhabitants cleanly in their habits, and in comfortable stations in life. Thus far, therefore, the epidemic at Loanhead was an exception to the usual mode of the diffusion of cholera. Dr Christison said that much more was to be gained from the examination of the disease in such small places as Loanhead, than in large towns, where the quantity of material was so great as to render results extremely difficult of attainment.

Dr Robertson communicated to the society the results of his experience in the Cholera Hospital, which were embodied in his "Account of the Practice in the Cholera Hospital," in the *Monthly Journal* for December 1848.

MEETING II.—*Wednesday, December* 13, 1848.—Dr ROBERT HAMILTON, President, in the Chair.

ON THE POSITIONS OF THE FŒTUS IN UTERO, AS RESULTING FROM THE ACTION OF THE REFLEX SYSTEM. BY PROFESSOR SIMPSON.

[Half of this memoir appeared in our January Number; the rest will be published in March.]

CONTAGION OF CHOLERA.

In the course of a conversation on this subject, *Dr Simpson* stated some curious facts, the result of inquiries made by him in Newhaven, near Edinburgh, as to the outbreak of the cholera. It is well known that the first case of cholera which appeared in Edinburgh was on Wednesday, the 4th of October last. On the Wednesday before this, three pilots from Newhaven went down to the Isle of May to look out for vessels. One of them was taken on board a vessel from Cronstadt bound to Leith. The other two remained in their boat on the lee side of the vessel, and were towed to Leith, a distance of four or five and twenty miles; both of these men were seized with diarrhœa on their passage. On arriving at Leith they went on board the ship. One of them died on Sunday of cholera. The vessel had left Leith, and the existence of cholera on board had not been ascertained. During the next eight or ten days, several cases of cholera occurred among the relations and immediate neighbours of the pilot that died, and these were the first cases in Scotland. If cholera was owing to an epidemic influence merely, whether terrestrial or atmospheric, it was strange that this influence should have passed westward from the continent of Europe, should have run for many miles up the populous banks of the Forth, and never attacked any village or district till it came to Newhaven; and it was still more strange that it should have attacked a pilot and his immediate neighbours first. Dr Simpson referred to the case of the smack Trusty in 1832, detailed in his paper on the Contagion of Cholera (*Edin. Med. and Surg. Jour.*, 1837). The disease broke out on board the vessel on the passage from London, where cholera prevailed, to Leith, where there was none at the time; and it not only affected extensively the crew of the Trusty, but also the pilot who joined the vessel in the Frith of Forth, and a Leith porter who remained on board after the vessel was placed in quarantine; and not only these, but also the nurses of those of the crew who were treated on board *another* vessel, which was strictly prohibited from all other intercourse with the affected one, or with the shore. These facts Dr Simpson regarded as unassailable proofs of the contagious nature of the disease.

MEETING III.—*Wednesday, January* 3, 1849.—JAMES SYME, Esq., President, in the Chair.

ADDRESS OF THE PRESIDENT.

The *President*, upon taking the chair, spoke to the following effect:—He expressed his sense of the honour which was attached to the occupation of that chair; and his gratitude for the kind feeling of the members of the society which had led them to place him in it. He said that the best, indeed the only, return he could make was, to exert every effort in his power to promote the objects for which the society was instituted. These, he understood, were assisting in the accumulation and diffusion of medical science, increasing the good understanding and fellowship of the members of the society, and maintaining the honour and dignity of the profession. With regard to the first of these objects, it appeared from their past history that many communications, distinguished by talent, research, and original observation, had been contributed; and, although they had ceased to publish their Transactions in a separate form, the records of medicine contained abundant evidence of the novel views and important facts which had been made known through their meetings. But, however much they might have already done, there could be no doubt in the mind of any one acquainted with the composition of the society, that it had never yet fairly put forth its strength; and that it possessed a latent power, which, if duly exercised, was adequate to the accomplishment of much more than had hitherto been achieved. He here particularly alluded to the great benefit that might be derived by themselves and their brethren in the country from the communication of detached cases occurring in the

ordinary course of practice, together with the exhibition of morbid parts concerned. Instead of any comments of his own upon this subject, he preferred reading an address, which had been circulated among the members by Dr Abercrombie while he was president of the society, feeling sure that the sentiments of one whom they all so much esteemed and respected must have much more weight than any thing he could say in favour of the proposal :—

"In such a society every one feels the difficulty of preparing regular communications in which any subject may be discussed in a full and connected manner :—some have not the necessary leisure, and others may imagine that they are not possessed of matter of sufficient value. But every member must often meet with facts of the greatest importance, and from which the most valuable deductions might be procured, provided an extensive collection of such facts could be viewed in connexion with each other. One of the causes, indeed, which retard the progress of medical science, appears to be the want of opportunities for such extensive comparison of individual facts ;—and from this source, probably, have arisen many of those partial inductions, which have, from time to time, been proposed with much appearance of truth, and soon found to be fallacious.

"Such an association of well-educated, zealous, and scientific practitioners, as compose the Medico-Chirurgical Society of Edinburgh, presents an opportunity, rarely to be met with, for attempting a plan, by which, on the principle now referred to, important benefit might result to medical science. In prosecuting it, no lengthened or studied communications are required ; but single cases simply and shortly detailed,—or even single facts, derived from cases which in other respects may present no particular interest. Such observations will, in the first place, be valuable by giving rise to conversation, in the course of which additional facts may be communicated. I would then propose, that these short communications shall be carefully preserved and arranged ;—that they shall be reviewed, from time to time, by select committees, and the facts arising out of them classified and generalized ;—and that, when thought expedient, portions shall be published, with the names of the contributors attached to the individual communications, and with observations by the committees pointing out the general conclusions which arise out of the whole."

Although communications of the kind alluded to had been occasionally introduced at their meetings, they had never constituted more than a secondary part of the business. The reason of this might have been the existence of a Pathological Society in Edinburgh, but as it had some time ago ceased to exist, there could be no objection to the plan being fully developed, and if they were to meet once a fortnight instead of monthly, the alternate evenings might be devoted to this purpose.

As to the second object of this society, there could be no question that the most certain way of accomplishing it, was through the successful attainment of the one just mentioned, since nothing could tend more to the establishment of a kind and friendly feeling between the members, than frequently meeting together for their mutual instruction. They all knew the effects of a good dinner in smoothing asperities and soothing animosities, and it seemed fair to anticipate a similar effect from mental repasts, at which they might all derive satisfaction from a common source.

In order to maintain the honour and dignity of the profession, they had of late years heard much of royal charters and acts of parliament ; but he must confess, that he altogether distrusted such means, either for elevating themselves, or for keeping down the presumptuous individuals who ventured to undertake the treatment of disease without having passed through the schools. In his opinion, a college of physicians or surgeons had no legitimate claim for respect, except what was founded upon the character of its members, and the assistance contributed by them to the advance of their respective departments of medical science. He saw no use in attempting to effect a classification of the profession, which the public assuredly always would, and alone could do.

And instead of attempting to suppress quackery by penal enactments, he thought the more reasonable course was to elevate their profession in the estimation of the public, so as to satisfy them that it was for their own advantage to avail themselves of services rendered safe and efficient by study and experience, in preference to those of ignorant and designing empirics. He was glad to find these sentiments meet with such a cordial reception; and expressed his fear, that there were other medical communities in which they might not have proved quite so palatable, since the members of the profession who were least scrupulous in degrading it, were ever the loudest in demanding legal protection. In London, it appeared that an association had been lately established under the specious title of "medical protection," with the object of compelling the payment of bad debts; for which purpose, they had provided the permanent services of a lawyer, who, seated in his office like a spider in his cell, was always ready to pounce upon any victims who should become entangled in the net that had been ingeniously woven for them, by the monthly publication of a printed list, containing the names of all the lords, ladies, and gentlemen who had changed their residences without paying their doctor's bills. He believed that there were no tradesmen however mean, of any craft however low, who would not reject such a contrivance with scorn and indignation. And yet, he observed, that next to squeezing out the money, the object most cherished by these associates, was increasing the stringency of legal enactments for protecting the dignity of the profession!

But to pass from such painful subjects, he had much pleasure in adverting to the advantages which they possessed as medical practitioners in Edinburgh. They enjoyed the best opportunities of cultivating their profession and its collateral branches. They had among them the greatest medical school, not only of this country, but of Europe, which had every prospect of an increasing prosperity, and must give life and energy to every medical institution connected with it. But best of all, they maintained a social position singularly favourable to freedom of sentiment and liberality of conduct, inasmuch as they were not pressed down by *stratum superstratum* of wealth, rank, and title, but practised a profession which stood so high in public respect, as to prevent them, in their social intercourse, from ever suffering that painful sense of inferiority in station which in other metropolises had probably in no small measure contributed to the backslidings of their brethren. He thought, that the possession of all these advantages rendered it peculiarly incumbent upon them to exert every effort in their power for the good of the profession; and he trusted that the Medico-Chirurgical Society would always prove a powerful instrument for this purpose. In conclusion, he begged their indulgence for the imperfections of his services, in discharging the duties they had confided to him; and expressed his hope that he might be able to afford them as much satisfaction as the amiable and excellent gentleman who had preceded him.

ANÆMIA AND GOITRE—ARE THEY RELATED? BY JAMES BEGBIE, M.D.

[This paper is printed in our present Number.]

In the course of the conversation on Dr Begbie's paper—*Dr Christison* stated that he had observed a case which bore the strongest resemblance to those related by Dr Begbie. There was marked anæmia, although its cause was not apparent; and the affection of the thyroid gland, together with the prominent eyeball, and somewhat maniacal expression, were exactly as Dr Begbie had described them. Iron had been used, but not persevered in, owing to its causing headach.

CASE OF SPONTANEOUS CURE OF OVARIAN DROPSY, BY MEANS OF AN ULCERATIVE OPENING INTO THE BLADDER. BY DR J. H. BENNETT.

[This communication appears in our present number.]

The President expressed some doubt as to whether a certain diagnosis was attainable in this and similar cases of abdominal tumour. He had seen so many

instances in which the diagnosis proved fallacious as to the nature of such tumours, that he was in the highest degree sceptical, and in some cases would not admit any evidence short of a post-mortem examination.

Dr Bennett thought the diagnosis sufficiently made out in this case; the situation and relations of the tumour at an early period of its growth, when it was examined by Dr Simpson, having been characteristic.

Dr Spittal thought that the means now employed in the physical diagnosis of ovarian tumours, enabled us in many cases to pronounce as to their existence with perfect confidence.

MEETING IV.—*January* 17, 1849.—Dr BEGBIE, V.P., in the Chair.

[This was the first pathological meeting of the society convened for the purposes alluded to in the address of the President at last meeting—the detail of individual cases, and examination of morbid parts.]

CASE OF VARIOLA, IN WHICH THE ERUPTION WAS FOUND ON THE MUCOUS MEMBRANE OF THE COLON.

Dr George Paterson read a case of smallpox occurring in a boy five years of age, unvaccinated—which proved fatal on the fourth day of the eruption—death having been preceded for about twenty hours by discharges of blood from the bowels, nose, and mouth, and the appearance of masses of ecchymosis beneath the integuments of the lower limbs. He exhibited a drawing of the appearances found in the colon after death. There had been pustules, greatly resembling those met with on the skin; and superficial incrustations, most abundant, and running together in the lower part of the intestine. Although most medical authorities were opposed to the opinion, that smallpox could exist upon the mucous membrane of the intestines, it was maintained, and cases in support of it recorded, by Sir Gilbert Blane, Rostan, Froriep, and others. And when we consider the analogies between mucous membranes and the skin—the fact pretty generally admitted, that smallpox pustules can occur on certain of the mucous membranes; and the additional fact, which some of the very impugners of an appearance such as the present allege, that they have been met with on membranes entirely different in their nature, as the serous coverings of the liver and spleen—there seems no good reason to doubt the possibility of their occasional occurrence in the course of the intestinal canal. It must be admitted that the appearance is extremely rare; but this may be accounted for by the isolation of the mucous membrane of the bowels from the external air; and still more, on considering the secretions and other matters which are more or less constantly in contact with its surface, and are calculated to modify the appearance, endanger the integrity, and so prevent the maturation of the variolous pustule. In the present instance, death occurred at so unusually early a period of the case, that there had probably not been time for these causes to produce their usual results—either by obliterating the pustule altogether, or by converting it into an ulceration, which, when met with, many will not allow to be other than the mucous follicles themselves inflamed and ulcerated.

Dr W. T. Gairdner (who had made the post-mortem examination along with Dr Paterson) said, there could be no doubt that the structure of the pustules in this case was identical with that of the variolous pustule. The variolous matter was always deposited, when in the skin, in the lower layers of the epidermis, and between it and the corion, which was only superficially ulcerated. In mucous membranes, the matter was thrown out between the epithelium and the submucous layer, and such was the case in the present instance; for, on removing the crusts, the injected submucous layer was seen flat and unbroken, excepting by a few minute points of superficial ulceration. This superficial form of disease could never be mistaken, as had been alleged, for the affection of the solitary glands, which invariably extended through the whole depth of the submucous layer. Dr G. considered this an unequivocal instance of variolous eruption in the colon.

Dr Bennett observed, that the experiment of inoculation with the matter taken from the pustules would, in such a case, be the most satisfactory evidence of its true nature.

DISTORTION AND DISLOCATION OF THE FORE-ARM THE RESULT OF A BURN.

Dr R. J. Mackenzie showed, in Mr Syme's absence, a dissected preparation of an arm, which had been amputated by Mr Syme a fortnight previously. The patient, a respectable young woman, aged twenty-two, had a severe burn of the arm when six years of age. From that time the bones of the fore-arm ceased to grow, and the cicatrix of the integuments contracted, so as to draw back the metacarpal bones of the hand upon the radius and ulna, leaving the knuckles on a line with the integuments, the fingers taking origin, as it were, from the lower surface of the middle of the fore-arm, and within three inches of the elbow. At the time of the operation, the fore-arm was bent at right angles to the arm. The dissection showed the bones of the fore-arm dislocated at both ends from the humerus and carpus. The use of the fingers had been so far preserved that the patient found the deformed hand of great service, more particularly in knitting, in which she constantly employed herself.

Mr Syme considered the case as particularly interesting in this point of view, as showing what an amount of deformity may exist without the limb being rendered useless; showing how reluctant we should be in performing amputation of limbs, the usefulness of which may be partially preserved.

The limb was ultimately removed, on account of an intractable sore, which had formed over the elbow, from which bleeding frequently took place, more particularly during the menstrual period—the discharge of blood at that time certainly taking the place of the catamenia.

ANEURISM OF THE HEART.

Dr Halliday Douglas presented to the society a specimen of aneurism of the left ventricle of the heart.

There were two aneurismal sacs—both near the apex of the heart, and communicating freely with the cavity of the ventricle. Each sac was about the size of a chestnut—one was empty, and communicated with the ventricle by an orifice having a diameter of three quarters of an inch; the other sac contained a decolorized fibrinous clot, with a small cavity in its interior, and communicated with the ventricle by an orifice less than a quarter of an inch in diameter. The margin of the orifice of this sac was *firm*, almost cartilaginous, and perfectly *smooth*. In several other points of the ventricular surface, there was the appearance of the commencing formation of a similar sac.

The heart was increased in bulk; its weight not stated. The ventricles, especially the left, were dilated. The endocardial surface was opaque and indurated. The valves were competent. Chronic adhesion of the pericardial surfaces existed in different situations, more extensively on the surface of the left ventricle, and on the surface of both aneurismal sacs. The state of the other organs was not described, except the kidneys, which were granular, and the liver, which was healthy.

The patient was a female, aged thirty, admitted into the hospital under charge of Dr Douglas, November 1847. The symptoms of cardiac disease had not been distinctly developed till within a year of her death. Previous to this she had suffered from chest symptoms, arising from chronic bronchitis, which latterly became intense, and she had worn a tracheotomy tube for twelve years.

She was under the care of Dr Douglas only for ten days previous to death. At this time there was general dropsy; there were the signs of intense general bronchitis, and a much enfeebled circulation; the cardiac signs were chiefly referable to the dilatation; but a murmur accompanied the first sound, which was doubtfully more distinct at the apex.

The urine was exceedingly scanty, and had been so for a length of time, and was ascertained to have been free of albumen till shortly before death.

Dr Spittal remarked that the case was a rare one, and asked if Dr Douglas had formed any opinion as to the mechanism by which such sacs were produced? Was there ulceration of the membrane lining the ventricle, and a consequent yielding of its parietes during the contraction of the heart?

Dr Douglas did not think that there was any ulceration, certainly there was no appearance of loss of substance of the heart where the sacs were formed. He conceived it quite possible that chronic inflammation of the endocardium had been the primary lesion, followed by loss of tonicity and mechanical expansion of a portion of the heart.

ANEURISM OF THE VESSELS AT THE BASE OF THE BRAIN.

Dr Douglas also presented to the Society a specimen of aneurism of the circle of Willis. The sac involved the internal carotid artery, where it divided into its terminal branches, and it was about the size of a field bean. It was removed from the body of a woman, aged thirty-nine, who was not seen by Dr Douglas during life. She was reported to have been suddenly attacked eight days before death by headach and vertigo. She afterwards became insensible, and died in the course of two hours.

She was stated to have suffered from headach fourteen years previously, when a seton was introduced, and other treatment employed. There was found on dissection a layer of blood effused generally on the surface of the arachnoid, but to a greater extent on the base of the brain. No calcareous deposit existed in the cerebral or other vessels.

Dr R. Mackenzie related a similar case which he had seen some years before in the Infirmary. The aneurism was seated in one of the communicating arteries of the circle of Willis, and was suddenly fatal by rupture.

ECCHYMOSIS IN THE COLON OCCURRING IN CHOLERA.

Dr W. Robertson showed a specimen of extensive extravasation of blood beneath the mucous membrane of the colon. The patient had died of cholera, fatal in the stage of collapse; there had been no unusual symptom in the case, except the discharge of reddish-brown fluid from the bowels. Stools of the same appearance had been observed in other cases during the present epidemic, but were not always associated with the ecchymosed condition of the mucous membrane.

Dr Spittal adverted to the frequency of ecchymosis in the course of the nerves, particularly the pneumogastric nerve in the neck, during the last epidemic of cholera.

Dr W. T. Gairdner said, that he had had his attention drawn to this point, in opening the bodies of the patients from the Cholera Hospital. He had examined the pneumogastric nerves in a number of cases, but had only once seen any considerable ecchymosis, and this was in a case in which extravasation had occurred in several places. Ecchymoses were very common in cholera in the intestines, and also on the pericardium, the conjunctiva, and the pleura. The blood coagulated very firmly in the heart and great vessels, and was in general very dense and viscid. The microscopic elements of the blood did not appear to be altered.

EDINBURGH OBSTETRIC SOCIETY.

SESSION VIII.

MEETING I.—DR SIMPSON, President, in the Chair.

STATISTICS OF THE OBSTETRIC PRACTICE OF DR LAWRENCE OF MONTROSE.

I.—GENERAL STATISTICS.

DURING the period of which a record has been kept, there occurred 360 cases. These 360 labours produced 368 children. There were thus 8 twin cases. 190 children were males: 178 females. 97 labours were primiparous: 263 were

second or subsequent deliveries. Of the 360 deliveries, 154 took place between 6 A.M. and 6 P.M.; and 206 between 6 P.M. and 6 A.M. 11 children were born dead; and in three of these putrefaction was more or less advanced. There were 13 tedious and difficult labours. Instrumental aid—the forceps—was required thrice. Of the 360 cases, 31 were over before my arrival, and from their rapidity, the presentation may be presumed to have been natural. 11 cases were preternatural presentations, viz., 3 arm, 2 funis, 5 breech, and 1 face. Deducting these numbers, and also the twin cases from 360, there remain 310 cases of cranial presentation; but the position of the head unfortunately has not been noted. Ante-partum hemorrhage occurred twice at full period of utero-gestation; and post-partum hemorrhage also twice. There was only 1 case of puerperal convulsions. In one of the cases the mother was a mute. All the women recovered. No attempt was made at recording the duration of labour, from the difficulty—if not impossibility—in private practice of fixing the exact commencement of the parturient process. Both nurses and patients often err greatly in this point. Neither have I any record of the *ages* of my patients. This is a point which, in private practice, we must be often contented to remain in ignorance of.

TABLE I.—*Showing the Proportion of the above Particulars.*

Male children, ...	190 to 368 births, being	$51\frac{2}{46}$	per cent.
Female do., ...	178 to 368	... $48\frac{7}{46}$	...
Primiparous births, ...	97 to 360	... $26\frac{1}{18}$	...
Second and subsequent births, ...	263 to 360	... $73\frac{1}{18}$	...
Delivered between 6 a.m. and 6 p.m.,	154 to 360	... $42\frac{7}{9}$	...
——————— 6 p.m. and 6 a.m.,	206 to 360	... $57\frac{2}{9}$	...
Tedious and difficult labours ...	13 to 360	... $3\frac{1}{18}$	...
Forceps cases, ...	3 to 360 do. being 1 in 120		
Children born dead	11 to 360	... $32\frac{8}{11}$	
Preternatural presentations, ...	11 to 360	... $32\frac{8}{11}$	
Twin births, ...	8 to 360	... 45	
Ante-partum hemorrhage, ...	2 to 360	... 180	
Post-partum	2 to 360	... 180	
Puerperal convulsions	1 to 360	... 360	

II.—SPECIAL STATISTICS.

1*st. Tedious and Difficult Labours.*

Of this class of labours it has been stated there were thirteen cases. The causes generally were deficiency of uterine action, rigidity of soft parts, or largeness of child. In only one case was it malposition. *Nine* of the children were boys, and *four* girls. *Five* were primiparæ; and *eight* second or subsequent births. *Ten* were delivered by the natural powers, and *three* by the forceps. *Twelve* of the children were born alive, and *one* dead.

TABLE II.—*Showing particulars of Tedious and Difficult Labours.*

No. of Cases.	Sex of Child.	Primiparæ.	Second or subsequent Births.	Delivered by Nat. Powers.	Delivd. by Forceps.	Born alive.	Born dead.
13	9 B. 4 G.	5	8	10	3	12	1

2*d. Children born Dead and Putrid.*

Of the *eleven* children born dead, *five* were males and *six* females. In *eight* of these death seemed to have taken place during labour. In *three* it had occurred some time previous, as evidenced by advancing putrefaction. Of these

latter, *two* were females and *one* male. In the *first* of these *three* cases, the death of the fœtus seemed to be owing to a severe convulsion fit experienced by the mother two or three weeks before its birth : in the *second*, to have been occasioned by dissipation on the part of the mother : and in the *third*, it appeared referrible to a syphilitic taint. In this last case the placenta was particularly soft, giving way on the slightest pressure.

3d. Preternatural Presentations.

A. *Arm Presentations.*—Three cases of arm presentation occurred, being 1 in 120 cases. All of these were unfortunate as regarded the child. In *two*, turning was performed by art, and in *one* by nature ;—in other words, it fell to my lot to witness that rare effort of nature, spontaneous evolution or expulsion. In both of the cases in which turning was performed, I did not see the patient till after the waters were evacuated ; the process was, therefore, accomplished with difficulty. In the third case, not only were the waters evacuated before I visited the patient (residing several miles distant in the country), but the uterus grasped the child with such vice-like firmness, as to render the introduction of the hand utterly impossible. The right shoulder was thrust firmly down into the cavity of the pelvis ; the head was directed to the right, the breech to the left iliac fossa. The uterine contractions were extremely powerful, and almost unintermittent ; indeed, I feared laceration of the uterus from excessive action. An anodyne was given on my arrival with a view of controlling the powerful uterine action ; and when, some time after, preparing to give another with the same intention, until I should procure the assistance of one of my brethren for the performance of evisceration (which seemed inevitable), I was dsudenly summoned to the bedside, and found, to my surprise and joy, that spontaneous evolution had taken place. The breech was protruding; another pain expelled the abdomen and shoulders, and delivery was speedily accomplished. This result, occurring so suddenly and unexpectedly, and at a time when I felt myself in the most helpless and distressing predicament I ever was in since commencing the practice of midwifery, I greatly regret that I did not sufficiently observe the phenomena presented, so as to be able to say whether the process corresponded to the description given by Denman or Douglas. But the impression left upon my mind decidedly is, that the protrusion of the arm was continued during the expulsion of the breech,—consequently, that the explanation of Dr Douglas is the correct one. The child was at the full time, and rather stout. It was dead. The mother made a good recovery.

B. *Funis Presentations.*—There were *two* cases of funis presentations or 1 to 180 cases. In the first, the head was so far down in the pelvis before I saw the patient, as to prevent the return of the cord into the uterus ; and, as the labour was advancing rapidly, I did not deem it necessary to apply the forceps. Delivery was speedily accomplished, but the child was dead. In the second case, the membranes were ruptured before my attendance commenced ; and, finding the os uteri well dilated and lax, the head high, the funis nearly pulseless, and that the uterine action intermitted, I proceeded immediately to perform version, which was easily and speedily accomplished, but the child showed no signs of life.

C. *Breech Presentations.*—Exclusive of the twin cases, there were *five* cases of breech presentation, or 1 in 72. This proportion is of course increased if the twin cases are taken into account. In all these cases the breech was allowed to pass. In *three*, the children presented at first no symptoms of animation, but had respiration established under the employment of the usual measures ; in the other two cases, they were vigorous from the first.

D. *Face Presentations.*—One case of this presentation occurred. The chin was directed to the pubis. Notwithstanding of this malposition, and of its being a primipara, delivery was accomplished in a comparatively short period. But, from the circumstance of the cord being tightly coiled around the neck, the child was dead.

4th. Twin Births.

Eight cases of twin births to 360 deliveries, or 1 in 45, is certainly a high proportion as compared with that given in published reports. From returns readily accessible to me, I have drawn up the following table, which exhibits a striking disparity between the result of my own practice and that of others.

TABLE III.—*Showing the Statistics of Twin Cases.*

Field of Practice.	Proportion of Cases.	Reporters.
1. PUBLIC.		
Dublin Lying-in Hospital,	1 in 64	Drs Hardy and M'Clintock.
British Lying-in Hospital,	1 in 85	Dr Lee, quoted by do.
University College Hospital,	1 in 156	Dr Murphy.
Maison d'Accouchement, Paris,	1 in 84	Quoted by Dr Burns.
Wurtemberg, . . .	1 in 86	Do. do.
2. PRIVATE.		
Glasgow, ...	1 in 63	Dr Maxwell Adams.
Maryhill, near Glasgow, .	1 in 182	John Stewart.
Montrose, ...	1 in 45	S. Lawrence.
Average—7 in 720, or 1 in $102\frac{6}{7}$; and 8 in 765, or 1 in $95\frac{5}{8}$.		

The first of these averages is *exclusive*, the second *inclusive*, of my own cases; and, even as compared with the lower of the two, my experience shows a proportion twice as large, while its relation to Dr Adams' return (the lowest in the above table) is as $2\frac{2}{9}$ to $1\frac{3}{64}$ per cent. However, the great probability is, that the addition of one or two hundred cases to the 360 I have recorded, will very much diminish the proportion stated. I draw this conclusion from the *reverse fact*, that while the first 251 cases of my list gave only *three* twin births, or about 1 to 84, the remaining 109 gave *five* twin births, or fully 1 to 22. Just, therefore, as the proportion has been greatly increased by the last hundred cases presenting a *run* of twins, is it likely that it will be equally diminished during the next one or two hundred cases by a paucity of such births.

Of the 16 children produced by the 8 twin births, 13 were girls and 3 boys: in 5 cases 2 girls being born together, and in 3 a boy and a girl.

In 6 cases the placenta was single.

In 2 cases the placentæ were separate.

In 5 cases the patient had reached the full term of utero-gestation; in 2 the labour was premature; in 1 it was an abortion.

In 2 the labours were primiparous; in 6 second or subsequent. Two of the cases occurred consecutively to the same patient, who twice gave birth to two daughters within a year! (The first twins, born in the 8th month, did not survive.) The presentations cannot be classified, for in two cases they have unfortunately not been noted, and in two delivery was effected before my arrival. Of the remaining four cases, in one the child was delivered by the breech, the other by the head; in another, one of the children was by the foot, the other by the head; and in the remaining two both were head presentations. The longest period between the delivery of the children in any of the cases was an hour and half; the shortest ten minutes.

One of these twin cases (that of the abortion) deserves special mention from having been associated with a complication of not very frequent occurrence,

dropsy of the amnion. The following is an abridgement of the notes I preserved of it:—Mrs ——— is four months gone in third pregnancy. At the end of the second month, she seemed larger than in due proportion to the period of pregnancy, but there was no abdominal pain, and the general health was good. The only peculiarity was, that the sickness incident to her state was much more severe than in either of her former pregnancies. About six weeks ago (between the second and third month) began to experience frequent pain in right lumbar and umbilical regions, which still persists. Her size since the occurrence of the pain has been rapidly increasing, and now she presents the appearance of a woman at the full period of utero-gestation. Fluctuation is well marked over the whole abdomen. Diagnosis, "Pregnancy with ascites." With the view of removing the latter, she was placed under the usual treatment, and, failing to derive any benefit from it, after eight or ten days met a medical friend in consultation on the case. He concurred in the diagnosis, and in the pending necessity for the performance of paracentesis. This operation was accordingly performed on Saturday at mid-day, and about six pounds of straw-coloured fluid were withdrawn. The stream through the canula was never full, and never clear, being always more or less tinged with blood; frequently it was altogether interrupted by a substance which felt soft and spongy when turned aside by the probe, and from which hemorrhage to the amount of five or six ounces proceeded. For some time before the canula was withdrawn, only blood flowed through it. The hemorrhage having ceased, the canula was withdrawn, and the wound closed. The size of the abdomen was not one half reduced by the operation. *Second day.*—Passed a good night; but between eight and nine this morning, after a little exertion moving round in bed, there was slight hemorrhage per vaginam, followed by rigor and quick pulse, but no abdominal pain. At five P.M. I was summoned suddenly to the patient. About an hour previous, while turning in bed, the membranes burst, and an immense quantity of water was discharged. The bed was literally flooded. In half an hour after this discharge, labour pains came on, and ere I arrived two female fœtuses were expelled, and the half of a double placenta. I delivered the remainder. The quantity of blood lost was trifling. The fœtuses seemed to have been between the fourth and fifth month, and the membranes were of very great size. The uterus was quite contracted, and the abdomen now reduced to its natural dimensions. There was a little febrile excitement for a few days after this; but subsequently the case progressed favourably, excepting that suppuration in one of the mammæ retarded recovery for a time.

A case very similar to this one is recorded by Desmerais, and quoted by Dr Davis, wherein the same error of diagnosis was committed, and the same treatment pursued. Twenty-one days, however, elapsed between the tapping and the occurrence of labour, whereas in the above case the interval was only about twenty-eight hours. Desmerais' case, like my own, was one of twins, and so also was that of M. Devilliers.—See *Dr Davis's Obstetric Medicine.*

One or two questions suggest themselves in connexion with such cases.

1st, What is the most probable cause of such monstrous accumulation?

2d, Are such cases always associated with twins; and what is the nature of this relation?

3d, How can such cases be best distinguished from pregnancy with ascites?

4th, When so distinguished, what is the best practice to be pursued?

5th, Is there any possibility of reducing the excess of the liquor amnii, without interrupting the process of utero-gestation?

5th. Labour with Complications.

A.—*Ante-partum Hemorrhage.*—It has been stated that only two cases of ante-partum hemorrhage occurred at the completion of pregnancy. Both were accidental, it having been my good fortune never to meet with any case of placental presentation. In both the hemorrhage was comparatively slight, and gave way as labour advanced.

B.—*Post-partum Hemorrhage.*—The only two cases of post-partum hemorrhage met with out of the 360 labours, occurred somewhat strangely on the same day. In each it occurred after the expulsion of the placenta. The flooding was very considerable in each of these cases, but was arrested by the usual remedies. In the second of them, in addition to the faintness and rapid pulse usually resulting from sudden loss of blood, there was immediate and great aberration of thought, which continued for upwards of two hours.

C.—*Puerperal Convulsions.*—The only instance of puerperal convulsions met with was in one of the twin cases. It occurred about an hour and a half after the birth of the second child, and was arrested immediately by venesection performed in the sitting posture.

I would, in closing this communication, advert to two points of practice, of which my experience is decidedly in favour, although I cannot offer any statistics regarding them. The first is gentle dilatation of the os uteri with the finger, when the pains are at all inefficient. That uterine action is often rendered much more brisk and effective in this manner, I entertain not the smallest doubt; while, on the other hand, I have never experienced any thing unfavourable to result from it. Query—Does the oxytoxic effect depend upon a reflex action being excited by the pressure of the finger, or upon the os uteri being placed in a more favourable position for being acted on by the longitudinal fibres of the body of the organ, when the os is pushed a little forward—just as the eye, when it has been deeply inverted by the internal rectus, cannot be advantageously acted on by the external rectus until it is first brought forward by the inferior oblique?

The second point of practice referred to, is the method adopted to secure timely expulsion of the placenta. Besides directing pressure to be made over the uterus after the birth of the child, I invariably lay hold of the cord, and, without pulling, *retain it in a state of tension.* This seems to have the effect of exciting the uterus to speedy contraction; and to this practice I attribute the fact of the placenta being expelled in all my cases within ten or fifteen minutes, in the majority within five minutes, after the birth of the child. While it cannot explain the fact of my fortunately having met with no cases of retained placenta from adhesion, I think I am warranted to ascribe to this method of procedure the fact of my having met with no cases of placental retention from inertia, or irregular contraction of the uterus.

I may also state, what may seem almost too petty to mention, that I never use tape for tying the funis before the separation of the child, but always *strong sewing thread.* I was led to this from having got some serious frights at the outset of practice by the slipping of the tape, and the alarming, and to the child almost fatal, hemorrhage which ensued. I have known the same casualty to occur in the practice of others, from the same cause.

ON A SUCTION-TRACTOR; OR NEW MECHANICAL POWER, AS A SUBSTITUTE FOR THE FORCEPS IN TEDIOUS LABOURS. BY DR SIMPSON.

Cases ever and anon occur of morbid protraction and delay in the second stage of labour, with the head of the child sunk down into the pelvic cavity, or resting upon the perineum; with the caput succedaneum enlarging more and more under the pressure and prolonged uterine contractions, and yet these contractions producing little or no effect upon the actual advancement of the infant; and with local and general symptoms threatening to supervene, and showing the propriety, if not the necessity, of the patient being relieved, and delivery being completed.

In such tedious and trying cases, if there were space for us to pass our hands between the soft parts and head, so as to seize with them and drag upon the head, we could curtail the patient's sufferings. There is not room for this. Our hands are too thick for the purpose. As a substitute, accoucheurs under such circumstances introduce a thin metallic hand (so to speak) on each side of the child's

head,—viz., the forceps—which exactly, like a couple of slender hands, take hold of the head, and enable us to apply, when necessary, extractive force to it. One objection which had been often urged against the use of the forceps (though it was an objection against their abuse rather than their use) was this, that the instrument, in being introduced between the maternal passages and head, was apt to injure these passages, and to contuse and even lacerate them during their working; that they were thus liable to inflict local damage on the mother at the very same time that they shortened the term of her sufferings and saved her infant.

Is there any other mechanical power which it is possible for us to apply to the infant's head in order to seize and move it forward, which would not be liable to the same objection of danger to the mother?

In such tedious cases as seem to require the use of the short forceps, the idea has perhaps crossed the minds of most accoucheurs, that if they could get hold of the *exposed* portion of the scalp of the child, or of the skin forming the caput succedaneum, and could possibly pull by *this* hold, they might thus expedite the process of delivery, and cut short the sufferings of their patients. The spherical form of the infant's head, and the intimate mode in which the scalp is spread over the arch of the cranium, prevent the possibility of taking any such hold by the fingers alone. By means of a small suctorial disc, the shell of the patella or limpet (which is so common upon our shores) fixes itself with great force to the stones or rocks in which it is placed. Dibranchiate cephalopods—as the cuttle-fish—fix their arms, by similar suctorial discs, so firmly to different surfaces, that their arms themselves will often tear before the suckers with which they are attached will give way. If we could fix upon the *exposed* portion of the fœtal scalp the suctorial disc of a limpet or cuttle-fish with the usual force with which they adhere to the sea rocks to which they are attached, we would have, in many cases, a power sufficient to enable us to apply by them the necessary amount of extractive force. The discs of the limpet and of the cuttle-fish attach themselves firmly to the surfaces to which they adhere, by being formed so as to act upon the principle of the common sucker used by the schoolboy to lift stones, &c.—viz., by removing, or rarifying as far as possible, the air placed between the attaching and attached body, and thus taking advantage of the great power exercised by the atmospheric pressure upon the surfaces of solids. This pressure is, as is well known to all, equal to fifteen pounds upon the square inch when the subjacent vacuum is perfect; or, in other words, it would require a force equal to fifteen pounds of every square inch attached, to effect the separation of surfaces thus united. The limpet and cuttle-fish have the surface of the acetabula or discs with which they fix themselves so strongly upon the rocks, bedewed with a thick mucous secretion; after placing the surface of the disc upon the part to which they are to attach themselves, they, by a muscular movement, raise the centre of the disc so as to produce a more or less perfect vacuum; and the cuttle-fish has a central body in the middle of each disc, which it draws up and uses for this purpose, exactly on the principle of the piston of a syringe.

Such an arrangement and apparatus may be imitated by art; and when rendered more perfect and complete, may perhaps give us a simpler and safer obstetric power for some cases than even the forceps. In one protracted case which Dr Simpson described, he had lately made use of this power to extract the child. When applied, the head was still high up in the pelvic cavity, and the instrument easily afforded such a hold of the head as to allow it to be slowly dragged forwards and extracted. During this extraction the instrument required to be reapplied once or twice. Dr Duncan and Mr Dickson were present at the delivery.

The instrument used in this case was very rude and imperfect. It consisted of a common metallic vaginal speculum, fitted with a piston, and with the edge of the trumpet-shaped concave disc at its outer or broader end covered with leather. This broader and leathered end was coated with lard, and applied to the head

of the child; and then an exhausting effect produced by moving the piston forwards. The apparatus would admit of much improvement and simplification, as by the mouth of it being made expansible, and capable of altering in shape, instead of metallic and fixed; by the inner edge of it being coated, as in atmospheric railways, by a thin layer or cushion of air inclosed in caoutchouc; by the exhausting apparatus being valved and more perfect, &c. &c.[1] But if the Air-tractor could not be made both simple and satisfactory in its application, it would not replace the forceps; and more experience would be required to decide whether it had any title to do so.

If the instrument, when properly constructed, should be found to succeed, it would be still more advantageous in replacing the long, than in replacing the short forceps. In the case in which it was used, the head was of the height in which long forceps are usually required. If a suctorial Tractor should answer in some long forceps cases, and enable us to drag with sufficient force upon the exposed portion of the scalp, it would save the danger dreaded by many, of wounding the uterus by introducing and working the blades of so long an instrument as the long forceps high up in the neck and cavity of the uterus itself.

Presentations of the breech sometimes require instrumental assistance. The hook passed over the flexure of the thigh is dangerous, and very apt to injure. The forceps, as recommended in these presentations by some authorities, are often inapplicable and inefficient. Perhaps the Air-tractor may afford us a new and sufficient instrumental force for the management of some of these cases. Its use would be simpler and safer than any of the other methods proposed.

Dr Simpson further observed, that he was not aware that any one had applied practically this obstetric means before it was employed in the case detailed to the society. But the idea of using such a power had been long ago proposed by a gentleman, for whose works and talents they all entertained the utmost respect, Dr Arnott of London. In his admirable work on Physics (p. 636), Dr Arnott alludes to the subject in the following words:—"The forceps (says he) to be well and safely used, requires address, which even the naturally dexterous man cannot possess without a certain degree of continued practical familiarity with it: and, except in large towns, a man must be unfortunate in his practice who often requires it: hence the really small number of persons who use it well. A tractor of three inches in diameter would act upon any body, to lift or draw it, with a force of about a hundred pounds—with more, therefore, than is ever required or allowable in obstetric practice. In lifting a stone, the tractor does not act as if it were glued or nailed to the stone, but merely bears or takes off the atmospheric pressure from one part, and allows the pressure on the opposite side, not then counterbalanced, to push the stone in the direction of the tractor; so when placed upon the child's head, it would not pull by the skin, in the manner of a very strong adhesive plaster applied there, as uninformed persons would be apt to suppose; but by taking off a certain atmospheric pressure from the part of the head on which it rested, it would allow the pressure on the other side or behind to urge the head forward on its way. Of course the pressure in such a case would not operate on the head directly, but through the intervening parietes and contents of the abdomen. It would be preferable to have a gentle

[1] Since the preceding abstract was drawn up, I have made a great variety of experiments, with the view of ascertaining the best form of disc or mouthpiece and exhauster. I find that a syringe and piston, valved like the common breast-pump, so as to make a pretty perfect vacuum, and having a disc attached to it formed of a double cup, the outer cup of caoutchouc, and overlapping considerably the edges of an inner and smaller cup of metal or gutta-percha, makes an Air-tractor possessed apparently of the necessary applicability, and requisite adhesive and extractive power. —J. Y. S.

and diffused action of the tractor over a large space, rather than an intense action on a small space, and therefore a tractor for the purpose now contemplated should not be very small, and should have a little air underneath it in a slight depression or cavity at its centre. The forceps must be more effective than the tractor for rectifying malposition of the head, and diminishing its transverse diameter; but the tractor will answer both these purposes in a greater degree than might at first be expected."

Several of the old surgeons, as Parè, Paaw, Hildanus, Scultetus, &c., have described and figured suckers, or tractors, as applied to the head with the object of removing depressions of the cranium in children.

IMPROVED SPECULUM FOR VESICO-VAGINAL FISTULA. BY DR KEILLER.

Dr Keiller exhibited a vaginal speculum which he had found extremely useful in conducting the treatment, by cauterization, of urinary and fæcal fistulæ. He considered the instruments in common use, from the simple tube to the quadruple dilator, in many cases objectionable, on the ground that they were neither well fitted to enable the practitioner to detect the nature and exact extent of breaches in the vesico-vaginal or recto-vaginal septa, nor very efficient appliances in conducting their surgical treatment. From what he had witnessed he believed that the simple handled spatula exhibited, would be found to be an useful addition to the double-bladed instruments generally used for vaginal exploration, more especially for the ready application of the actual cautery to minute fistulous apertures communicating with the urinary or fæcal passages. Dr K.'s "*Fistula Speculum*" consists of *a single branch or blade, with a handle attached at an obtuse angle*, like that of a blunt gorget, and which, when used conjointly with the original two-branched instrument of M. Ricord, or the improved speculum of Charriere without its third or sliding blade, can be readily moved, *by means of the handle*, so as to stretch at pleasure any point of the vaginal wall on the sides or extremity of the blade, without withdrawing the speculum used for dilatation, which is all but useless when used without this third or *handled blade*, for the obvious reason, that, whenever its two branches are separated in the vagina, the wall opposite to that on which they rest being freed from all resistance by the opening of the blades, becomes flaccid, and immediately fills the concavity of the expanded instrument, obstructing, more or less completely, the necessary view of the parts particularly involved. These disadvantages are readily overcome by means of this modification of the third blade, which greatly facilitates the safe and efficient cauterization of fistulous apertures.

Dr Keiller cited the particulars of the cases which he had treated, and particularly referred to the case that first induced him to employ the above instrument, and which presented a somewhat peculiar feature in regard to the *position* of the vaginal aperture of the urinary fistula, it being situated in the *cul-de-sac posterior to the cervix uteri*, a peculiarity of position probably consequent on the degree of adhesive action that had followed the ulcerative process, which in this instance, as in most instances of vesico-vaginal fistulæ, was caused by the too late use of the forceps in a severe and long-protracted labour. As the usual barrier to success—the close proximity of the vesical to the vaginal aperture—did not obtain in this case, a few applications of the cautery established a complete cure, the extent of structure intervening, together with the circuitous route of the communication, and the valvular tendency which the urinary aperture must have consequently presented, rendering, in all probability, this apparently bad case a very successful one.

Dr Keiller also stated that he had succeeded in making an *artificial anus* in the usual site of that aperture, in the perineum, *in a girl seven years of age, who was born with imperforate anus*. The cure, however, being as yet incomplete, in as far as a small recto-vaginal fistula still exists, Dr K. reserved the

particulars of this very interesting case for another occasion, when he could better report the completed result.

EXPULSION OF THE MEMBRANES BEFORE THE CHILD. BY DR M'COWAN.

Mrs B., æt. 25, was delivered on the 17th December 1848, of her second child. The head of the child presented, with the face directed to the left acetabulum, and was born in that position without making the usual turn to bring the occiput in front. The bag of membranes came away almost entire about two hours before the head was born. The bag contained a large quantity of liquor amnii, aud was considerably larger than a child's head. It was rolled into a basin in order to be preserved, but female curiosity could not be restrained, and it was somewhat torn while Dr M'Cowan was engaged with the mother.

PECULIARITY OBSERVED IN THE SPONTANEOUS EXPULSION OF THE CHILD IN AN ARM PRESENTATION. BY DR SIMPSON.

Since the subject of spontaneous evolution was discussed by the Society—(See *Journal*, May 1847)—Dr Simpson has seen one case of its occurrence The patient was under the charge of a midwife. Mr Alexander, who was called in, requested Dr S. to see the case, with the view of turning the child. The mother had already borne four children. The present pregnancy had advanced to between the seventh and eighth month. When born, the child weighed 3 lbs. 9 oz., and measured 17½ inches in length. The labour commenced at 8 A.M., but made very little progress till the afternoon. At 7 P.M., when Dr S. arrived, the shoulder of the child was pressing deeply into the pelvis. The rectum seemed loaded; and Mr Alexander and Dr Simpson left the room for a short time, in order that the nurse might empty the bowel with an enema, before turning was practised. On being recalled they found the pains strong, and the *body* of the child beginning to press down through the pelvic brim—indicating the commencement of the usual process of spontaneous evolution. Dr S. remarked that this peculiarity and change in the presentation had taken place during the ten or fifteen minutes they had left the patient:—the fœtus had made a slight turn upon its long axis, so that the fœtus presented much more of the back, and less of the side than before, the shoulder still, however, maintaining its deep position in the cavity of the pelvis. The child's body came down, in other words, not with the spine or trunk bent and doubled up laterally, but bent and doubled up in a great measure anteriorly. The anatomical structure of the spinal column admitted far more easily of this anterior flexion than of lateral flexion. Was this a common turn in the mechanism of spontaneous expulsion? If so, did the want of it delay or prevent the process of spontaneous expulsion in some cases. And could it be imitated by art so as to allow the process to go on in appropriate cases? After being forced into this new position, the child was very quickly expelled by the natural efforts, and according to the common mechanism of spontaneous evolution. As usual, it was born dead.

TRANSFUSION IN HEMORRHAGE. BY DR WEIR.

Dr Weir showed a new instrument for transfusing blood in cases of excessive hemorrhage. The instrument was provided with a very large cylinder, intended to contain at once the quantity of blood required to be injected.

Dr Simpson stated that he had seen transfusion tried in three consultation cases, where the patients were dying from the results of uterine hemorrhage. In a case last week to which he had been called by Dr Purdie—the lady, who was infirm and jaundiced before delivery, kept well for an hour and a half after delivery. Sudden hemorrhage then supervened, which was speedily and entirely arrested by Dr Purdie; but the patient was left by it in a collapsed, pulseless state, and was incapable of being rallied by the strong stimulants used. Dr S. saw her about an hour subsequently, and injected into the basilic vein twelve ounces of a saline solution, like that used here in the last cholera

epidemic. The solution roused the patient for a short time, but not much; and she remained three hours afterwards in the same sunk, collapsed state, and died without any renewal of the flooding. In another case Dr S. had seen the same practice followed, and when the patient was quite moribund, with no result and no rallying. Three or four years ago, he was called to a case a few miles from Edinburgh, where he found the patient left in the same collapsed and pulseless state, after an early miscarriage, accompanied with much hemorrhage. In this as in the two other cases, the state of pulseless collapse, not capable of being affected by brandy or other stimulants, continued for hours after the hemorrhage was stopped, and before the patient died. A surgeon was called out of town, but failed in finding a vein large enough to inject. He had not seen pure blood transfused; but the result did not seem to promise much or any benefit from the injection of saline fluids, intended merely to fill and re-distend the vascular system.

EARLY CORPORA LUTEA, AND STATE OF OVARIES DURING MENSTRUATION. BY DR SIMPSON.

Dr Simpson exhibited three early corpora lutea taken from subjects who had died of epidemic cholera. In all the three women the uteri showed the decidual membrane beautifully developed, and one or two lines thick. The decidua was very vascular and injected, the vessels running at right angles to the surface. The decidua was, Dr S. thought, proved from these and such cases to be, merely the normal lining membrane of the uterus thickened, as Seiler, Sharpey, Goodsir, Coste, &c., mentioned, and not a new membrane. The corpora lutea were each of about the size of a small cherry, granulous, plicated, and of a reddish or reddish-yellow tint, like the early corpus luteum figured by Dr Paterson of Leith. The plicæ radiated in each from a central spot, the remnant of the central cavity.

Dr S. exhibited other uteri and ovaries from cholera subjects, dying during the function of menstruation. He showed the series and succession of changes occurring in one of the Graafian vesicles in connexion with menstruation, and pointed out the difference between these changes and the formation of true corpora lutea. The particular Graafian vesicle affected in menstruation, becomes at first congested in its coats and lining membrane; the lining membrane becomes more and more injected; at last blood is effused from these injected vessels into the cavity of the vesicle, and this blood forms at first a dark unorganized clot, easily lifted out of the cavity of the vesicle, and quite different in character, in form, structure, and seat, from the organized, granulous, plicated tissue of a corpus luteum.

LETTER FROM DR EBEN WATSON, ON ALLEGED CASES OF RECOVERY FROM RUPTURE OF THE BLADDER.

To the Editors of the Monthly Journal of Medical Science.

Gentlemen,—In your Number for December last, I stated it as my belief, that the case published by Mr Syme in his "Pathology and Practice of Surgery," was "one of the only *two cases on record* of recovery from rupture of the bladder." The second case referred to was that contained in the "Provincial Medical and Surgical Journal for 1846" (p. 333). This case has, since its publication, been very generally received as what its title proclaims it to be, viz., a case of recovery from rupture of the bladder; and I, among perhaps many others, must plead guilty to having accepted the diagnosis of the attendant surgeons without judging independently of their report. Since the publication of the paper, in which the statement above referred to occurs, I have had my attention directed to that report, and feel bound, though with considerable regret, to withdraw the consent tacitly given to the diagnosis of the case in question.

I beg, with due deference, to submit the following questions to those who believe, as I did, that this really was a case of rupture of the bladder.

1st. Is it likely that the rupture could be so extensive and in such a situation as to admit of the escape of *all the contents* of the distended bladder into the peritoneum? So far as my memory serves me regarding recorded cases, except in this one instance, complete evacuation of the bladder never occurs when it is ruptured. There are many reasons for this fact, such as the fixed position of the viscus owing to its ligaments, the diminution of the rent by contraction of its muscular coat, and the absence of a stimulus to further contraction after part of the urine has escaped. In the case under consideration, however, no urine flowed through the catheter when introduced half an hour after the occurrence of the accident.

2d. The first urine obtained by catheterism was eighteen hours after the accident, and that urine was bloody, but never afterwards in the history of this case. Now, would not the urine have been bloody for several days after rupture of the bladder? I humbly think it would.

3d. Is it consistent with experience that such an effusion of urine into the peritoneum should cause so little nervous depression as that the man, after its occurrence, should be able, though with great difficulty, to reach his home, a distance of 100 yards; and that, on being put to bed and heated by means of fomentations, symptoms of moderate reaction should be exhibited? Is it possible, further, that the peritonitis, consequent on such a cause, should have been subdued by the following, viz.—twenty leeches, and fomentations to the abdomen, together with two scruples of Battley's sedative liquor in one dose? Truly, I never found "alarming peritonitis" yield so readily, nor have I ever read of it before.

4th. Does it not give a character of great fancifulness to this report, to find in it the following unhesitating assumption, viz. that, "*no doubt*," *gout* was, in this man's case, *caused by absorption of urine?*

5th. Is it not too much also to assume, that a rupture of the bladder could re-open on the ninth day after its supposed closure, allow the urine to be poured into the peritoneum, cause a repetition of the former symptoms, and *heal up again in four hours?*

6th. Is it not, morover, rash to found a diagnosis of rupture of the bladder upon an absence of urine from that organ for so short a period as eighteen hours, even taking into account the blow received on the abdomen? How much less was the surgeon in this case warranted to conclude that the rupture, he believed to have existed and to have been closed for nine days, had re-opened, merely from the circumstance that he could find no urine in the bladder for the space of four hours?

It seems evident, then, that the report of this case contains many assumptions which are quite unwarranted by its facts. I cannot but think that the bladder of the subject of this case remained entire throughout, and that, instead of the grave lesion of that viscus supposed to have occurred, *an attack of gout* was induced by sudden exposure to the cold air after the heated atmosphere of the concert room, combined with the effect of the blow and the consequent fall. Instead, moreover, of the *absorption* of urine *causing* the gout in this case, it is far more likely that *suppression* of the urine *occurred* in the *intervals* of the paroxysms; and that what is in the report called peritonitis, was the flatulent colic which so often precedes or accompanies that disease. But, whatever may have been the real nature of this case, I am forced to the conclusion that, at all events, it was not one of rupture of the bladder. Mr Syme's case is therefore the only one, with which we are at present acquainted, of recovery from this grave lesion; and it is still consistent with universal experience, that "if the rupture (of the bladder) takes place above or within the peritoneum, there cannot be the slightest chance of escape" for the patient.—Your obedient servant,

EBEN WATSON.

152, ST VINCENT STREET, GLASGOW,
January 2, 1849.

[The case referred to by Dr Watson was reported by Mr Chaldecott of Dorking, and was also seen by Mr Key. It was that of a man æt. 50, who, going in a dark night to empty his bladder after passing some hours at a concert, received a blow on the lower part of the abdomen from a post with which he accidentally came into violent collision. He went home, became very faint, and had severe pain in the abdomen. No urine had escaped into his clothes, nor could any be brought away by a catheter passed freely into the bladder. The treatment indicated above by Dr Watson was pursued. Tenderness and swelling of the abdomen came on, with rapid and feeble pulse. Eighteen hours after the accident, about an ounce of *bloody* urine came away by the catheter. After this the catheter on its introduction always brought away *clear* urine.

On the third day after the accident the patient had an attack of gout. On the sixth day he "made many straining efforts to pass his water, and he had scarcely passed a teaspoonful, when he felt (to use his own expression) something give way, and a burning pain all over his stomach and bowels, as if boiling water had been poured over them, and the same symptoms of faintness and distress occurred as when the accident first happened." (This Mr Chaldecott considers to have heen a re-opening of the wound). No urine could be brought away by the catheter: the same treatment as before was pursued.

Four hours after, the bladder was again found to contain urine. The catheter continued for a fortnight to be used regularly. Symptoms of peritonitis, which had again come on, became subdued, and the recovery was only retarded by another attack of gout. Two months after the accident, he felt no other inconvenience except a dragging sensation in his abdomen, chiefly on the right side, and much increased by lying on the opposite side. (This is thought by the reporter of the case to result from adhesions).

"It must be recollected," says Mr Chaldecott, "that the bladder was known to be full at the time of the accident; no urine escaped into the clothes, nor did any come through the urethra for more than eighteen hours after, and then only about an ounce, mixed with blood. If, therefore, it did not escape into the cavity of the peritoneum, what else could have become of it?"—*Prov. Med. Journal*, July 22, 1846.

We leave this curious and interesting case without comment, although it appears to us that there is a good deal to be said in favour both of the author's view and that of our correspondent, Dr Watson.]

PROGRESS OF CHOLERA.

The cholera is making rapid progress, having extended its ravages to many new localities in Scotland and England, to Ireland, France, and America.

In France the disease first appeared at Dunkerque. M. Magendie was commissioned by the government to go to that town and observe the cases. He reported that they were not cases of Asiatic cholera. It is now suspected, however, that his report was dictated by the desire of the government to conceal the fact for commercial purposes. The ravages of the epidemic in Lille, Pas-de-Calais, Yport, Fécamp, and other towns, soon rendered concealment impossible.

The minister of agriculture and commerce has published general instructions on the subject of cholera, after the fashion of our Board of Health. They are well enough, only much too long. He has copied the Board in his exclusive view of the origin of the disease. "Le choléra n'est point une maladie contagieuse."

The captain of the American packet, New York, has published lately a letter describing what occurred during the passage. They left Havre on the 9th of November, having on board twenty-one cabin and 331 steerage passengers. Nothing occurred during the first fifteen days. On the sixteenth day of the voyage, a man aged twenty-nine years, steerage passenger, was attacked with cholera and died on the 29th of November. The second victim was a man of sixty-two years; the third, a little girl of eight years, and the fourth a boy of eight years.

When the vessel arrived at the quarantine hospital of New York, on the 30th of November, there had been seven deaths, and twelve were sick. Up to the 19th of December sixty-three were seized in the hospital, and of these thirty-one died. All these cases were confined to the steerage passengers. The quarantine hospital is situated upon an island, six miles from New York. There was free communication between this island and the town. One of the steerage passengers, who had left the quarantine hospital, and was residing in a hotel in New York, was seized and died. Some days afterwards another inmate of this hotel—not a passenger in the New York—and who occupied the same room and bed of the case last noticed, fell a victim to the disease.

It is reported that cholera has appeared in two emigrant vessels bound respectively for New Orleans and Baltimore.—*New York*, 20th Dec. 1848.

CHOLERA IN SELKIRK.—(*From a Correspondent*).—Diarrhœa has been prevailing for some time, and the first case of decided cholera occurred on the 10th of January. Up to the present time (January 22) there have been in all twenty-four cases, and four deaths. Selkirk presents a striking illustration of the independent production of the disease. A searching inquiry was instituted by Dr Anderson of Selkirk, and his son, Dr Henry Anderson (to whom I am indebted for this information), into the origin of the first cases, without eliciting any proof of contagion. Kelso, Jedburgh, and Edinburgh, are the infected localities from which it was possible the disease might have extended to Selkirk; but there is very little communication between the inhabitants of Selkirk and these places, and none could be traced among those who have been seized with cholera.

The locality in which the epidemic began, and to which it has been almost entirely confined, is named Dunsdale, and is well adapted to originate the disease. It lies close to the river, and the floors of most of the houses have a lower level than the river. They are consequently always damp, and, from the entire want of drainage, large pools of stagnant water collect around the houses. For the same reason, the filth accumulates, and disgusting odours beset one on all sides. Only four of the cases recorded were not resident in this locality. Two of these, though they did not sleep in Dunsdale, were occupied the entire day there as operatives in the mills. The third and fourth were in close communication with one of these two cases.

CHOLERA IN DOUNE.—(*From a Correspondent*).—I have to state, that, by the most minute investigation, I find that the cholera in this place has been imported from Glasgow.

The first patient was a horse-dealer, who had arrived from Glasgow on Thursday evening, and was taken ill on Saturday morning, his daughter on Sunday morning. An individual who was in his company on Friday night was the first victim—he died on Monday night. Four men who had gone to the well for water before daylight on Monday, saw Clerk's wife empty something into the common sewer beside the pump, but could not get water from it in consequence of the frost; one of the four got a kettle of hot water and poured its contents into the pump to thaw the ice. No sooner had they pumped out the hot water into the common sewer than all of them complained of a disagreeable smell—they were all affected that day, and three of the number died next day. The total number of cases of cholera in Doune is thirty-eight, and the deaths eighteen. The disease first appeared on the 6th, and there has been no case since the 15th instant.

Towards the end of last week, two or three fatal cases have occurred in Deanston—a crowded manufacturing village about a mile south-west from Doune. It was reported that the first person seized had been visiting a man who died of it in Doune on Wednesday, and took it himself next day.

THE LATE DR PRICHARD.

WE have to announce the death of Dr James Cowles Prichard, which took pacel recently, at his residence in London. Dr Prichard, who was born at

Ross, in Herefordshire, settled as a physician in Bristol in the year 1810, and was a few years after appointed physician to the Clifton Dispensary and St Peter's Hospital. In addition to his professional avocations, he occupied himself at this period in writing the first edition of his "Researches into the Physical History of Man," which appeared in 1813, and his work upon "Egyptian Mythology." In the year 1816 he was elected physician to the Bristol Infirmary, which appointment he filled in conjunction with that of physician to St Peter's Hospital; and in the year 1822 he published a work on the "Diseases of the Nervous System." In 1829 he wrote a small octavo work, entitled, "An Essay on the Vital Principle," dedicated to the patrons of the Bristol Philosophical Institution, of which he was one of the founders, and where he frequently gave lectures, and read papers on various subjects. He also took an active part in founding the Bristol College, and was for many years one of the members of its council. The degree of Doctor of Medicine of the University of Oxford was conferred upon him, by diploma, upon the occasion of the installation of the Duke of Wellington as Chancellor of that University. He was one of the visiting physicians of the Gloucestershire Lunatic Asylum, and a Metropolitan Commissioner in Lunacy before his appointment under the recent Act. About this time he wrote his work on "Insanity," a book upon the "Eastern Origin of the Celtic Language," and a little volume on insanity as connected with jurisprudence, and contributed various articles to the "Cyclopædia of Practical Medicine, and the Library of Medicine." In the year 1845 he was appointed one of her Majesty's Commissioners in Lunacy, and removed to London, where, besides the active duties of his commission, he completed the third edition of his "Physical History of Man," in five volumes, as well as his popular work on "The Natural History of Man." He was a Fellow of the Royal, and a great number of British and Foreign Societies, and at the time of his death was President of the Ethnological Society of London. In his intercourse with his professional brethren and colleagues, Dr Prichard's conduct was straightforward, honourable, and generous; to his patients he was gentle, attentive, and kind. High moral and religious principle, an affectionate disposition, an instinctive sentiment of delicacy, propriety, and consideration for the feelings of others, and a retiring modesty and simplicity of deportment, as much distinguished and endeared him in the domestic and social relations of life, as his literary and scientific attainments have elevated him to the eminence he held in public estimation. He furnished, indeed, a bright example of the scholar, the gentleman, and the Christian.—*Medical Times.*

THE HOSPITALS OF PARIS.

The General Administration of the civil hospitals in Paris has published an account of its receipts and expenditure in 1847. Those establishments, sixteen in number, contain 17,583 beds. In the course of that year 88,493 patients were admitted into them; namely, in the medical section, 66,048—of whom 27,264 were men, 27,562 women, 5715 boys, and 5507 girls; in the surgical section, 23,292—of whom 14,951 were men, 7083 women, 676 boys, and 582 girls. The number of sick attended in the hospitals was 5800 daily; and the average duration of their sojourn in them was 23 73-100ths days in the medical section, and 31 81-100ths days in the surgical section. The deaths were in the proportion of 1 to 10, namely, 8317—in the medical section 7190, of whom 3004 were men, 2769 women, 718 boys, and 699 girls; and in the surgical section 1127, of whom 658 were men, 319 women, 72 boys, and 78 girls. No less than 214,552 persons were attended or relieved in 1847, namely, 88,493 in the hospitals; 9253 old men and women, 3437 insane, 27,903 foundlings and orphans; of whom 6157 in the *hospices*, and 21,746 out of doors, and 85,468 indigent poor, derived assistance from the *bureaux de bienfaisance*. The expenditures of the Administration amounted in 1847 to 14,970,211 francs, or 1,451,987 francs more than in 1846.—*Medical Times.*

QUACKS IN LONDON.

THERE are five of this fraternity in the metropolis, preying by wholesale on the public purse, whose joint estates cannot be less than a million sterling.

THE VEGETARIAN SOCIETY.

THERE is a society in existence under this title, the members of which abstain from eating meat and drinking intoxicating beverages. The annual meeting was celebrated a short time since by a public dinner at Manchester, and 232 persons (many of whom had been abstainers from animal food for periods varying from twenty to forty years) sat down; Mr Joseph Brotherton, M.P., was in the chair. The following is an account of the courses served up:—First course—Large savoury omelet, vegetables, rice fritters, vegetables, beet-root; onion and sage fritters, vegetables, savoury pie; mushroom pie, vegetables, bread and parsley fritters, beet-root, vegetables, force-meat fritters, vegetables, large macaroni omelet: water the only beverage. Second course—Plum pudding, moulded rice, almonds and raisins, cheesecakes, figs; custards, grapes, flummery; sponge cakes, flummery, gooseberries, creams, nuts, cheesecakes, red and white currants, moulded sago, fruit tarts; water the only beverage.

BOOKS RECEIVED.

1. Taylor's Medical Jurisprudence. Third edition. 12mo. London. 1849.
2. Hope on the Diseases of the Heart and Great Vessels. Fourth edition. 12mo. London. 1849.
3. Stilon on the Cholera in Malta. Translated by Seth B. Watson, D.M. 12mo. London. 1848.
4. Day on the Domestic Management and Diseases of Advanced Life. 8vo. London. 1848.
5. Quain's Anatomy. Fifth edition. By Dr Sharpey and Mr Quain. Part III. 8vo. London. 1848.
6. Ellis's Demonstrations of Anatomy. Part I. 12mo. London. 1849.
7. Die Diagnostik Verdächtigen Flecke in Criminal-Fällen. Von Carl Schmidt. 8vo. Mitau und Leipzig. 1848.
8. Die Krankhafte Erweichung und Durchlöcherung des Magens und Darmkanals. Von Th v. Dieterich. Mitau und Leipzig. 1847.
9. Handwörterbuch der Physiologie. Von Rudolph Wagner. Achtzehnte Lieferung. 8vo. Braunschweig. 1848.
10. Ethnological Journal. No. VIII. 8vo. London. 1849.
11. Bennett on Cancerous and Cancroid Growths. 8vo. Edinburgh. 1849.
12. Tomes—Lectures on Dental Physiology. 8vo. London. 1848.
13. Mayne on Scientific Nomenclature. 8vo. London. 1849.
14. Aitken on Inflammatory Effusions into the Substance of the Lungs. 8vo. Edinburgh. 1848.
15. Tilt—The Serpentine "as it is" and "as it ought to be," and the Board of Health "as it is" and "ought to be." 8vo. London. 1848.
16. Winn's Critical Treatise on the General Paralysis of the Insane. 8vo. London. 1848.
17. Boddy on Diet and Cholera. 8vo. London. 1848.
18. Tyler's Remarks on the Forceps, with Illustrative Cases. 8vo. Manchester. 1848.
19. Gilbert on a less Painful and more Scientific Method of extracting Teeth. 8vo. London.
20. Mémoires de l'Académie Royale de Belgique. Second Fascicule du Tome I. L'Emploi Thérapeutique des Ferrugineux; par M. le Docteur E. A. Jacques. Brussels. 1848.
21. Directions for the Management of Cholera in the absence of Medical Advice. By Henry M'Cormac, M.D. Belfast. 1848.
22. Practical Pharmacy. By Francis Mohr, Ph. D., and Theophilus Redwood. London. 1849.
22. Chloroform in Dental Surgery. By J. C. Clendon. London. 1849.
23. Walker on the Cure of Ulcers by Fumigation. 8vo. Plates. London. 1848.
24. Cook on the Teeth. 12mo. London. 1848.
25. Fownes—Chemistry as Exemplifying the Wisdom and Goodness of God. 12mo. London. 1849.
26. The London and Provincial Medical Directory. 12mo. London. 1849.
27. Braithwaite's Retrospect of Practical Medicine. 12mo. London. 1849.
28. Freke's Reflections on Organization; or, Suggestions for the Construction of an Organic Atomic Theory. 8vo. Dublin. 1848.
29. Tanner's Memoranda on Poisons. 16mo. London. 1848.
30. Skiers on a Popular Treatment of Diarrhœa and Dysentery. 8vo. London. 1849.

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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Pathology and Treatment of the Deafness attendant upon Old Age; illustrated by Dissections and Cases.* By JOSEPH TOYNBEE, F.R.S., Fellow of the Royal College of Surgeons of England, and Surgeon to the St George's and St James' Dispensaries, London.—(*Continued from* p. 525.)

DISSECTION VI.—*Membrana Tympani very concave; Adhesions in Tympanic Cavity.*

MRS F. O. died of gangrene at the age of sixty-two. She had been deaf for some time, especially in the left ear.

Right Ear. The membrana tympani is unusually concave, and in consequence its internal surface, about the central part, is not more than a quarter of a line from the promontory. The membrane is also, in parts, rather opaque, especially at its circumference; its internal layer is white and slightly thickened. The mucous membrane of the tympanic cavity is rather thicker and more vascular than is natural, and it is very tough. A firm band of adhesion connects the cervix of the malleus to the long process of the incus, and another membranous band of adhesion connects the anterior surface of the long process of the incus with the promontory and with the stapes, which latter bone it completely envelopes. The tensor tympani muscle is of diminished size.

Left Ear. Although the surface of the *membrana tympani* is smooth, it is very white around the line of attachment of the malleus; the blood-vessels are enlarged, and they are much distended with blood. The *membrana tympani* is much more concave than natural. The *cavitas tympani* is three parts filled with a thick, tenacious, white mucus, which is partly the cause of the white appearance of the membrana tympani, though the inner layer of the latter membrane, being opaque and white, aids in producing this effect. The mucous membrane lining the tympanum is thick and very vascular, and that portion of it which covers the body of the incus is red, the vessels being completely distended with blood. The upper wall of the tympanum is very thin and translucent; the blood-vessels contained in it are distended with blood, and connect the vessels of the mucous lining of the tympanum with those of the *dura mater;* the latter membrane being more vascular than natural. The

tensor tympani muscle is atrophied, and tension of the membrana tympani is not produced by pulling it. The concavity of the membrana tympani seems to have been produced by a gradual contraction of the tensor tympani muscle.

DISSECTION VII.—*Mucous Membrane of Tympanum thick; Adhesions in the Tympanic Cavity; Labyrinth diseased.*

Mrs L. B. died of consumption at the age of eighty. In each ear the membrana tympani is somewhat thickened, and membranous bands of adhesion connect the incus and the stapes with the walls of the tympanum. The vestibule is full of a rather turbid fluid, and the membranous labyrinth is thicker than natural.

DISSECTION VIII.—*Membrana Tympani opaque; Mucous Membrane of Tympanum thick.*

Mr S. D. died of inflammation of the bowels at the age of seventy-four. He was slightly deaf for a short time previous to his death.

Right Ear. The membrana tympani is rather opaque and thick about its central part.

Left Ear. The membrana tympani and the mucous membrane of the tympanum are thickened.

DISSECTION IX.—*Membrana Tympani white; Mucous Membrane of Tympanum thick; Carotid Canal partially closed.*

Mrs D. E. died with dropsy at the age of sixiy-nine. She had been deaf for some time previous to her death.

Right Ear. The membrana tympani is white, and there is a quantity of thick mucus in the cavity of the tympanum, the mucous lining of which is also white.

Left Ear. The membrana tympani is thicker than natural. The mucous membrane lining the tympanic cavity is thick, and the latter is full of mucus. The canal for the carotid artery is partially closed by an enlargement of its osseous walls, and the *dura mater* over the petrous bone is thick.

DISSECTION X.—*Membrana Tympani white; Mucous Membrane of Tympanum very thick.*

Mrs E. B. died at the age of sixty-eight; she had for some time been deaf in the right ear.

Right Ear. The membrana tympani is white, and the mucous membrane of the tympanic cavity is so thick as to bury the stapes, and nearly wholly conceal it.

DISSECTION XI.—*Collection of Cerumen.*

Mrs L. R. died of asthma at the age of seventy; she had been deaf for several months in both ears. The only pathological condition discovered was a collection of cerumen in the external tube of each ear.

DISSECTION XII.—*An Orifice in the Membrana Tympani; Ulceration of the External Meatus.*

Mr M. S. died at the age of seventy; he had been deaf in the right ear for a considerable period previous to his death.

Right Ear. The membrana tympani presents a large orifice, which is evidently the result of ulceration. There is also ulceration of the membranous lining of the external tube. The mucous membrane of the tympanum is healthy.

DISSECTION XIII.—*Mucous Membrane lining the Tympanum very thick; Bands of Adhesion in the Tympanic Cavity; Membrana Tympani concave.*

Mrs N. O. died at the age of ninety; she had been deaf in the left ear many years previous to her death.

Right Ear. The mucous lining of the tympanic cavity is thick, and the stapes is concealed by adhesions.

Left Ear. The membrana tympani is so much drawn inwards as to be nearly in contact with the promontory; where it looks towards the external meatus, it is so concave that a pea would lie in it. The tympanic cavity is full of bands of adhesion, which encircle the ossicula and connect them with the internal wall; the cavity of the vestibule appears to contain less fluid than natural.

Dissection XIV.—*Orifice in Membrana Tympani; Mucous Membrane of Tympanum thick.*

Mrs D. S. died of fever at the age of seventy-three. She stated that so long as she could remember she had been troubled with a discharge from the right ear; she had become deaf in the left ear only at a recent period.

Right Ear.—The membrana tympani presents a large orifice at its central part; the remaining portion is thick and white. The mucous membrane lining the tympanic cavity is very thick, and is concealed by purulent matter. The stapes is adherent more firmly than natural to the circumference of the fenestra ovalis.

Left Ear.—The membrana tympani is thick, opaque, and softer than natural. The mucous membrane of the tympanum is thick, and the cavity contains mucus.

Dissection XV.—*Membrana Tympani white and thick; Mucous Membrane of Tympanum thick; Stapes anchylosed to Fenestra Ovalis.*

Mrs G. C. died from gangrene senilis at the age of seventy-nine; she had been deaf for several years; the disease commenced by a succession of attacks of earach.

Right Ear. The membrana tympani is white, and thicker than ordinary parchment, to which it bears a great likeness. The mucous membrane of the tympanic cavity is thick, and the base of the stapes is firmly anchylosed to the circumference of the fenestra ovalis.

Left Ear in the same state.

Dissection XVI.—*Membrana Tympani opaque and adherent to Promontory; Mucous Membrane of Tympanum thick.*

Mr R. D., æt. sixty-five, died of dropsy. He had been deaf for some time previous to his death.

Right Ear. The membrana tympani is rather opaque; and it is connected to the stapes by membranous bands.

Left Ear. The meatus externus is full of cerumen mixed with epithelium. The membrana tympani is white, thick, and drawn inwards so as to be very concave, and its internal surface is in contact with the promontory, to which it is attached by firm adhesions. The cavity of the tympanum is nearly obliterated by the approximation of the membrana tympani to the inner wall of the tympanum. The stapes is entirely concealed; the tendon of the tensor tympani muscle is completely enveloped by the thick membrane, and the substance of the muscle is much atrophied. The incus has been removed by absorption, and part of the malleus has also disappeared. The upper wall of the tympanum is very thin, the bone is brittle, and the dura mater separates from the bone with greater facility than natural.

Dissection XVII.—*Membrana Tympani soft and concave; Mucous Membrane of Tympanum thick; Petrous Bone diseased.*

Mrs F. B. died from pneumonia at the age of seventy. She had been deaf in the right ear for some time previous to her death.

Right Ear. The external meatus is filled with a dark-coloured thick pus, which seems to have been secreted by the external surface of the membrana

tympani. The membrana tympani is very soft, and so concave that its inner surface is in contact with the inner wall of the tympanic cavity and the stapes, to both of which it is firmly adherent. The malleus is but slightly connected to the membrana tympani; the long process of the incus and its orbicular process have been wholly absorbed. The mucous membrane of the tympanum is very thick. The upper bony wall of the tympanum is thick and diseased, showing the effects upon it of chronic inflammation of the mucous membrane lining its inferior surface. From its cranial surface a small rough exostosis projects, to which the dura mater is very firmly adherent. The posterior surface of the petrous bone also shows symptoms of chronic inflammation. The mastoid cells contain a large quantity of dark-coloured mucus.

Left Ear. The membrana tympani has commenced to undergo calcareous degeneration.

In the following case it could not be ascertained whether the patient was deaf during life, but there can be very little doubt that she was so; the particulars of the dissection are so interesting, that I think it right to insert them at length.

DISSECTION XVIII.—*Adhesions in Tympanic Cavity; Stapes more firmly adherent than natural to the Fenestra Ovalis.*

Mrs E. N. died of phthisis at the age of sixty-five.

Right Ear. A transparent band of adhesion connects the membrana tympani posteriorly with the long process of the incus; and the base and crura of the stapes are joined by numerous firm bands to the circumference of the *fossa fenestræ ovalis.*

Left Ear. The inner surface of the membrana tympani is connected with the long process of the incus and with the head of the stapes, by transparent but firm bands of adhesion, which nearly conceal the stapes, and firmly bind it to the promontory. The membrane surrounding the articulation of the incus and the stapes is so thick and tough, that the two bones required for their separation considerable force. Besides the adhesions connecting the stapes to the promontory, the membrane connecting the base of the stapes to the fenestra ovalis is much more firm than natural; so that, in endeavouring to separate the stapes by means of the forceps, the crura were fractured, and the base of the bone remained firmly adherent to the fenestra ovalis.

In addition to the dissections, the particulars of which have just been related, I have made others of the ears of fifty-one persons, who were more than seventy years of age at the time of death. Of these one hundred and two ears, *sixty-two* were in a diseased, and *forty* in a healthy state. As these dissections are included among the 915 which form the groundwork of a paper [1] now before the Medico-Chirurgical Society of London, I can in this place merely state, that the principal pathological condition presented by them was a thickening of the mucous membrane lining the tympanic cavity.

TREATMENT.

It being now established by dissection that the most frequent pathological condition in the ears of elderly deaf persons consists of a thickened state of the mucous membrane, the presence of bands of adhesion, and a thickened condition of the membrana tympani—

[1] Pathological Researches into the Diseases of the Ear.

any one of which circumstances is sufficient to prevent the passage of sonorous undulations from the membrana tympani to the expansions of the auditory nerve—it is highly important to inquire whether any remedial measures can be suggested which will tend to diminish these diseased conditions, and consequently improve the power of hearing. Practical experience induces me to believe, that not only may the thick membrana tympani be relieved, but the thickened mucous membrane be so reduced, and in some cases the bands of adhesion so far relaxed, that their presence will offer scarcely any impediment to the function of hearing.

The local application most suitable for this purpose which I have tried is that of a solution of argenti nitras, of a strength varying from half a drachm to two drachms of the salt to an ounce of distilled water. Proceeding from the exterior orifice of the meatus externus, the passage may be touched to an extent varying from one-half to two-thirds of its length every third or fourth day. In some cases the membrana tympani also may be washed with a solution of argenti nitras, of six grains to the ounce. Where the noises are loud, and the symptoms indicate much congestion in the ear, leeches should be applied immediately *below*, not *behind*, the ears; and, where there is irritation of the external tube, an ointment, composed of half a drachm of pulvis cantharidis added to an ounce of simple ointment, and applied behind and below the ear, either daily or every other day, will be found beneficial.

The administration of alterative doses of pilula hydrargyri, hydrargyrum cum creta, or the hydrargyri bichloridum, is very useful; but it must be always recollected that these doses ought to be so proportioned, that neither debility nor any other unpleasant symptom shall be produced; in other words, so gentle should be the alterative, that no sensation should suggest to the patients that they are under a course of medicine.

In addition to the medicines recommended, patients should be cautioned to avoid warm close rooms, and sitting very near the fire; no wine should be taken unless diluted with water; daily exercise, and where possible on foot, should be taken in the open air; together with a warm bath every week or ten days. This course of treatment has been productive of the greatest advantage in several cases of deafness of a most unpromising character. All the cases which I shall now proceed to cite, are those of patients who considered old age to be the cause of their failure of hearing, and some of them were older in constitution than the mere statement of their years would indicate.

Case II.—R. B., Esq., aged eighty, in tolerable health, consulted me on the 29th March 1844, on account of deafness in both ears. He stated that, three years before, the power of hearing began gradually to decline in the right ear, and had continued to do so up to the time of consultation, that, about six months previously, the left ear had been similarly affected, and that his deafness had so much increased as to disable him from hearing the

voice without the aid of a speaking trumpet. He was unable to assign any cause for the deafness. Upon examination, the membrana tympani in each ear was observed to be dull and opalescent, and although, by aid of the otoscope, the air was heard to pass into the tympanic cavities, yet it did so with a bubbling crackling sound, indicating obstruction. Two grains of *pilula hydrargyri* were ordered to be taken every night, and a stimulating liniment to be applied around and below the ears. This plan having been persevered in for about three weeks, and some slight improvement experienced, the patient was directed to take one grain of *hydrargyrum cum creta* daily; and at the end of two months this gentleman recovered his hearing, and gave up the use of the speaking-trumpet.

CASE III.—J. P., Esq., aged sixty-four, consulted me in July 1845. During the last four or five years the right ear has been growing deaf, and the deafness is now so far advanced as to render the ear useless to him. Has been suffering from a cold for a few days, during which there has been a sensation of singing and of vibration in the head and ears, accompanied with deafness. In each ear there was a large collection of wax, on the removal of which the symptoms somewhat abated. The membrana tympani of both ears was white. Air passed freely into the tympanic cavities. The fifteenth of a grain of *hydrargyri bichloridum* thrice a-day was prescribed, and counter irritation around the ears. In the course of six weeks the patient had perfectly recovered.

CASE IV.—Lady R., aged sixty-two, consulted me in Dec. 1843, for a deafness which had come on during the preceding month, and gradually increased, till, by the time I saw her, it was requisite to speak loud and close to the ears. The deafness had been first perceived after a cold, and was accelerated by an attack of influenza. The feeling in the right ear was that of a veil hanging over it. In each ear the *membrana tympani* was white, and air passed freely into the tympanic cavities. The treatment pursued consisted in the application of a solution of *argenti nitras* to the outer half of the external meatus; beginning with the strength of a drachm of the salt to an ounce of distilled water, afterwards increasing it to double that strength, and occasionally applying the *argenti nitras* in the solid form. This course of proceeding, coupled with the administration of alterative doses of *pilula hydrargyri*, effected so great an improvement, that in two months this lady had no difficulty in hearing in ordinary society.

CASE V.—Mrs A. T., aged sixty-seven, consulted me in April 1845. She stated, that when eight years of age she fell down on the *left ear*, and had been deaf of that ear ever since. About four years ago, loud internal noises disturbed the right ear, and increased to so distressing a degree, that this lady felt as if she were continually travelling in a carriage over gravel; at times a loud explosion would be heard, succeeded by acute pain. She can scarcely hear her own voice, and is obliged to make use of a trumpet in society. The ears seem to her stopped up with pegs. She attributes this deafness to a close attendance upon the sick room of her husband during a long illness.

Right Ear. Membrana tympani concave, and evidently nearer to the promontory than is natural, and the membrane is so white that the malleus is not distinguishable.

Left Ear. Membrana tympani has been entirely removed by ulceration.

TREATMENT.—In the first place, leeches were placed immediately below the ear; tincture of iodine was applied to the external meatus of the right ear; and three grains of *pilula hydrargyri* were given every night.

June 3.—Feels much better; has less confusion in the head, and more confidence in herself.

June 15.—The noises are so much diminished, that she is no longer troubled by them; is feeling stronger and better, and the hearing is improved.

Case VI.—Mrs T., aged seventy, applied to me in May 1845. Has been deaf for the last two or three years, especially in the *right ear*. The deafness came on with a feeling of pulsation, and was increased by travelling on a railway. There is a constant singing in the right ear.

Right Ear. Hearing distance[1] half an inch. External meatus slightly tumified. *Membrana tympani* white like parchment.

Left Ear. Hearing distance one inch. Membrana tympani quite white.

I had not an opportunity of watching the progress of this case, but it is introduced here to show the common condition of the membrana tympani in senile deafness.

Case VII.—J. C., Esq., aged sixty-four, consulted me in November 1844. His father became deaf at the age of fifty, and he has a sister deaf. About a year ago, he found that he was deaf in the left ear; might have been deaf a longer time; but at the period mentioned, a singing commenced in the left ear which has continued without intermission ever since. Occasionally it is much diminished. The noise and deafness are both worse during a cold. The right ear is not so bad as the left. When he closes the right ear he cannot hear any sound naturally.

Right Ear. Membrana tympani opaque; the handle of the malleus is only just discernible. When air is forced, the otoscope enables air to be heard entering the tympanum in a series of small puffs. After the air has been forced into the tympanum, a cracking sensation is experienced. Hearing distance two inches.

Left Ear. Membrana tympani white; handle of malleus not discernible; air enters the tympanum in a short puff. Hearing distance, absolute contact.

I prescribed for this gentleman two grains of the *pilula hydrargyri*, to be taken every night, and tincture of iodine was applied behind the ears. In the course of three months I saw him again, and found the hearing decidedly improved; the noises also had much diminished.

Case VIII.—Mrs R. N., aged sixty-four, consulted me August 2, 1844. For the preceding four or five months deafness had been coming on, and had lately so much increased, that she finds it difficult to hear any conversation. Has for several years been subject to occasional dulness of hearing. The present deafness was apparently produced by an attack of cold, which left a sensation of fulness in both ears. The membrana tympani of each ear is quite white.

Treatment.—One-twentieth of a grain of *hydrargyri bichloridum*, in conjunction with *vinum ferri*, was administered three times a-day. The dose of bichloride was subsequently increased to one-sixteenth of a grain, and a solution of *argenti nitras*, half a-drachm of the salt to an ounce of distilled water, was applied to the outer half of the external meatus. In the course of three months this patient recovered her hearing, and has remained quite well ever since.

In the above paper I have only adverted to the more frequent causes of deafness in elderly persons. There are other cases in which the stapes becomes more or less firmly anchylosed to the margin of the fenestra ovalis, and in which little or no relief can be anticipated; these cases, however, as far as my experience enables me to judge, are comparatively rare.

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[1] I have used a watch for several years which is heard distinctly at a distance of three feet by a healthy ear: with this watch I have always calculated the hearing distance.

ARTICLE II.—*Contributions to Toxicology. Cases of Poisoning, with Remarks.* By DOUGLAS MACLAGAN, M.D., F.R.S.E.—(Continued from p. 393.)

Symptoms of Irritant Poisoning; Death; Failure of Chemical Analysis to detect Poison.

A TRADESMAN and his wife came to reside in a Scottish manufacturing town, where the husband possessed some property. They had with them their only child, a daughter of twenty years of age. The father was a confirmed sot, and drank himself to death. The mother was a drunken, violent-tempered woman. The mother having left her husband and daughter in Scotland, went to a distant part of the kingdom, from which she suddenly returned on Christmas day, on hearing that her husband had made a will, in which he had bequeathed all his property to his daughter. She was drunk when she arrived, and was hardly within the house when she began to discharge upon her husband and daughter a torrent of the foulest abuse, and subsequently had recourse to blows. Her violence on this and other occasions was such, that her daughter threatened to "swear the peace" against her; and her husband, in a confidential conversation with his man of business, stated his belief, that the ill-will of the mother towards the daughter was such, that he did not consider the girl's life to be safe. Among other expressions regarding her daughter, she threatened to one witness to "swinge" her; and in presence of another she said to her daughter, that her father, who was in very bad health, would to all appearance be dead before to-morrow, and "she wished she saw her stretched alongside of him." The mother had been drinking a little, but was not intoxicated when she said this. The parties continued to drink and quarrel as before; and on new-year's day the father and mother removed to a fresh lodging, the daughter continuing to live in another house, where she had resided from Christmas day. She went, however to her parents' lodging, and took all her meals with them.

On the morning of the 4th of January, the girl as usual went to breakfast with her parents. She herself prepared, in a frying-pan, some finnon haddocks for the breakfast, which, besides these, consisted of tea and bread and butter, and was taken about nine A.M. By the concurrent testimony of several witnesses, she was that morning in excellent health and spirits, and shortly after breakfast she went to a distant part of the town to get some vegetables for dinner. When, however, she was in the house where she got the vegetables, at eleven A.M., she complained of a burning pain in the stomach, and looked pale; but as she frequently had a pale aspect, the person to whom she made this complaint thought nothing of it. This, however, seems to have been the beginning of the fatal illness. She came home with the vegetables and cleaned them; but when she

had finished doing so, she came into the room where her parents were, and complained of severe bellyach. The chief witnesses of her illness were her mother, who was the party accused of poisoning her, and her besotted and enfeebled father, neither of whom could thus furnish trustworthy evidence. The following notes, made at the time of the investigation of the case, and drawn up by the Sheriff-substitute of the district, give the most correct account that can be obtained of the symptoms:—

The girl was seen in robust health and high spirits at nine o'clock on Tuesday morning, 4th January. After that breakfast was prepared, and she is *supposed* to have breakfasted upon tea, bread, and butter, and the whole of a small finnon haddock.

She was seen again at or before eleven o'clock still in excellent spirits, but complaining of a pain in her stomach. Shortly after this she was seized with retching, and became so ill as to go to bed. She complained of a burning pain about her heart or stomach. This pain was increased by pressure.

She seems to have had some purging during the forenoon. A witness, who saw one stool, describes it as resembling what might be produced by a person chewing grass and spitting the juice of it out, being greenish liquid, and with foam on the top of it.

She was seen by a medical attendant going to stool about three o'clock, when the result was, half a tea-spoonful of mucus, tinged of a brick-dust colour, he thought by blood. She was then affected by tenesmus, and there is no proof of any diarrhœa after that time.

During her whole illness she was affected with intense thirst, and constantly drinking, principally water, but also immense quantities of ginger beer: she had also some whisky, whisky toddy, hot ale with pepper in it, brandy and rum made into toddy.

Every thing she swallowed was immediately discharged, and the medical man who attended her, saw her also vomit repeatedly a greenish or yellowish bilious fluid.

She died between eight and nine o'clock on the morning of Wednesday the 5th, and the vomiting continued down to the time of her death. The woman who attended her says, that her vomitings continued getting greener and greener to the last.

She once or twice, on the evening of Tuesday, showed decided symptoms of delirium, but these seem to have continued only for a very short time. From ten o'clock P.M., on Tuesday, she suffered from cold, and had to be lifted up to the fire to be warmed, having then lost the power of her limbs.

She complained that her heart was smothering—that it was "dipping"—by which she perhaps meant throbbing; but her general complaint during her whole illness was, that her heart was burning, and an incessant demand for liquids of any kind. She is described as having drank a "stoupful" of water during the night, and vomited the whole of it.

She was not seen by a medical man except on Tuesday, at three o'clock P.M., and then the expression of her countenance was rather anxious.

He prescribed a turpentine cloth for her belly, a calomel powder with opium in it, and also senna, but probably they would be immediately vomited.

The only other circumstance connected with her illness which requires notice is the fact, that about six o'clock at night, *i. e.* about seven hours from the beginning of her illness, the girl left the lodging of her parents, where she had been taken ill, walked across unassisted, though with great feebleness, to her own lodging, and then

her illness went on to its fatal termination. Why her mother should not have accompanied her when she saw her so seriously ill, and why she should not have gone to see her daugher for more than two hours thereafter, was not accounted for by her. The father was so feeble as to require assistance himself, rather than to afford it to another.

The girl's previous health appears to have been generally good. She had shown some slight hysterical symptoms some months previously, and about a fortnight before her death had slight sore throat; but she never had any abdominal symptoms, or other manifestation of disease which could be at all connected with the fatal attack.

The body was examined by two medical men, under the injunctions of the authorities, and the following is the report of the autopsy :—

6th January 1848.

We, the undersigned, certify that we have this day examined the body of —— —— (*post-mortem*), and that the following appearances were observed :—

Externally, the body presented a plump healthy appearance, and on the depending portions extensive sugillations were observed. On opening the thorax the lungs were found healthy, except that they were considerably congested, and of a darker red colour than natural; the heart and pericardium were quite healthy and natural in appearance, as were also found the auricles and ventricles, on being opened. On opening the abdominal cavity, the intestines were observed moderately distended, and externally of a healthy aspect; eight ounces of a reddish-brown turbid fluid was found effused in the abdominal cavity. The œsophagus being tied, the whole contents of the abdominal cavity were then removed, and the alimentary canal slit open throughout its length. The stomach presented a reddish tinge, more or less deep, over the whole of its mucous membrane, and at the large curvature where this tint was deepest, several patches of a bright red colour, apparently from blood effused under the mucous membrane, were observed. The stomach contained several ounces of a brownish-green fluid.

The mucous membrane of the duodenum, throughout its entire length and over its whole surface, was found covered with patches partly of a reddish colour, and varying from that to a brown and almost to a black colour, the prominent rugæ, in particular, presented these appearances; the duodenum was filled with a dark-brown fluid. The jejunum, for four inches, presented a vascular appearance, the blood-vessels being congested and arborescent, the rest of it was apparently natural, as was the ileon, except for four or five inches where it joins the colon, where it presented the same arborescent appearance of the blood-vessels as was observed at the commencement of the jejunum. The arch of the colon presented very great vascularity, the vessels having a strikingly arborescent appearance. The rectum was healthy. The jejunum and ileon contained yellowish feculent matter; the large intestines were nearly empty, containing only a little mucus. Two lumbrici were found in the small intestines.

The bladder was empty, and healthy in appearance, as were the uterus and appendages; the kidneys, liver, spleen, and pancreas were also quite healthy.

The tongue with the œsophagus and trachea were now removed, the lining membrane of the œsophagus presented only some slight reddish streaks, but the mucous membrane of the fauces, that covering the epiglottis, and that lining the trachea, were found highly congested, and of a very deep red colour.

On opening the head, the brain and its membranes were found quite healthy.

From the above appearances, we are of opinion that in this case death was caused by the action of some irritant poison.

To this it is proper to add, that although no allusion is made to

it in this report, the inspecting medical men made very special search for perforation of the stomach or intestines, as was stated by them in precognition; and when the viscera were subsequently sent to Dr Anderson and myself for analysis, we carefully inspected every part of the alimentary canal, and could find no appearance of any perforation.

Moreover, the fluid found in the peritoneal cavity was stated by the medical men to be quite distinct in appearance from the contents of the stomach and bowels. I had, however, no opportunity of judging of this fact for myself; for unfortunately, at the *post-mortem* examination, in putting the contents of the stomach into a bottle, a small proportion of this had been accidentally poured into the vessel which at the moment contained the fluid from the peritoneal cavity.

The various articles contained in the following enumeration, were sent to me for analysis; and my colleague, Dr Thomas Anderson, was associated with me in the investigation.

1. Bottle containing the effused fluid from peritoneal cavity, mingled with part of contents of stomach. 2. Bottle with remainder of contents of stomach, and contents of duodenum. 3. Contents of intestines. 4. Bladder containing tongue, œsophagus, trachea, stomach, and duodenum. 5. Remainder of intestinal canal. 6. A piece of cloth on which the girl had vomited, but which had subsequently been washed. 7. A shift partly wetted by vomitings. 8. A bottle of tea stated to have been made for the girl by her mother, and given to her.

The result of our chemical examination is contained in the following report by Dr Anderson and myself:—

3, Surgeon Square, Edinburgh.
22*d January* 1848.

We have examined the articles sent to us for analysis in the case of —— ——, and have to report thereon:—

The bladders, Nos. 4 and 5 of inventory (which with the other articles were all duly sealed, the seals unbroken), were found to contain the stomach, intestines, and other organs, as specified on the labels. The appearances presented by them corresponded with the description given in the report of the *post-mortem* examination by Drs —— and ——, with this exception, that the dark coloration of the duodenum mentioned by them had disappeared. The appearance of the stomach was such as, in our opinion, to warrant the conclusion that the deceased had taken irritant poison. In our chemical researches we confined our investigations to poisons of this class.

We have to state, that in none of the articles sent to us, have we been able to detect any poison which could account for the morbid appearances observed by us, or the symptoms, so far as we have been informed concerning them.

We do not think it necessary to detail all the steps of the chemical processes by which we have arrived at this negative result; but we have to state, that we have applied in vain, both to the contents of the bottles and to the tissues of the stomach itself, all the most approved and delicate tests for oxalic acid, arsenic, corrosive sublimate, copper, lead, zinc, and the corrosive mineral acids. The latter, however, were excluded from such a case by the morbid appearances, and the impossibility of their being administered as poison to an adult.

We have to state, that in our search for corrosive sublimate we found, on testing for the second time a portion of No. 2, "Remainder of the contents of the stomach, and contents of the duodenum," we separated from it a small portion of mercury. This at first led to the supposition that corrosive sublimate was present. But seeing that, on repeating our experiments several times with the same, and subsequently with more delicate tests, we could find no more mercury, we came to the conclusion that what we had found was probably derived from calomel, which, being perfectly insoluble, might have happened to be attached to some of the solid particles, and therefore not found in the rest of the fluid. This opinion has been confirmed by our finding no traces of mercury in the tissues of the stomach itself, and by the fact subsequently communicated to us, that calomel had been administered to the deceased. It is proper, however, to add, that there are no chemical means of positively distinguishing calomel from corrosive sublimate, when the latter has been long in contact with animal matters.

We also found some globules of oil floating on the contents of bottles Nos. 1 and 2. The only substance of this nature which might have caused irritant poisoning is croton oil; but what we examined, being completely soluble in its own volume of cold alcohol, was not croton oil, but seemed to us to be castor oil.

Our experiments having thus failed to afford any evidence of mineral poison, and retaining our belief, as deduced from symptoms and *post-mortem* appearances, that this was a case of irritant poisoning, we are of opinion—either that if mineral poison was administered, it has been rejected by vomiting in an early stage of the case, and so lost; or that the poison may have been a vegetable or animal acrid substance. We know of no tests which could be depended on to detect, much less to identify such matters, and therefore have not specially searched for them. Moreover, as they are generally substances possessed of strong acrid or bitter taste, they are not easily swallowed unconsciously.

We have also examined some tea sent to us subsequently, likewise a piece of rag, and a shift belonging to the deceased, but have been unable to extract any poisonous substance from them.

We have not used the whole of the articles in our experiments, and the remainders of them have been sealed up and preserved by us, that crown counsel may, if they wish it, have them further examined by us or by any other analyst.

ANDREW DOUGLAS MACLAGAN, M.D.
THOMAS ANDERSON, M.D.

It is unnecessary to enter into any details as to the processes which were followed by Dr Anderson and myself in our chemical investigation. We followed all the most approved and delicate methods of analysis known to us, and most of our experiments were performed twice. Dr Christison, who was subsequently consulted on the case, could suggest no other methods of investigation than those which we had followed; but, on his recommendation, the liver and kidneys were subsequently procured, and examined by us for arsenic, mercury, and copper; but neither of these, nor of any other metallic poison, could a trace be detected.

In this case we were deprived of one important subject of research, from which, in all probability, some more certain evidence might have been obtained. The vomitings were all thrown away. No attempt seems to have been made to preserve them; but it does not appear that any particular care was taken by the mother, or by any

other party, to get them out of the way. At all events, that was certainly not done in defiance of any instructions from the medical man who saw the girl during her illness. He does not, in fact, seem to have entertained any suspicion of poisoning at the time of his visit.

The above case was never brought to trial. The want of chemical proof; the uncertainty of evidence from symptoms and *post-mortem* appearances alone; and the absence of any known facts which could bring home to the party accused the possession of poison, rendered it hopeless to look for a conviction.

In the absence of positive proof, what explanation are we to give of the above case? Was it a case of death from natural disease simulating poisoning, or was it really a case of poisoning where chemistry failed to detect the cause of the symptoms? That the latter is the true interpretation I have no doubt. There was the general evidence of poisoning, both moral and medical. That part of the former, which directed suspicion to the particular party accused, does not concern our present purpose. It is enough for us to bear in mind, that there was a party present who bore an ill-will to the deceased, had motives for wishing her dead, and had expressed a wish to that effect, and which party was of depraved habits and violent temper. The proof of administration is certainly defective. It could only be at the breakfast that the poison could have been given, and the daughter, mother, and father took this meal at the same table, and at the same time. It is true, that the father stated that the mother poured out the tea, and that she gave an entire finnon haddock to her daughter, of which the latter alone partook; but the enfeebled state of the father's mind and memory, at the time his deposition was taken from him on his deathbed, renders his evidence not trustworthy. It is moreover not easy to see how a fish, cooked by the girl herself, could be made the vehicle for poisoning at the table, without something suspicious on the part of the accused being observed. The tea, which the accused poured out, seems the more likely article in which poison may have been given. The tea, in which Dr Anderson and I failed to find poison, was not a portion of that which had been used at the breakfast on 4th January, but the remains of some which had been given to the deceased during her illness on the morning of her death.

If from the moral, we turn to the medical view of the case, we have the general evidence of poisoning very decidedly before us. The deceased is in previous good health and spirits; no evidence of disease, no inducement to suicide. The symptoms commence not long after a meal; they are those which characterise the action of irritant poisons; they commence without rigors or other symptoms premonitory of acute disease; they rapidly acquire a great intensity; and they go on progressively and continuously to a fatal termination in twenty-four hours. The *post-mortem* appearances are consistent with the supposition of irritant poisoning; in some respects are cha-

racteristic of it; and they at all events do not enable us to account for death by natural causes.

If this was not a case of poisoning, of what disease did this girl die? Perforation of the stomach or intestines is looked for, but not seen, and the peritoneum did not contain matters effused from any part of the alimentary canal; nor, indeed, were the symptoms such as are usually observed in cases of this disease. Peritonitis might be supposed to be indicated by the effusion of reddish-brown turbid fluid in the peritoneal cavity; but this, though not common, does appear sometimes to occur in irritant poisoning, and the peritoneum itself presented no other morbid appearance which could warrant the idea; moreover, constant vomiting is not a common symptom, and diarrhœa not only is not usual, but is generally absent in peritoneal inflammation.

The presence of two lumbrici in the bowels will not account for the symptoms, or fatal termination. In all the cases simulating poisoning, which have been supposed to be connected with worms, these parasites have been present in unusually great numbers.

Are we to regard this as an example of acute spontaneous gastritis, of which so much has been written, but so little seen? To use the words of Dr Christison, "the possibility of the occurrence of a case of the kind from natural causes, must be granted;" and in the more recent work of Taylor, three cases are actually quoted which seem to countenance this belief; but it is to be observed, that in these instances diarrhœa was absent, whilst in the present case, besides some powerful source of irritation of the stomach, there was some agent at work operating on the bowels, so as to cause not only bilious alvine discharges, but tenesmus and bloody slimy stools.

It appears to me, then, that the legitimate conclusion in this case is, that the girl died from the effects of an irritant poison; but that this had, which is by no means a common occurrence, been entirely got rid of by vomiting. I have not presumed to form any opinion as to what the individual poison may have been.

66, FREDERICK STREET, EDINBURGH,
February 1849.

ARTICLE III.—*Cases of Obstructed Hernia, with Remarks.* By JOHN GAY, F.R.C.S., Surgeon to the Royal Free Hospital, &c. &c.

THE following is one of a class of cases more numerous than is generally expected, and which presents to the mind of the surgeon anomalies more readily observed than explained. The term "obstructed," was applied by Mr Stephens to the form of hernia under consideration; and the credit is due to that gentleman of having

first defined some of those conditions of the herniated parts which become its distinguishing pathological features. It is, however, to be regretted, that in the zeal for his subject Mr Stephens should have been led to illustrate it by cases, which evidently had not those characteristics in common that would justify their being arranged together in one class; and hence his readers were left without any very definite directions for distinguishing between the symptoms of an obstructed and strangulated hernial protrusion in the living subject. I have said enough to assure my readers, that I disclaim any title to originality in the views I am about to present; and only claim a desire to find for them more general acceptation than they have hitherto received, being fully persuaded that they contain the germs of important practical truths.

Case I.—I accompanied Mr Childs during the past autumn to see a lady, the mother of several children, who had been for seven years subject to an umbilical hernia. She was above fifty, very corpulent, and had for several years been declining in regard to her physical strength. She had not made use of a truss, but had worn a band or belt with the view of restraining any further descent of the abdominal contents. From time to time she had suffered from colicky pains in the neighbourhood of the umbilicus.

Two days before we saw her the bowels were moved; but passing forty-eight hours without her usual recourse to the water-closet, she felt a general sense of discomfort, which led her to suspect some serious cause of obstruction. On our first interview, our patient had little topically to complain of but distension, and that principally of the upper part of the abdomen from flatus. The lower part of the abdomen gave the slightest indications of uneasiness upon *deep* pressure. The hernial tumour was perfectly flaccid, free from tenderness or tension. Pulse 80 in the minute; tongue clean. We ordered a turpentine injection, a few leeches over the lower part of the abdomen, a dose of calomel and opium, and half an ounce of castor oil mixed with white of egg and syrup.

This draught was rejected; and the injection was immediately returned without any appearance of its admixture with fæcal matter. A saline mixture, containing drachm doses of sulphate of magnesia, was now ordered; and hot fomentations over the abdomen. From this period the progress of her symptoms was remarkably rapid. On the following day, the upper part and right side of the abdomen had become enormously tympanitic. The bowels had not been moved, although the injections had been repeated. Pain, referred to the back, came on in paroxysms of so forcing a character, that during their continuance the urinary bladder was emptied of its contents. The pulse had risen to 120. She passed a very large quantity of urine. No change, however, had taken place in the hernial tumour; but such was the general aspect of her symptoms, that her case was pronounced hopeless, and on the following day she died.

Post-mortem examination twenty-four hours after death. Fat over the abdomen at least two and a half inches in thickness. Abdomen enormously distended above the umbilicus. The cœcum and colon were more distended with gas than the small intestines, although these were more than usually inflated. The distension existed in that portion of intestine above and to the right of the umbilical ring. The whole of this portion of the intestinal canal was of a dull colour, and its tissue broke down on the application of a slight amount of force, but betrayed no symptoms whatever that inflammatory action had existed. The hernial mass consisted of thin integument, to which the sac was universally and inseparably united, and a large quantity of omentum, which partially enclosed a knuckle of the transverse arch of the colon. The ring

readily admitted the passage of two fingers by the side of the neck of the herniated viscera; so that these had suffered no constriction. The *omentum* was perfectly healthy; but the *intestine* was flaccid, of a dull colour, evidently going on to sphacelation, *and adherent on one side to the mouth of the sac.* Our examination could not be carried further; but from the obesity of this patient, and the fact that her physical strength had been gradually failing her, so that for the last few months of her life she was unequal to any but the slightest amount of exercise, it might be safely inferred that the heart had suffered from the general tendency to fatty deposition.

The case is one of death from an irreducible hernia without evidences that either inflammatory action or constriction had been its immediate cause; or, as Mr Lawrence well defines it, from an interruption of the function of the protruded bowels without the presence of stricture or inflammation. The inference is, that simple irreducibility is a condition of a hernial tumour replete with danger to the ruptured person.

The circumstances which render a hernial tumour irreducible are not always the same. For the most part the bowel is retained by adhesions to the sac, either mediately or immediately; but this is not always the case, for in several cases of old umbilical hernia which have resisted the usual efforts at reduction, and have therefore been considered irreducible, I have found the knuckle of intestine unadherent, but so enveloped as to be held fast by a portion of omentum which was adherent to the sac, the presence of which had obviously prevented the due effects of the taxis from reaching the intestinal nucleus. But are unadherent intestinal herniæ equally liable to become fatal by "obstruction" as those in which adhesions to the sac exist? From my note-book the following is taken:—

CASE II.—In the month of August 1844, M. O., æt. sixty-three, a *very fat* woman, with an enormous hernia (umbilical) was brought into the Royal Free Hospital for an erysipelatous affection of the leg. Her condition was one of extreme prostration; but from the employment of stimuli, and the application of cotton wool to the extremities, she began to recover. On the third day after admission, however, her bowels became confined. (Opium had been administered with ammonia, on account of the pain and restlessness which constituted, beside the affection of her leg, the principal symptoms at the time of her admission). A mild cathartic was administered, but ineffectually. On the following day the abdomen became tense; she refused her food on account of a distressing sense of nausea which now set in; her pulse became quickened, 110 in the minute; the tongue brown and inclined to be dry; to which succeeded great restlessness. The abdomen was examined, but, on account of her excessive obesity, without any prospect of obtaining therefrom any satisfactory result. The hernial tumour *was found to be perfectly free from pain, and flaccid;* and not to have in the least degree increased in bulk. The hernia had existed for many years; but she had used a concave tin support, kept up by means of a broad bandage of her own construction, which embraced the abdomen. A slight effort was made to reduce it; but from its apparent fixedness, and the supposition that the hernial contents were irreducible, the attempt was not persisted in.

Injections of castor oil and turpentine were given, calomel administered in doses of two grains, with three grains of the extract of colocynth every four hours, as the saline effervescent aperients were found to be objectionable from their bulk.

On the following day her condition was worse. She suffered from constant nausea; and vomiting was produced by every attempt to take either food or medicine. The abdomen was *generally* more distended. The hernial tumour remained, however, still flaccid and free from uneasiness; the pulse had increased to 130; the tongue had become dry; hiccup set in; in short, her death, which took place on the evening of the third day, was obviously at hand.

The *post-mortem* examination disclosed a large knuckle of intestine (a portion of the ascending colon, with the last part of the cœcum) in a state of flaccidity, empty, discoloured, and readily broken up, in the hernial sac. It was every where surrounded by omentum, the tissue of which had evidently become hypertrophied since its incarceration, which was here and there adherent to the sac, but not by very strong bands. The sac was adherent to the adjacent integuments. The mouth of the sac, which exhibited a very firm edge, was large; and from its size could not exert any constriction on the neck of the hernial mass. It was however apparent, that although the *omentum* enjoyed some amount of freedom from some room which still existed between it and the sac, the *bowel* itself was closely confined by the omentum. The whole of the intestinal canal above the umbilical ring was distended with fluid and gaseous matters; and in some parts the small intestines exhibited patches of old, but not of recent, inflammatory action. The peritoneal cavity contained some fluid; the heart was large and very flabby; the lungs emphysematous; the lining membrane of the bronchi in a state of chronic inflammation; the liver large and fatty.

This case clearly demonstrates that not only adhesions, but other causes of limitation to the function of a herniated bowel, may, *under peculiar circumstances*, be a source of fatal obstruction. Mr Stephens made the existence of adhesions *essential* to the production of symptoms characteristic of obstructed hernia; but I think it is clear that the class thus characterised is too limited, and should be made to include cases analogous to that which has just been related. But why are herniæ allowed to become irreducible? Doubtless in most instances on account of personal inattention to them, and of neglect properly to adjust the mechanical means which are used to prevent their descent. But this is not all. The doctrine, that absolute *strangulation* of herniated parts can alone justify recourse to an operation for their reduction, is favourable to that view of an *irreducible* but *not strangulated* hernia, which regards it as less free from peril than is absolutely the case. Thus a hernia becomes suddenly protruded; the surgeon is called and finds it simply incarcerated, but not in a condition to admit of the means being used for its reduction, from the protruded parts being in a state of inflammation. Remedies are successfully adopted for putting a check to the inflammatory action; but lymph has been in the mean time effused, the *materiel* of adhesions between the contents of the sac and the sac itself, and the hernia becomes at length irreducible, and too frequently is permitted to remain so. The axiom should clearly be, that a patient who has an irreducible hernia should be warned of the peril of not having it constantly and effectively supported; and that a recently incarcerated, though not strangulated, hernia should be returned, even by the aid of an operation, rather than be allowed to become irreducible from the subsequent formation of adhesions.

The importance of this remark is still more obvious from the experience of most surgeons in reference to old, especially umbilical, herniæ. Of twenty-three cases of persons dying with old umbilical, ventral, and inguinal herniæ, under my own notice, in nineteen some disease of the chylopoietic viscera was the immediate cause of death, and in seventeen the fatal disease had a still closer relation to the existence of the hernial tumour.

It is of the utmost importance that the indications of this affection should be detected at the outset; for that is the only stage of the complaint during which the necessary remedies can be hopefully employed. Although the incarceration of the herniated knuckle of intestine is the condition of the parts essential to the production of the results we are contemplating, it is obvious that some other circumstances of a casual or more uncertain character are necessary to bring about the obstruction, which circumstances stand in relation to the obstruction as its *immediate* cause. No doubt the ordinary mechanical causes of intestinal obstruction will produce their effects more readily when acting upon a portion of intestine so situated; but *generally* in these cases no such causes are to be found. I think in the majority of instances the immediate cause is a temporary exhaustion, to a greater or less degree, of muscular power, especially of that belonging to the organic system; and this might arise as the result of a very gradual and irretrievable loss of power or tone generally, or of its temporary abatement from such accidental circumstances as are calculated to produce it. Hence, in cases of irreducible herniæ, especially of the ventral and umbilical order, it would be injudicious to administer opium, lead, or strychnine. These agents might not so affect the *free* portions of bowel as to interrupt their peristaltic action, but they might produce in the *incarcerated portion* a complete suspension of its functions. With this view of obstructed hernia, it is obvious that the advance of the case renders it increasingly unfavourable; and that what is requisite to be done for its relief should be done at the earliest part of its history.

What, then, are the diagnostic symptoms of this affection? They have not been clearly defined. A case of "obstructed hernia" is distinguished by the insidiousness of its symptoms, and the absence of pain in the earliest part of its career. There is simply intestinal obstruction; but usually it is attended with a consciousness of danger on the part of the patient. Vomiting may occur, but this, whilst it is in some cases an early symptom, is in others late; and in cases marked by a rapidity of progress, by great exhaustion of the vital powers, and in which the bowel incarcerated happens to be the lowest portion of the large intestine, it may not occur at all.

The hernial tumour is more than *ordinarily flaccid*. This is an important criterion. Its usual fulness is lost, and there is no pain on handling it.

Distension of the abdomen from flatus is a symptom, but not a diagnostic or distinguishing feature of this affection, since it is com-

mon to other forms of hernia, and to visceral obstruction without hernia.

Briefly, then, obstinate constipation, low vital powers, an old and irreducible hernia in an unusually flaccid and painless state, are the positive indications of an "obstructed hernia."

What is to be done? No delay should be suffered when these symptoms coexist. The hernial tumour should be cut down upon, the incarcerated bowel relieved, if necessary, by the breaking up of the adhesions, and the patient's strength revived by the administration of stimuli.

I am aware of the difficulty and danger ordinarily attendant upon such an operation: Arnaud points them out most forcibly, especially when the hernial parts consist of *omentum*, as well as intestine. That forcible writer says:—"Le nombre des inconveniences est si grand, qu'il n'y a que ceux qui ne sont eclairés que par les lumières trop distantes de la speculation a la pratique, qui osent proposer ou entrependre de pareilles operations, pour des hernies habituelles, que l'on peut porter sans danger de la vie;" and subsequently shows that the sources of danger are, "les irritations spasmodiques, la fièvre, les inflammations, et la suppuration qui font peur aux malades, sans qu'il soit possible quelquefois de vaincre ces accidens."

Surgical science, however, has done much to lessen the probability of the occurrence of these accidents after interference with hernial tumours; and I believe that the doctrine of *non-interference with the fasciæ and other parts of the hernial mass not immediately involved in its detention*, if carried out, would render such operations comparatively harmless as far as such consequences are concerned. The cutting and separation of fasciæ in operations for hernia have nothing to do with the *relief of the parts*—they are simply *guides* in the course of the operation, and as such ought to be abandoned. The operation can be performed, and that easily, by a fair anatomist, without such wholesale (as I have often seen them) *lamellar* separations, and would thereby be freed from much of the fatality which at present proceeds from this process, *and this process alone.*

One word about the return of old herniæ which have adhesions to the sac, and which have become sources of pain and other inconvenience. These cases are too frequently regarded as irreducible, and are allowed to remain until more serious evils, in connexion with them, call for interference—when in the majority of instances the treatment, which before might have been successful, cannot be adopted.

The older surgeons, Le Dran and Arnaud, contended that old and adherent herniæ, by caution and perseverance in the employment of suitable means, can be returned; and that it is not necessary for the patient's safety, and restoration to comparative health and comfort, that the adhesions between the viscus and the sac should be destroyed. Arnaud recites interesting cases to this effect—one of

an old woman, sixty-eight years of age, who was quite disabled by an old hernia for twenty years:—"La malade etoit fort agée, et la tumeur très ancienne. Renfermée dans la peau qui formoit la grande lévre, elle occupoit tout l'espace depuis l'anneau jusqu'a la marge de l'anus." The treatment was as follows:—"Elle fut reduite aux bouillons de veau très légers. Elle prenoit tous les jours deux grains de mercure doux. Elle recevoit un lavement emollient, et fut purgée très souvent. On lui faisoit des embrocations huileuses sur le ventre. On lui appliqua un emplâtre de muscilages sur la partie de la tumeur qui n'etoit point ulcéré, et l'on mettoit sur la partie excoriée un linge fin trempé dans une legére dissolution de pierre médicamenteuse faite avec l'eau de sureau. Je lui recommandai d'uriner dans un bassin de lit, sans le lever, de porter la tumeur un peu en dehors d'une main, et d'ecarter avec l'autre la lévre du côte opposé autant qu'il lui etoit possible pour ramener l'urethra à sa direction." The urethra was turned from its usual course by the pressure of the tumour.

To shorten the case. In eight days the size of the tumour was by these means lessened, and on the fifteenth day the whole was returned, and the patient completely recovered.

Modern surgeons have advocated this practice (see Mr Lawrence's admirable treatise), but its value and efficacy have yet to be taken full advantage of.

ARTICLE IV.—*Contributions to Legal Medicine; Reports on Cases of Injuries of the Head, with Remarks.* By J. A. EASTON, M.D., Medical Officer to the Glasgow Police Establishment; one of the Physicians to the Glasgow Royal Infirmary; Lecturer on Materia Medica in Anderson's Institution, &c.

(*Read before the Glasgow Medical Society on* 19*th December* 1848.)

THE following cases of injuries of the head are submitted to this Society, not that the details connected with them offer any thing novel or interesting in a surgical point of view; but because they present some difficulties frequently met with by the medical jurist, and inculcate the procedure which, in my opinion, ought to be followed under similar circumstances.

The first case I shall bring under your notice, is one in which two separate injuries were inflicted on the head; first by a fall on a stony pavement, thereafter by a kick with a heavy iron-pointed boot; and in order to understand the bearing of some of the subsequent observations as to which of these injuries occasioned the death, it is necessary to premise, that the procurator-fiscal in his petition for a remit to surgeons to examine, and for a warrant to commit to prison, when narrating, as is usual, the principal circumstances of the case, charged the accused, Richard Alexander, *alias*

Dublin Dick, with "having knocked down, and kicked on the head, James Shaw, whereby the latter was killed instantaneously." It seems that these young men, who were perfect strangers to each other, met on the street on a Sunday morning, both being slightly under the influence of liquor; that a frivolous expression, on the part of one of them in passing by, led to an altercation; from words they proceeded to blows; but had no sooner commenced to strike, than, getting into grips, they both fell, Shaw being undermost, and his head falling with great force upon the pavement. While Shaw was still on the ground, Alexander, who had on a pair of those ponderous iron-pointed boots so frequently worn by the class of Irish labourers popularly known as "navvies," sprung to his feet, and inflicted a heavy kick behind the right ear of his opponent, who thereupon convulsively drew up his legs, and instantly expired. This part of the narrative, however, I shall give in the words of an eyewitness, partly because they furnish an accurate description of the *res gestæ* of this rapidly conducted tragedy, and partly because they convey the impression which was made on the mind of an ordinary observer, as to the precise act of violence to which, in his opinion, the death was exclusively attributable. "In passing along the head of Drygate Street," says the individual just referred to, "I saw the prisoner and the deceased go into a court, and there found them in grips, and almost immediately after saw both fall, Shaw undermost, his head striking the pavement, and in falling the prisoner came down with all his weight upon Shaw; that the prisoner sprung to his feet, and, before Shaw had time to rise, gave him with all his force a kick on the head behind the right ear. That he believes that Shaw suffered no material harm from the fall, and that it was the kick which killed him." It is almost unnecessary to add, that when the subjoined report was drawn out, the reporters were ignorant of some of the circumstances just narrated; or that even though they had been cognisant of the whole facts, as these were afterwards disclosed, they were bound to form their opinion regarding the cause of death, not from hearsay evidence, but from the pathological alterations which were observed at the *post-mortem* examination.

The medical report was as follows:—

"This is to certify, that, under the authority of a warrant from James Bogle, Esq., one of the magistrates of Glasgow, we, this day, inspected and carefully examined the body of James Shaw, in his mother's house, No. 12, Drygate Street.

"At the lower part of the head, immediately behind the middle of the right ear, and extending downwards more than two inches on the neck, there was a livid recently produced swelling, fully larger than a hen's egg, and presenting at its upper and lower borders two small, recent, and superficial abrasions. The skin and muscles in this situation were contused and infiltrated with blood, partly fluid and partly coagulated—a deviation from natural structure which could only have been caused by violence applied externally, and which, though it could not have been the result of a fall, might have been induced by

repeated blows with the fist, by kicks, or by a heavy blunt instrument impelled with great force against the part.

"On the under side of the scalp, corresponding to the middle of the posterior margin of the right parietal bone, and extending backwards from that point, there was an ecchymosis, or mark of effused blood, of the size of a crown piece in circumference. This injury was also of recent infliction; like the former, it must be ascribed to external violence alone, but might have been caused by a fall on a hard unyielding body, as well as by any of the causes above enumerated as being likely to produce the contused swelling in the region of the right ear and right side of the neck, already referred to.

"On removing the skull-cap and *dura mater*, or outer covering of the brain, we found copious extravasation of blood, both fluid and in clots, throughout the entire extent of the brain, on its surface, at the base, and in its cavities. So universal was the effusion of blood in and around this organ, that we find it impossible to particularize any one portion of it which was completely free from this unnatural appearance.

"While there can be no doubt that this extravasation of blood in the brain, preceded as it likely was by concussion of that organ, was the immediate cause of death, and that such concussion and extravasation were produced by the same violence which inflicted the external injuries behind the right ear, and in the situation of the right parietal bone, it is at the same time impossible to state to which of these injuries such effects should be attributed. Either of them was sufficient to produce that alteration of structure which occasioned the death; combined, they would of course accelerate that crisis; but to which of them the fatal result ought to be attributed, the medical examination did not enable us to determine.

"The contents of the chest and belly were healthy. All this we attest to be true on soul and conscience."

There being this dubiety, therefore, on the part of the reporters regarding the particular kind of injury which occasioned the death, they deemed it advisable to append the following explanatory note:—

"If an indictment be raised against Richard Alexander corresponding to the tenor of the petition of the procurator-fiscal in the warrant of commitment, it may not be improper to consider how far the medical evidence will bear out the allegations which are contained in the aforesaid document. Though in that petition it is stated undoubtedly that the deceased James Shaw was 'knocked down,' yet the principal allegation seems to be, that this person died in consequence of being 'kicked on the head' by the prisoner, Richard Alexander. Now, from the accompanying medical report, it will be observed that there are two injuries of the head described, and that while the reporters are clearly of opinion that the immediate cause of death was copious and extensive extravasation of blood in the brain, and that such extravasation was produced by the same violence which caused the one, or other, or both of the external injuries, they are unable to certify with precision to which of these ought to be ascribed that injury done to the brain which led to the fatal result. But in addition to the circumstance, that the medical evidence could not bear out that the death had been caused by the particular description of mal-treatment more especially set forth in the petition of the public prosecutor, it is further to be observed, that the reporters give it as their opinion, that one of the injuries on the head—that on the right parietal bone—might have been produced by a fall on a hard unyielding body, as well as by other kinds of external violence. Recollecting, then, that the reporters maintain further, that the fatal extravasation of blood in and on the brain might have been the result of *this* injury, as readily as of that one behind the right ear, it is obvious that the mere *fall*—on a hard pavement, for instance—and not the *kick*, alleged to have been in-

flicted by Alexander, might have been the primary cause of death. It is perfectly conceivable that, supposing the deceased had sustained such a fall *antecedent* to the receiving of the alleged kick, the deviation from natural structure which led to the fatal issue might have ensued *from that single cause alone;* and that the death might *thus* have been induced without the superadded violence of the kick, which, under the supposed circumstances just adverted to, can only be regarded, *in a medical sense*, as having aggravated the previous injury, and rendered recovery more hopeless.

"These considerations are submitted with the most respectful deference, and an apology, if such be needed, will be found for them, it is hoped, in the importance of the case, in the deep responsibility which devolves on the medical reporters, and in their anxiety to do justice to the accused, to the public, and to their own consciences."

The practical question, then, was simply this; was the injury done to the brain the effect of the fall on the pavement, whereby concussion and simultaneous extravasation were induced, or were these the consequences of the kick behind the right ear? While, morally speaking, I entertain but little doubt that it was the infliction of the latter which killed this unfortunate man, yet, in a medico-legal sense, it appeared to me impossible to affirm that the former was incapable of producing a similar result. If, therefore, the fall *might* have done all the mischief, then Alexander was not guilty of murder; for, though the kick which he inflicted would of itself have killed his antagonist, and was, I strongly suspect, the only murderous instrument employed on that melancholy occasion, yet it was possible that the mortal lesion had been induced, prior to that act of violence on his part, by an injury, for the infliction of which he was not more responsible in a moral, though perhaps not in a legal, sense than his ill-fated opponent. These men, be it remembered, voluntarily engaged in a combat, and a fall, sustained by either during their struggle, was an event as likely to occur to the one as to the other; and death, resulting from that circumstance alone, or even from any of the barbarities of a pugilistic contest conducted on the principles of what is vulgarly called fair play, would involve the survivor in the responsibilities of culpable homicide, but not in those of murder.

The crown, however, charged Alexander with murder alone, but when the trial came on at the Circuit Court of Justiciary here, the Advocate-Depute, finding it impossible, I presume, from the medical evidence to substantiate his charge, accepted the minor plea of culpable homicide, which, as is well known to all who are conversant with the proceedings of criminal courts, is always implied as an alternative charge in the graver one of murder. The prisoner was sentenced to six months' imprisonment.

The next case I propose calling your attention to, is one in which a drunken husband lived on very bad terms with an equally drunken wife, whose person was seldom without marks of violence of some kind or other, resulting either from her own centripetal tendencies, or from her husband's assiduous exertions to thrash her into temperance. The case is illustrative of the course to be followed when two

diseases exist, one induced by violence, the other the effect of natural causes; and in reference to the medical report which follows, I may state, that the usual preamble has been omitted, and I may also mention, lest some of the junior members think too much circumlocution has been employed, that a medical reporter is expected to use as few technicalities as possible, and that, where these cannot be avoided, it is advisable, as far as he can, to explain them. The employment, for example, of such cacophonious jargon as the *Hippocampus major*, or the *Massa carnea Jacobi Sylvii*, is not likely to be of much service to a jury in conducting them through the perplexities of an intricate case, notwithstanding the reporter be of opinion, that his reputation for profound erudition will be greatly increased thereby.

"Above and below each eye was a deeply livid contusion, and the whole of the brow was contused and of a yellowish-green colour. On the back of the right arm, about four inches above the wrist, was a small superficial bruise; on the fore part and middle of the left leg was a very deep abrasion, the skin being completely detached to the size of a shilling, and the surrounding parts loaded with effused fluid. A smaller and less deep abrasion existed about two inches higher up on the same leg, and there was another of similar size and appearance on the middle of the right leg also. Both legs bore the marks of severe, recent, and extensive bruises—indeed the front of the left leg was one contused mass.

"The under side of the scalp, corresponding to the entire forehead and considerably upwards, was in a state of ecchymosis, or red discoloration.

"None of the injuries above detailed could have been caused but by violence applied externally.

"The vessels of the brain were excessively congested; through its substance there were numberless bloody points; at its base were four ounces of bloody serum; the arachnoid sac, or middle covering of the brain, was distended with serous effusion; while in two places, namely, over the anterior part of both hemispheres, and over the back part of the left hemisphere, this same membrane presented the undoubted marks of recent inflammation. These appearances in the brain and arachnoid membrane were the result of recent disease of an active character, and it is reasonable to conclude that the same violence which produced the external injuries on the forehead and scalp—accompanied, as these must have been, by great excitement of the circulation of blood in the brain—had occasioned that congestion of this organ and inflamed condition of one of its membranes, which have been described above.

"The contents of the belly were healthy, with the exception of the liver, which presented those characteristic alterations of structure and appearance that are induced by the inordinate use of alcoholic fluids.

"The heart and left lung exhibited no deviations from the natural structure; but in the right side of the chest there were firm adhesions between the surfaces of the pleura, or investing membrane, while the lung itself was infiltrated with bloody serum, and in some places completely disorganized. These appearances in the right side of the chest, denoted that serious disease of an inflammatory type had existed for some time previous to dissolution.

"In drawing a conclusion as to the cause of death from the morbid appearances in this case, we feel bound to state, that the fatal result might be attributed either to the changes from healthy structure which were found in the brain, or to those which existed in the right lung and pleura; and that, had either of these diseases of these parts existed by itself, we should have found in it an unequivocal cause of death. Certainly the disease in the brain—accompanied as it was, if not produced, by external violence—was the more acute of the two, the more recent in its character, probably the more rapid in its pro-

gress, and the more likely to have caused the fatal result; but then it is impossible to say, that the disease of the right lung and pleura, irrespective of any other ailment, was not the immediate means of inducing that event.

"This being the case, then, we decline giving any more explicit and definite opinion as to the cause of death, until we hear what were the precise symptoms which this woman exhibited for some time before her decease."

On receiving this report, the procurator-fiscal liberated the accused, and abandoned all further proceedings.

The last case I shall present you with, is one in which a man was accused of having killed his wife by striking her on the head and face, though it afterwards turned out that the injuries which were visible on her person had been occasioned by her having fallen repeatedly while labouring under intoxication.

The following is the principal part of the medical report on this case:—

"Both eyes presented the appearance familiarly known by the term a 'black eye.' The conjunctiva, or outer covering, of the right eyeball was injected with blood; and, towards the outer angle of the right eyebrow, there was a scar, indicative of a recent cut in that situation. On the under side of the scalp, covering the forehead, and towards the right side, there was a deeply red discoloration, measuring five inches in the transverse, and one inch in the perpendicular, direction. These injuries could only have been caused by blows or falls, and they must have been received within the last ten days.

"The vessels of the brain were congested, and there was a considerable amount of serous effusion beneath the middle membrane; but there was no extravasation of blood on the *surface* of the organ. In the left hemisphere of the cerebellum or smaller brain, however, there was a large clot of blood, while the substance surrounding the clot was softened, pulpy, and disintegrated. In the left lateral cavity of the larger brain likewise, there was a similar clot, but of smaller size. The contents of the chest and belly were healthy.

"The death in this case was undoubtedly owing to extravasation of blood in the substance of the larger and smaller brain. That the extravasation in the smaller brain, at all events, had existed for some time, was evident from the disorganizing effects which its presence had induced in the structure surrounding it; and that such an extravasation might have resulted from injuries of the head (such as those of which we have described the marks) is true; but it is true, also, that this apoplectic condition of the brain might have been the effect of natural causes.

"Under these circumstances, therefore, while we are of opinion that the external injuries of the head *might* have occasioned the alterations in the brain and its vessels which led to the fatal issue, we are unable notwithstanding, in the present state of our information, to connect the two as cause and effect."

On this case I shall only observe, that the existence of the marks on the head and face not only complicated what otherwise was plain and simple in a pathological sense, but led to the preferring of a groundless charge, and the apprehension of an innocent individual, with the concomitant grievances of endangered reputation and lacerated feelings. On receiving the foregoing report, no further proceedings were instituted.

In conclusion, permit me to remark, that it will very likely occur to some, on reviewing the facts and opinions now submitted, that undue caution has been displayed by the medical reporters, or rather by myself; for, though I have used the plural number throughout, it may here be proper to state—chiefly to attach the responsibility to the proper quarter—that the reporters who were associated with me evinced their concurrence in the opinions set forth, by simply adhibiting their signatures to the documents which have now been read. Giving way to our feelings, and influenced by them alone, the atrocities of the first case, for example, provoke our indignation, and almost justify the wish that the heartless perpetrator had received a punishment more proportioned to the enormity of his offence; while the details of the last make us regret that, in the discharge of public duty, innocence is often harassed, and the administration of justice so imperfect as to offer no redress to the victim, it may be, of calumny and vindictiveness. But imperfections in the law, and in the administration of justice, though well entitled to occupy the attention of the legislator, come not within the sphere of the medical jurist, to whom, as to the surgeon, the appellation of *immisericors*, in the sense given to it by Celsus, is equally applicable. Recognising neither prosecutor nor prisoner, the medical jurist never can forget that the moral obligations of his position place him beyond the reach of either, and lead him to record what in his judgment and conscience he believes to be true, no matter what be the result of his deliberate, unbiassed declaration. If doubt exist, however, he is bound to give the accused the benefit of that doubt, recollecting with Marc, that, "in order to punish, it is necessary to establish; and when the limits of our art refuse us that degree of certainty of which we are anxious, the fear of sacrificing innocence ought to absorb every other consideration."

ARTICLE V.—*Contributions to the Pathology of the Heart and Arteries — Aneurism of the Coronary Artery.* By THOMAS BEVILL PEACOCK, M.D., Physician to the Royal Free Hospital.

ANEURISM of the coronary artery is a very rare form of disease: on a somewhat extended search into the records of medicine, I find only two cases fully reported, and imperfect notices of two others; and an examination of most of the various pathological museums in London, has failed to bring under my notice any specimen of the disease.[1] In the present paper I purpose, in the first place, to give

[1] In the museum of St Bartholomew's Hospital there is a preparation, formerly described as a case of aneurism of the coronary artery, but which, it has been shown by Dr Thurnam, is an aneurism of the sinus of Valsalva.

a concise notice of the cases of this affection which I have found recorded, and I shall then detail the particulars of one which fell under my own observation during the course of the last winter, and of which a short report has already appeared in the "Transactions" of the Pathological Society of London, vol. i. p. 227.

The first case of aneurism of the coronary artery which I have met with was published by M. Bougon,[1] in 1812. The particulars are as follows:—

An old soldier, addicted for many years to habits of intemperance, and who had suffered from attacks of low fever and from rheumatism, began, after severe domestic distress, and four years before falling under the notice of M. Bougon, to experience pains in the chest, with a sense of suffocation and inability to sleep. These pains occupied the back, front, and middle of the chest; and the attacks commenced about eleven or twelve o'clock at night, and continued till the morning. Various remedies were tried without advantage, and his sufferings increased. General bleeding rather added to the pain and palpitation; but relief was obtained by the application of leeches. During the attacks, he experienced some mitigation of his sufferings by walking rapidly, or suspending himself by the arms. Six weeks before his death, after a long and fatiguing journey, he had an attack of fever, with pain in the calf of the left leg; and during this indisposition the cardiac symptoms entirely disappeared, but, on his recovery, recurred with greater severity. For the whole of his illness, and during the last periods of life, he was free from cough or expectoration, but had pain in the middle of the left arm. The cardiac and radial pulsations were isochronous, and were sometimes feeble, and sometimes strong and intermittent. There was no lividity of the face, and the extremities were not œdematous. He was seized one night, when retiring to rest, with pain passing rapidly along the course of the spine to the back of the head, and very shortly expired. On examination, the pericardium was found to contain about two pints of blood, partly in a liquid state, partly coagulated. The heart was of natural size, and its cavities were free from blood. The aortic orifice was so contracted by the growth of vegetations there and on the semilunar valves, that the little finger could only with difficulty be introduced. The aorta was natural as to size, and free from disease; but the right coronary artery was aneurismal for the extent of an inch from its origin, and so dilated as to admit a female sound; and the coats of the vessels, the adjacent cellular and adipose tissue, and the pericardium, were converted into a fatty material, studded with osseous plates and granules. In the aneurismal sac there was an oblong opening, into which a goose-quill could be introduced. This opening communicated with the cavity of the pericardium, and its edges were unequal, soft, reddened, and very lacerable, as if gangrenous.

The second case of aneurism of the coronary artery which I have found was published in 1843, by M. Peste.[2]

An old currier, seventy-seven years of age, was admitted into the Bicêtre, under the care of M. Rochoux, in 1843. He was very temperate, and had enjoyed good health till upwards of two years before, when he was seized with an attack of apoplexy, followed by paralysis of the left side of the body. During the eight months he was in the hospital his intelligence was unimpaired, and he made no complaint of pain, though he was constantly confined to his bed. On the 15th of June he took his usual meal freely, but was afterwards seized with vomiting, and returned the food taken. From this time he

[1] Bibliotheque Médicale, tom. xxxvii. 1812.
[2] Archives Générales de Médicine, 4me serie, tom. ii. p. 472.

experienced some feeling of indisposition, with difficulty of breathing, and pain in the præcordia, occasionally very severe. He died suddenly, without having presented any other symptoms, on the morning of the 19th.

On examination the pericardium was seen much distended with bloody serum, and the heart was enveloped in a firm coagulum. At the anterior surface of the left ventricle, and near its centre, a laceration, fourteen millimetres in length, was found extending through the walls of the ventricle in a somewhat transverse direction, though tending from above downwards. This aperture had ragged edges, and was surrounded by a delicate false membrane. The heart was covered by a layer of fat, especially thick at the base. At a short distance from the origin of the left coronary artery there was a patch of extravasated blood, which was traced to a rupture in the walls of a small aneurism of that vessel. A probe, introduced into the lacerated opening on the surface of the ventricle, penetrated into its cavity between two of the fleshy columns, and could also be passed into the sac of the aneurism.

The left coronary artery was dilated throughout its whole course, so as to equal in size the brachial artery, and its coats were in places studded with osseous plates. The aneurism was situated at the point where the artery divides into its branches, and was of the size of a large nut. Inferiorly the sac presented an aperture by which the blood had escaped, and the coats around this point were extremely thin. The sac contained laminated coagula, resembling those ordinarily found in aneurisms. The right coronary artery was free from disease, and the substance of the heart was in no degree softened or otherwise altered, and its valves were natural. The cavity of the left ventricle contained some blood—partly fluid, partly coagulated; and a nearly decolorized coagulum adhered to its lining membrane, in the situation corresponding to the external opening. The coats of the aorta were very readily lacerable, and displayed osseous scales in places.

An apoplectic cell, surrounded by false membrane, existed in the brain, near the surface of the right corpus striatum. M. Peste regards the rupture of the left ventricle, which was the immediate cause of death in this case, as having originated, during the effort of vomiting four days prior to the fatal event, in a laceration of the coats of the aneurism, forming an opening into the cavity of the left ventricle. He further supposes that the internal fibres of the ventricular parietes being thus torn, the pressure of the blood subsequently extended the laceration through to the pericardium.

Of the two other cases of aneurism of the coronary artery to which I have alluded, one is related by Hedland, and referred to by Otto,[1] as having occurred in a "man forty years old, and proved fatal by bursting, and the effusion of blood into the pericardium." To the periodical in which this case is reported I have not been able to obtain access.

The last case is briefly noticed by Merat,[2] who states that he had observed "an erosion of the coronary artery, which had given rise to a pouch formed in the thickness of the parietes of the left auricle which would have contained a small nut." The subject of this disease had never presented any cardiac symptoms, and the affection seemed to have had no connexion with the cause of death.

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[1] Compendium of Pathological Anatomy, translated by South, p. 319; quoted from the Svenska Läkare Sällskapets Handlingar, vol. iii. p. 181.

[2] Art. Cœur (Pathologie) Dict. des Sc. Méd., tom. v. p. 484. See also a paper by M. Aran, in which the cases of Bougon, Peste, and Merat are quoted.—Arch. Gén. de Méd. 4me serie, tom. xiv.

After premising this account of the other examples of aneurism of the coronary artery on record, I proceed to narrate the particulars of my own case.

Aneurism of the Left Coronary Artery; Enlarged Heart; Peri- and Endocarditis; Bronchitis.

Richard Appleton, aged fifty-one, by trade a butcher, was admitted into the Royal Free Hospital during the prevalence of the influenza epidemic on the 2d December 1847. It was ascertained by inquiry of his friends, that he had formerly been in a respectable rank of life, but was of very irregular habits, and for the last month had been so ill as to be incapable of following his employment, and had in consequence received parish relief. He had been subject to rheumatic pains, but had never, so far as could be ascertained, had rheumatic fever. Though never a stout person, he boasted of the great weights which he had formerly been in the habit of carrying. He had long had a severe cough, and had suffered from difficulty of breathing. The following notes were taken the day after his admission into the hospital. Yesterday he was extremely collapsed, but he has passed a tolerably good night under the influence of an anodyne, and to-day he is less prostrated, though still extremely feeble. He has a severe cough, breathes rapidly, is much emaciated, and, from the torpid state of his mind, cannot be got to give any account of his previous state. The pulse is 144, and extremely feeble. The tongue is red at the edges and tip, and covered in the centre with the whitish-brown fur characteristic of the cases of the prevalent influenza. The extreme prostration of strength does not allow of any accurate examination of the state of the chest; but there is some deficiency of resonance on percussion in the right dorsal region, and fine crepitation is there heard, and there is dry sonorous rhonchus in all other parts of the chest. He lies on his back, and his face is not flushed or livid. The liver extends far into the abdomen, and feels hard and resistent. He was cupped between the shoulders to five ounces at the time of admission, and he is now to have a blister applied to the chest, and to take a mixture containing ipecaccuan wine, compound tincture of camphor, and aromatic spirit of ammonia, at frequent intervals, and to have two ounces of brandy.

Dec. 4.—The pulse is 136, and of somewhat better character. The tongue is red at the edges and tip, and covered with a brownish fur in the centre. He complains of the cough, and has expectorated greenish-yellow coloured and very viscid mucus, but only in small quantity. He is extremely weak and helpless, and cannot without difficulty be moved in bed. To continue the mixture and brandy.

Dec. 8.—For three days after the last entry he continued in some degree to improve,—the pulse became less frequent, and, though still feeble, was of better character than before, and he was stronger and more intelligent. Yesterday, however, this improvement was superseded by an increase of debility and greater torpor of mind. The pulse is now 120, extremely feeble and unequal both in force and frequency. He has a troublesome but not very severe cough, and expectorates a scanty spectum of a greenish-yellow colour, very adhesive, and containing small air bubbles. He breathes feebly and without effort, and is not livid in the face, and remains in any position in which he is placed without apparent inconvenience from dyspnœa. He is extremely weak, so as to be altogether incapable of moving himself, and lies in a semi-comatose state, from which there is some difficulty in arousing him. He passes his stools and urine regularly and consciously. Yesterday a blister was applied to the nape of the neck. The former mixture was replaced by one consisting of the infusion of serpentary, aromatic spirit of ammonia, compound sulphuric ether, and compound tincture of camphor, and the brandy was increased to four ounces. This treatment is to be continued, and a blister to be applied to the front of the chest.

Dec. 12.—After having continued much in the same state, he complained yesterday of pain at the lower part of the left side of the chest. The pulse was very feeble and intermittent, and beat about 120 times in the minute. The cough was less frequent, and the spectum was more copious and muco-purulent in character. The skin was cool and moist. He had been somewhat purged, and had passed blood in the stools. Loud sonorous rhonchus, with sub-crepitation, existed in all parts of the chest; and in the region of the heart, and along the course of the sternum, a peculiar sound was heard exactly comparable to that produced by beating egg with a spoon or fork. This sound was very distinct about the middle of the sternum and on the right side of that bone, while it was imperfectly audible in the situation of the apex. It was double in its character, and seemed rather to replace, than to be added to, the natural sounds. A blister was applied to the seat of pain.

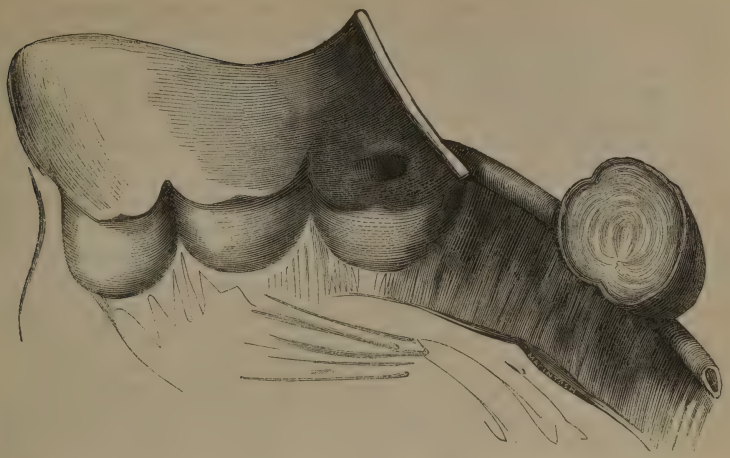
To-day he still complains of pain at the præcordia, and in the left lower lateral region, and in the latter situation there is much coarse crepitation, and the same with sonorous rhonchus is audible posteriorly in the dorsal regions. The peculiar sound heard yesterday in the cardiac region, is not now detectable, and the sounds of the heart are almost inaudible. He has twice passed blood in the stools since yesterday, and he is still more collapsed than before. The pulse is extremely weak and faltering. The tongue is dry, red at the sides and tip, and brown in the centre. He has but little cough, and expectorates spectum, partly muco-purulent in character, partly of a more viscid description. From this time he gradually got weaker, and died the same evening.

The *post-mortem* examination took place the following afternoon.

The brain displayed old thickening and opacity of the arachnoid membrane on the superior surface of the hemispheres, and adhesions between that membrane and the pia mater. The amount of fluid in the ventricles and at the base was trivial. The substance of the organ was healthy. The cerebrum weighed thirty-nine and a half ounces avoirdupois. The cerebellum four and a half ounces; and the pons varolii and medulla oblongata one ounce. The whole encephalon thus having a weight of forty-five ounces.

Both lungs, especially the right one, were emphysematous in front, and covered the pericardium so as to leave only a limited space exposed. At the posterior part of the upper lobe of each lung, there were loose cellular adhesions, by which they were attached to the parietes. The posterior, and especially the inferior, parts of both lungs were much engorged and loaded with frothy serum, so as scarcely to crepitate when compressed. The bronchial tubes were dilated, and contained much muco-purulent fluid, and the mucous membrane was reddened. The degree of injection of the membrane, and the amount of secretion, increased as the tubes diminished in size, and were most marked in the lower lobes of the lungs. The left lung was more considerably engorged than the right.

The pericardium was distended by a large accumulation of pale yellow-coloured sero-purulent fluid, and the surfaces, both of the attached and free serous membrane, were covered by layers of lymph of a soft consistence and delicate lace-like texture. On the surfaces of the auricles the effusion had a more flocculent character, while on the ventricles it was more solid and laminated. The sub-serous cellular tissue over all parts of the heart was extensively infiltrated with serum and pus, and the muscular substance was also softened and readily lacerable. At the upper and outer part of the front of the left ventricle, there existed a protuberance about the size of the half of a walnut, which, like the rest of the surface, was covered by a layer of lymph, and beneath this by a patch of white false membrane of old date. On making a section through the walls of the ventricle, so as to bisect this projection, it was found to consist of an aneurism of the left coronary artery.



The *aneurism* was situated on the anterior branch of the artery, at a distance of thirteen French lines from the origin of the vessel from the sinus of Valsalva, and ten lines from the point at which it divided into its two primary branches. It was of a nearly spherical form, and had a diameter of eight and a half lines. Externally it was covered by the pericardium and the layer of false membrane before mentioned, and internally it was imbedded in the wall of the left ventricle.

The cavity of the aneurism was filled by coagula, arranged in the form of laminæ, and some of them partially decolorized; and the coats on the external side were so extremely thin, and so intimately connected with the outer layers of coagula, as to be inseparable from them. The sac was, however, entire, and the artery communicated with it at its posterior side, and somewhat obliquely, the distal portion of the vessel being displaced by the pressure of the tumour, so that a probe could not be passed from the canal of the artery above the aneurism, into the vessel beyond it. The orifice of the left coronary artery was considerably dilated, and its calibre, from its origin to the seat of the aneurismal sac, was much increased in capacity. Its coats were also extensively ossified, so that the whole was converted into a solid cylinder of bone; beyond the aneurism the vessel was less extensively diseased. The right coronary artery was somewhat dilated, and its coats were in places studded with cretaceous plates.

The heart weighed thirteen ounces avoirdupois, and was of a peculiarly elongated form. The right ventricle was dilated, and its walls hypertrophied, having an average width of three to four lines; externally, it was covered by a deposit of fat of moderate thickness. The tricuspid and pulmonic valves were healthy. The left ventricle was also dilated, and its walls thinner than natural, being at the base five lines, at the mid point three, and at the apex two lines. The cavity of the ventricle was prolonged and expanded at the apex, and the muscular substance was there extremely soft and lacerable, and a coagulum, partly decolorized, adhered firmly to the endocardium. The aortic valves were somewhat thickened and adherent at their angles, but were competent, and the mitral valve was healthy. The aortic aperture admitted a bulb measuring thirty-six French lines in circumference, and the orifice of the pulmonary artery one of the circumference of thirty-nine lines. There was some thickening and opacity of the lining membrane of the left auricle.

The aorta was lined by a distinct layer of lymph over a considerable extent

of its ascending portion, and the serous membrane was much reddened. There were several patches of atheroma around the origins of the coronary artery, and in other parts of the aorta.

The liver was very large, coarsely granular, and engorged. The spleen was of large size, and its surface was covered with nodules of fibro-cartilaginous deposit. The kidneys were very solid and engorged, but not otherwise diseased. The intestinal mucous membrane was not examined, but, externally, the intestines presented no appearance of disease.

The case which has been last detailed, affords a good example of the affections which were of such frequent occurrence during the prevalence of the influenza of 1847. The peculiar mental hebetude, the extreme prostration of strength, and the great muscular debility, were symptoms which very generally characterized the epidemic; and the complication of extensive capillary bronchitis, with peri- and endo-carditis, was of occasional occurrence in the severer class of cases. It may, indeed, be doubted whether the aneurism of the coronary artery exerted much, if any, influence on the fatal result; the accumulation of coagula in the aneurismal sac, and the consequent obstruction of the artery, having probably taken place only within the last periods of life.

The liability of different arteries to dilatation, bears in general a direct relation to their size. In the ascending portion and arch of the aorta, and in the arteries thence originating, aneurisms are especially frequent; while in the smaller arteries, which are not liable to external injury, they are of very rare occurrence. So far, however, as the coronary arteries are concerned, this law would scarcely be expected to obtain. Arising from the very origin of the aorta, and exposed to greater changes of tension than any other vessels, they would appear to be especially liable to undergo dilatation; yet the few cases of aneurism which have been recorded, and the improbability that such a lesion should frequently escape observation, if it existed, would evince that they possess a remarkable immunity from that disease. In estimating the causes to which this immunity must be ascribed, the peculiar mode in which these vessels are given off from the aorta, and their course at a somewhat acute angle to that vessel, must be taken into account; yet this will not alone explain their escape from the ordinary effects of undue distension, and a more powerful influence must be assigned to the resistance of their tunics. Dr Norman Chevers[1] has drawn attention to a peculiar arrangement of the fibres of the elastic coats of the coronary arteries, calculated to afford great resistance to the column of blood. In all arteries he has shown that there exists a distinct layer of fibrous tissue, having a longitudinal direction, interposed between the serous lining of the canal and the circular fibres of the ordinarily described middle tunic. In the aorta[2] this layer constitutes an important defence to the vessel;

[1] Guy's Hospital Reports, 2d series, vol. i. 1843, p. 103.
[2] Ibid., vol. v. p. 40.

while in the arteries of the extremities the longitudinal fibres are only few in number. It is otherwise, however, with the coronary arteries; if the lining membrane of these vessels be raised, a strong band of longitudinal fibres is exposed, and this layer seems to be very largely developed around all parts of the canal, at the expense of the subjacent circular fibres. It is evident that a provision of this description must tend most materially to defend the coronary arteries from the occurrence of dilatation, and especially of partial dilatation. Indeed, a sacculated aneurism seems only likely to be formed after the rupture of the longitudinal fibres. Thus we can understand, that while fusiform dilatation of the origins of the coronary arteries is occasionally seen, and, under long-continued obstruction at the right side of the heart, the coronary arteries throughout their whole course may be enlarged, yet that true circumscribed aneurism has occurred in only three or four recorded instances.

If rupture of the internal tunics necessarily precedes the occurrence of aneurism of these vessels, the small size of the aneurismal sac, which in the three instances related did not exceed that of a nut, is readily understood. For, though, as in other instances of tumours projecting from the surface of the heart, slight inflammation, leading first to thickening of the adjacent pericardium, and subsequently to adhesion of its two layers, will take place; yet in no long time the coats of the aneurism must give way, and occasion a fatal extravasation of blood. Such a result ensued in the case of M. Peste, in which the aneurism was sacculated; in that of M. Bougon, where the sac seems to have been less distinctly circumscribed; and in the case of M. Hedland, of which no particulars are given. In my own case, had the life of the patient been prolonged, and the sac not been filled with coagula, the tunics of the aneurism must very shortly have ruptured; and, as the layers of pericardium had not become adherent, a fatal hemorrhage into its cavity would have followed.

In these brief remarks, I have made no allusion to the diagnosis of aneurism of the coronary artery. M. Gendrin, in a lecture at the Ecole Pratique in 1843, laid down directions by which this disease may be discriminated during life; yet, as in the few cases related, no observations of the physical signs have been recorded, we may safely doubt whether this is not an instance of the hasty generalization in which our neighbours are apt sometimes to indulge. The peculiar sound which I have described as heard the day before death in my own case, would seem to be due to the beating of the heart in the large amount of thickish fluid contained in the pericardium; and, it is also probable, that the presence of the emphysematous lung in front of the distended pericardium, was concerned in the production of the sound.

The extension of inflammation in this case from the pericardium through the substance of the heart, so as to involve the endocardium

at the apex of the left ventricle, and occasion a prolongation and expansion of the ventricular cavity in that situation, is worthy of note, as affording an example of one of the modes in which, according to Rokitansky,[1] the partial aneurisms of the heart are produced.

Part Second.

REVIEWS.

Lectures on the Principles and Practice of Physic; delivered at King's College, London. By THOMAS WATSON, M.D., Fellow of the Royal College of Physicians; late Physician to the Middlesex Hospital, and formerly Fellow of St John's College, Cambridge. In Two volumes; the Third Edition revised. London: 1848. 8vo. Pp. 852, 881.

Clinical Lectures on the Practice of Medicine. By ROBERT J. GRAVES, M.D., M.R.I.A., &c. &c. Second Edition. Edited by J. MOORE NELIGAN, M.D., M.R.I.A., &c. In Two volumes. Dublin: 1848. 8vo. Pp. 586, 570.—(*Continued from* p. 469.)

WHEN the cholera existed in Dublin in the spring of 1832, the principal modes of treatment were bleeding in violent spasmodic cases, emetics of ipecacuanha and mustard, the external application of heat, and the use of stimulants, but above all, calomel, not in small, but in large and frequently repeated doses, either alone or combined with opium. All these remedies were tried; they all failed; and at length Dr Graves, having seen the excellent results following the free use of acetate of lead in many cases of obstinate diarrhœa, resolved to try it in this fearful disease:—

"The mode in which I administered it was this:—A scruple of the acetate of lead, combined with a grain of opium, was divided into twelve pills, and of these one was given every half hour, until the rice water discharges from the stomach and rectum began to diminish. In all cases where medicine promised any chance of relief, this remedy was attended with the very best effects. It gradually checked the serous discharges from the bowels, and stopped the vomiting. I need not say of what importance this is: as long as these exhausting discharges continue, as long as the serum of the entire body continues to be drained off by the intestinal exhalants, what hope can we entertain? What benefit can be expected from calomel and stimulants, when every function of the digestive mucous membrane seems to be totally extinguished except that of exhalation, and while profuse discharges, occurring every five or ten minutes, are reducing the patient to a state of alarming prostration?"—Vol. I. p. 419.

"I may observe, that this mode of treatment has now become universal here, and that it has almost completely superseded the use of calomel and opium. I will confess, that this fact is a source of high gratification to me, and I

[1] Handbuch der Pathologischen Anatomie, ii. Band, p. 450.

point also with pleasure to the fact, that since it became extensively known (as it did during the last invasion of the epidemic), the profession has gained more credit than before, and the number of cures has been proportionately greater."—Vol. I. p. 420.

The evidence of Drs Parkes and Thom speaks strongly in favour of this mode of treating the disease as it occurs in India.

There is little that strikes us as new in the Lecture on Influenza. Dr Graves states, that he has observed that this disease seldom attacked persons labouring under acute diseases until the period of convalescence arrived, when their immunity ceased:—"Thus, patients labouring under typhus escaped as long as the fever continued; but frequently, on the very day the crisis occurred, and symptoms of returning convalescence appeared, they were seized with influenza."

The disease was very fatal to persons who had been subject to chronic bronchitis, or had laboured under any asthmatic affection, in very aged persons, and in persons (of all ages) who laboured under disease of the heart. "The mortality was also very great among persons in advanced life who laboured under *tussis senilis;* in a word, all persons labouring under pulmonary irritation or weakness, were exposed to very considerable danger. Subsequent experience has proved also, that where influenza left behind an obstinate and irritating cough, and where the constitution had a scrofulous taint, the disease was very apt to pass into tubercular phthisis." From a comparison of the general characters of the influenza of 1847 with that of 1837, Dr Graves concludes, that in the epidemic of last winter the depression of the vital powers was much more marked, but that the feverish symptoms were less than in the previous one. In all the fatal cases death occurred with symptoms of *paralysis* of the lungs.

The remarks on treatment are sound, but contain nothing particularly new. We shall not enter into this subject, because we believe that, generally speaking, every epidemic of this nature presents certain peculiarities, and requires its own treatment.

The thirtieth lecture treats somewhat cursorily of "the connexion between diseases of different organs,"—a subject of great importance, and one regarding which we hoped to have found much additional information in this edition. In this respect we must confess ourselves disappointed. Dr Graves mentions, that he has eight or nine times seen persons labouring under inflammation of the joints get an attack of hepatitis, accompanied by jaundice and followed by urticaria:—

"Since my attention," says Dr Graves, "has been drawn to the connexion between these three diseases, I have seen and heard of several other instances in which they appear thus associated together. . . . In the first place, nothing has been longer recognized by physicians as an established fact, than the intimate sympathy which exists, both in health and disease, between the digestive organs and the skin. Now, acute hepatitis always produces more or less derangement of the stomach and alimentary canal; and we may therefore

consider its connexion with urticaria in the same way that we are in the habit of viewing the cases so commonly observed, in which certain sorts of fish have produced serious symptoms of indigestion, followed by nettle-rash. The association between these two diseases is rendered more remarkable by the fact, that when fish taken as food exerts a poisonous effect on the system, it frequently produces not merely violent stomach and bowel complaint, but also inflammation of the joints and rheumatic pains."—P. 447.

Dr Graves adduces various examples (the Otaheitan eel, the *murœna conger*, the *sparus pargus*, &c.) with the view of thus showing, that the connexion between arthritis, disease of the digestive organs, and urticaria, "must be looked on as owing to and arising from the operation of some fixed law, which regulates and originates this development of morbid actions in, if not a frequent, at least an uniform, mode of succession."

In connexion with this subject, Dr Graves narrates and comments on a case of "venereal, abuse of mercury, periostitic inflammation, abuse of mercury followed by exacerbation of the periostitis, and the establishment of mercurial cachexy; and the history of the case is wound up with hypertrophy of the liver." Four times has Dr Graves observed this sequence,—first, the abuse of mercury; then periostitic inflammation and mercurial cachexy; and the scene is enclosed by morbid enlargement of the liver,—each successive condition being, he believes, consequent on the previous one. He also shows, that hypertrophy and disease of the liver is a frequent sequence to *morbus coxœ*, to *scarlatina*, to certain morbid states of the heart, and to ague. We fully agree with our author in the remark with which he concludes this chapter, that "additional observations are much wanted on this subject; but based, as to prove useful they must be, on the accumulation of facts derived from experience, much difficulty lies in the way of their being undertaken."

Passing, from want of space, over the Lectures on *gout* and *rheumatism*, we arrive at the *diseases of the brain and nervous system*, to which six chapters are devoted. They embrace the consideration of *apoplexy*, *delirium tremens*, *chorea*, *epilepsy*, *paralysis*, and various neuralgic affections.

The remarks on chorea are extremely interesting. Dr Graves mentions a case in which this affection first showed itself in a man seventy years of age, and seems not aware that chorea had ever been observed to attack persons of advanced age. We think he will find a parallel case recorded by Dr Maton in the fifth volume of the *Transactions of the College of Physicians*.

We had intended to have directed the attention of our readers somewhat fully to Dr Graves' excellent description of the creeping paraplegia, arising from impressions made by cold and wet on the lower extremities,—a form of paralysis often observed in young men who, in the pursuit of field-sports, are in the constant habit of having their feet wet, and in labourers engaged in draining and similar occupations. We must, however, restrict ourselves to his observations on its prognosis and treatment:—

"The prognosis," he observes, "is generally unfavourable, particularly when the disease has lasted for some time, and is accompanied by morbid irritation, or loss of power in the bladder or rectum. It is also bad in proportion to the slowness with which it has come on, and the absence of pain or formication of the lower extremities. With respect to treatment I may observe, that I have never seen any benefit derived from applications to the spine. The application of blisters or issues over the back or loins, does not appear to be productive of the least good effect; of the latter I can speak positively from experience. They are an enduring source of annoyance to the patient, and never produce the least amelioration of symptoms.

"I am in the habit of applying my local remedies to the legs and thighs, selecting those parts in which the greatest cutaneous sensibility exists. What I generally do is, to keep up a succession of blisters along the inside of the legs, and over the anterior and inner parts of the thighs. The practice of medicine furnishes many proofs of the utility of stimulant applications to the nervous branches, in case of disease affecting the larger trunks. Thus, in sciatica, a blister applied over the ham or calf of the leg, where many of the ultimate ramifications of that nerve are superficial, will frequently produce a much more decided effect than when applied over the origin of the nerve itself. Liniments of a stimulating kind, and blisters repeatedly applied, are the local means on which I chiefly rely in the treatment of this form of paraplegia. After some time I commence with the use of strychnia, and continue it until some sensible effect on the system is produced, when I omit its further use, and have recourse to the exhibition of sulphur. These are the two internal remedies from which I have derived most benefit. I have, in such cases, seen very good effects from a perseverance in the use of the sulphur electuary. Much also will be accomplished by the external use of sulphur in the form of baths; and hence cases of paraplegia of this kind might be materially benefited by the internal and external use of the waters of Lucan, Harrowgate, Baden, Barège, &c. With respect to the use of mercury, it appears to be decidedly injurious. I have seen it given in three cases; in all it did much more harm than good."—Vol. I. pp. 566-7.

We believe that the *thermic treatment*, as recommended by Dr Day,[1] will be found the most effectual external application in cases of this nature.

In his remarks on neuralgia, Dr Graves takes the opportunity of expressing his disapprobation of the system pursued by certain physicians, of giving enormous doses of carbonate of iron and other tonics:—"In those cases of neuralgia in which carbonate of iron proves useful, I never found it necessary to raise the dose beyond one drachm three times a-day; indeed, a larger dose than half a drachm is seldom required." Dr Graves even goes so far as to lay it down as a general rule, of whose accuracy we entertain no question, that tonics are rarely indicated where moderate doses do not effect the desired purpose:—

"This applies," he adds, "more particularly to the stronger tonics, such as the salts of iron, of arsenic, and quina. I can scarcely conceive a case possible, in which a judicious physician will find it necessary, for instance, to give more than ten grains of sulphate of quinine in a day; and yet much larger doses are not unusual here and elsewhere. Wherever the symptoms supposed to call for such a treatment resist moderate doses of sulphate of quinine, we

[1] See the Appendix to his *Practical Treatise on the Diseases of Advanced Life*.

ought to pause and reflect whether another plan of treatment ought not to be adopted. . . . When sulphate of quinine is intended to act as a *tonic*, I am persuaded the dose should never exceed a grain three times a day, and generally even smaller quantities are sufficient; when a combination of tonic and purgative medicines is required, all our intentions may be answered by a combination of sulphate of quinine in proper quantity with the compound extract of colocynth or the aloetic pill with myrrh."—Vol. I. pp. 576-7.

The first 148 pages of Dr Graves' second volume (lectures thirty-nine to forty-six inclusive) are devoted to the *diseases of the respiratory organs*, and are rich in sound practical information. We must restrict our remarks to a brief notice of his observations on the causes of cough.

Passing over the obvious and well-known sources of pulmonary irritation producing cough, such as bronchitis, pneumonia, &c., the first cause to which he directs attention is one of not unfrequent occurrence, namely, the presence of worms. He gives the following illustrative case:—A young lady was attacked with symptoms of violent and alarming bronchitis. The fits of coughing went on for hours with extraordinary intensity; the cough was dry, extremely loud, hollow, and repeated every five or six seconds, night and day, when she was asleep as well as when she was awake; yet she fell not away proportionally in flesh, had no fever, and her chest exhibited nothing beyond the râles usually attendant on dry bronchitis. She was bled, leeched, blistered, and got the tartar emetic mixture, without experiencing the least relief. Antispasmodics were equally unsuccessful; and conium, hyoscyamus, opium, and prussic acid were administered without the slightest benefit. He and his friend Dr Shekleton gave up the case in despair, and discontinued their visits; when they heard soon afterwards that *she had been cured all at once by an old woman*, a servant in the family, who gave her a large dose of oil of turpentine with castor oil, for the purpose of relieving an attack of colic. The medicine caused the expulsion of a large mass of tape-worm, and from that moment every symptom of pulmonary irritation disappeared.

The next kind of cough noticed by Dr Graves, as one often misunderstood, is that which occurs in hysterical females. In some cases it is loud, ringing, incessant, and so intensely violent that one wonders how the air-cells or blood-vessels escape being ruptured. In others it is quite as incessant, occurring every two or three seconds, night and day, but is not very loud, and indeed in some it scarcely amounts to more than a constant teazing hem. The pulse is generally quick; but it is the quick pulse of hysteria, not of inflammation or fever. A deep inspiration causes no aggravation of pain. There is no lividity of the countenance from the mal-aëration of blood; and notwithstanding that the cough continues for weeks, or even months, the patient does not become emaciated, like a person in incipient phthisis, although so much disturbed by the cough, and subsisting on so little food.

The history of the case, and an examination of the chest, will ge-

nerally lead us to correct ideas regarding the nature of the disease. These cases are best treated by stimulants, antispasmodic and stimulant purgatives, together with change of air, travelling, and the use of chalybeate spa water. Depletion is, generally speaking, injurious.

The third species of obscure cough noticed by our author, is that which depends on a pulmonary irritation connected with a venereal taint in the system:—"I entertain," he observes, "a firm conviction, that syphilis may affect the pulmonary as well as it does the cutaneous, or mucous, or osseous tissues, and that a patient labouring under a venereal taint, may have irritation from this cause set up in the lungs as well as in any of those organs in which it is usually manifested." The paramount importance of a correct diagnosis in these cases is obvious:—"A patient comes to consult you for cough; you find him pale, emaciated, and feeble; he sleeps badly, and is feverish at night, and has a tendency to sweat. If the disease be mistaken for tubercle, and mercury not given, bad consequences will result; on the other hand, if tubercles be present, the effect of administering mercury may be to precipitate the disease to a fatal issue." Our diagnosis must be mainly based on the history of the disease. If our patient's sufferings have commenced at that period, after the appearance of primary sores, when secondary symptoms usually develope themselves; if with debility, night-sweats, emaciation, nervous irritability, and broken rest, we find cough; and if these symptoms are associated with others evidently syphilitic, such as periostitis, sore throat, and eruption on the skin, we may be tolerably safe in referring all the symptoms to the same origin, and regarding the cough as one of the individual manifestations of a general syphilitic taint. A correct diagnosis having been established, "we may then proceed to act upon our decision, and may advise a sufficient but cautious use of mercury. Under such circumstances, it is most pleasing to observe the speedy improvement in the patient's looks and symptoms; the fever, night-sweats, and watchfulness diminish; he begins to get flesh and strength, and with the symptoms of lues, the cough and pectoral affection disappear."

The fourth species of obscure pulmonary irritation producing cough, is that which is connected with a gouty diathesis. The mucous membrane investing the trachea and bronchial tubes is often attacked, and a dry, annoying, and usually very obstinate cough is set up. This sometimes becomes chronic, continuing till it is relieved by a regular fit of gout; in other cases, it yields to the use of alteratives combined with small doses of colchicum.

The fifth species of obscure pulmonary irritation is that which is connected with the scorbutic diathesis, which not only manifests itself in the form of purpura, or in tendencies to hemorrhage from the nose, stomach, bowels, or bladder, but sometimes attacks the lungs, producing irritation of the bronchial mucous membrane, with cough and spitting of blood, and sometimes giving rise to pulmonary apo-

plexy. It is evident, as Dr Graves observes, that pulmonic cases of this nature, originating in a scorbutic diathesis produced by confined air, damp lodgings, and insufficient diet, will require a treatment peculiar to themselves, both during the attack and during convalescence.

The last source of pulmonary irritation to which he directs our attention, is that which proceeds from scrofula:—"Scrofulous irritation may affect either the lining membrane or the parenchyma of the lung, giving rise in one case to scrofulous bronchitis, in the other to scrofulous pneumonia; two affections which may exist either separately or combined, and either of which may prove fatal with or without the development of tubercles in the lungs."

The Lecture concludes with a strong recommendation of the decoction of sarsaparilla and dilute nitric acid (a pint of the former with a drachm of the latter daily) in cases of chronic cough, attended with redness and relaxation of the mucous membrane of the fauces, elongation of the uvula, and general debility; and in cases of persons of a reduced and relaxed habit, who are troubled with slight but frequently recurring cough or hem, and the expectoration of a few bronchial sputa, occasionally mixed with blood, which appear to come not from the lungs, but from the eroded mucous membrane at the top of the pharynx and larynx. Dr Graves likewise adds, that the same combination may advantageously be given to patients whose mouths have been recently made sore by mercury administered for the cure of bronchitis and pneumonia, and will occasionally be found useful in removing the still lingering remnant of pulmonary disease at a time when mercury could not be pushed further with safety.

It is with feelings of regret that we are compelled to pass unnoticed the subjects of *pneumonia, gangrene of the lung, pleurisy, cancer of the lung, pneumo-thorax, pneumatosis, spasmodic asthma, phthisis, and hæmoptysis.* The Lectures on *phthisis* and *hæmoptysis* are especially worthy of attention.

Diseases of the heart are next considered. We extract the following practical remarks from his observations on *pericarditis*:—

"It is of paramount practical importance to recollect, *that a weak, irregular, intermitting pulse may exist in the very commencement of pericarditis*, that it may not exceed the natural frequency, or (as in two cases detailed) may fall much below that standard, and yet antiphlogistic treatment be required.

"The result of my experience is, that in carditis and pericarditis, when the pulse is weak, irregular, and intermitting, when it is soft, natural in its frequency, or else morbidly slow, that general venesection should never be employed; leeches over the region of the heart, cupping, blisters, calomel with opium, are best suited to this emergency, if it occurs during the acme of the disease; but when, towards the close, our chief reliance must be placed on powerfully blistering the region of the heart, dressing the vesicated surface with mercurial ointment, and exhibiting internally small doses of calomel, with large doses of opium, and, if necessary, wine.

"Digitalis exerts little or no control over inflammation of the heart, and, like colchicum, if given in doses at all proportioned to the danger, it often

suddenly produces dangerous or even fatal prostration of the nervous system. In protracted forms of cardiac or pericardiac inflammation, I have found colchicum, combined with mercury and opium, a useful adjunct; and where the disease is decidedly chronic, refusing to yield to treatment, much benefit is sometimes derived from hydriodate of potash."—Vol. II. p. 163.

The length to which our notice of the volumes of Dr Graves has already run in this and a preceding Number, warns us that we must, without further delay, bring this article to a close, although in so doing we omit all notice of some of the most important subjects treated of in these lectures. We will merely add, that, amongst other subjects, the reader will find that these concluding lectures treat of *diseases of the digestive canal, of the kidneys*, and *of the skin, the diseases of females*, and *syphilis*, in the same practical and satisfactory manner as the subjects into which we have more fully entered.[1]

Having thus closely followed our author through nearly one thousand pages, with much pleasure to ourselves, and we trust not without profit to our readers, our final parting from him shall be in his last lecture, *on the mode of administration and effects of various medicines*.

The first point treated of, is regarding the best mode of administering calomel in acute inflammations. He believes, that in cases of very acute pericarditis (and the remark applies equally to the more acute and violent forms of peritonitis, hepatitis, pneumonia, pleuritis, and perhaps dysentery), in addition to the lancet we ought to affect the constitution decidedly, and as speedily as possible by means of calomel, given not in small doses often repeated, but in doses of a scruple once or even twice daily. When we wish a scruple to be taken at once, an excellent method is to place the powder on the tongue, and make the patient wash it down with some thin gruel.

"In most cases," says Dr Graves, "one such dose daily is sufficient; but it now and then happens that very imminent danger will prompt us to give a second dose after the lapse of twelve hours. By this management we are often enabled to mercurialize the system fully in a very short space of time indeed, and we thereby not only cut short a dangerous inflammation, and save our patient's life, but we often effect this purpose without exciting any considerable griping pains or bowel complaint. Another most important question is, whether mercury so used for the cure of internal inflammation injures the constitution permanently? With the greatest confidence I can answer, it does not. I never saw a single bad effect follow the use of mercury in cases where the first consequence of its exhibition was the rapid and complete removal of a dangerous inflammation; a remedy can scarcely serve and hurt the constitution at the same time. Mercury, when it abates inflammation, never irritates the system; and if it be discontinued when it has performed this important office, its after effects will be employed in the same way, in curing the remnant

[1] In the *Retrospect* for the present month (Practice of Medicine), the reader will find two interesting cases of idiopathic glossitis, by Drs Graves and Neligan, extracted from p. 197 of the second volume.

of the inflammatory action. Whatever cuts short the inflammation, provided it be applied in due proportion, cannot injure the constitution."—Vol. II. p. 543-4.

Dr Graves speaks highly of the service he has derived in chronic diseases from the opposite mode of administering calomel and other preparations of mercury; and mentions by way of illustration a case where periostitis, nodes, venereal eruptions, &c., succeeded each other for more than two years, which was cured in less than three months by the administration of *one grain* of blue pill daily; and a case of epilepsy, in which one grain of calomel was given every night for two years, did not produce salivation or any other manifest constitutional effect, and yet at the end of that time the fits were stopped.

Dr Graves corroborates the testimony of Dr Bennett regarding the use of cod-liver oil in phthisis and strumous diseases generally. So also have certain London physicians, Sir Charles Scudamore,[1] Dr C. J. B. Williams,[2] and Dr Lankester,[3] during the last few months. Aconite has also been used by him with great benefit, both internally and externally, in the treatment of painful gouty rheumatic and neuralgic affections.

That Dr Graves can amuse as well as instruct his class, is pretty clear from the following remarks on the mode of administering carbonate of ammonia in effervescence. Our respected President must pardon the classical allusion made to his name; the lecturer doubtless could not resist so apt a quotation.

"We had a woman in fever here some time ago, to whom we gave the carbonate of ammonia in a state of effervescence; and as the form in which we administered it proves extremely useful, I think it necessary to remind you of it. The carbonate of ammonia is given in excess, in the proportion of about two grains and a half in each draught, as you will perceive by the formula employed:—℞ Aquæ fontis f℥vss, Syrupi zingiberis f℥ss, Carbonatis ammoniæ ℥i; signetur, No. 1.

"The syrup of ginger is used to cover the taste produced by the excess of ammonia, and to prolong the effervescence. Every thing syrupy prevents the too rapid effervescence of the carbonic acid. If the acid and alkaline solutions consist of water alone, there is an instant extrication of carbonic acid, and it escapes as it were in a very rapid succession of bubbles; the patient scarcely has raised the vessel to his lips when effervescence is over. The syrup thickens the water, and thus offers a resistance to the extrication of the fixed air, and moreover gives the mixture a more agreeable flavour. You next proceed to prescribe the acid solution as follows:—℞ Acidi citrici ℥i, Aquæ fontis f℥iij; signetur, No. 2. And then you add, sumantur cochlearia duo ampla ex No. 1; effervescentiâ cum cochleare uno amplo ex No. 2.

"You perceive, gentlemen, that I am not so poetic as Dr Paris, who, with a phraseology almost Homeric, says, '*Sumatur in impetu ipso effervescentiæ.*'"[4]

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[1] Medical Gazette, Nov. 24, 1848.

[2] London Journal of Medicine, vol. i. pp. 1–18.

[3] Lancet, Dec. 23, 1848. [Report of a Meeting of the Westminster Society.]

[4] "So spake the guardian of the Trojan state,
Then rushed impetuous through the Icæan gate:
Him Paris followed."—*Pope's Iliad.*

We shall conclude this article with a few remarks on the alterations which Dr Watson's lectures have undergone since their first appearance in a collective form, and on certain deficiencies which we trust to see remedied in the next edition.

The additions to the present edition of Dr Watson's lectures, as compared with the first, are in our opinion somewhat more scanty than they need be, and embrace little more than a few extracts from, and references to Dr Burrows on Disorders of the Cerebral Circulation; Dr Latham's Lectures on Diseases of the Heart; Dr Budd's volume on Diseases of the Liver; Dr J. H. Bennett's memoir on the Structural Relation of Oil and Albumen in the Animal Economy; the recent investigations of Johnson, Toynbee, and Simon on the Kidney; those of Garrod on Gout and Scurvy; and the published lectures of Dr Bence Jones, of Dr West, and of Mr Paget.

We observe that our author considers that "Dr Burrows has done the science of medicine an essential service," by showing "most convincingly that Dr Kellie's conclusions were erroneous." We entertain a high feeling of personal and professional respect for Dr Burrows; yet we must confess that, on a strictly physiological question, we prefer the opinion of a professed physiologist of established reputation, such as Dr John Reid; and we think, if Dr Watson had read that gentleman's review of Dr Burrows' volume in the *Monthly Journal* for August 1846, he would hardly have expressed so positive an opinion as he has done. The subject is one of high importance in consequence of its many practical bearings; and we are glad to find that it has been, during the last few months, taken up by Hamernik of Prague.[1]

As we are upon the subject of Dr Burrows' work, we may also remark, that Dr Watson agrees with him (and perhaps even goes further, see Vol. I. p. 489) in supposing, that the tendency to apoplexy increases in a direct ratio with advancing years. Dr Richard Quain has recently shown by statistical evidence, that persons between the ages of fifty or sixty are considerably more liable to apoplexy than those who are *older* or younger; an opinion, we believe, pretty closely coinciding with that of Hippocrates.

Dr Bence Jones has contributed a few plates of urinary deposits. Although they cannot be said to be inaccurate, yet they are not very successful in conveying to the unpractised eye a distinct idea of what they are intended to represent.

Our readers are well aware that much has been done within the last few years abroad, as well as at home, to elucidate the morbid anatomy and pathology of renal disease. Dr Watson has availed himself, as we have already observed, of the labours of Johnson, Simon, and Toynbee, but seems to be unacquainted with those of

[1] Physiologisch-patholog. Untersuchungen über die Verhältnisse des Kreislaufes in der Schädelhohle, in the *Prager Vierteljahrsch.* 1848.

our colleague Dr W. T. Gairdner, which were originally published in this *Journal*, and have since appeared in a separate form; neither is there the slightest allusion to the labours of any of the continental (especially German) pathological anatomists, some of which are of great value, and are not yet sufficiently understood or appreciated in this country.

The last addition we shall notice, is contained in the note to p. 840 of Dr Watson's second volume. It is an extract from a letter by Dr Bowling, an American physician, on the treatment of senile prurigo.

"I have in the last fifteen years prescribed for a great number of cases of *prurigo senilis*, and I can say with a most rigid adherence to truth, that I have not failed in a single instance to effect a permanent cure.

"I direct that the affected parts be sponged for a minute or so with good apple vinegar, and then be allowed time to dry. After this they are to be *smeared* over with the citrine ointment (*unguentum hydrargyri nitratis*). The applications are to be made twice a day. The cure is usually effected in a week. I have never known the constitutional effects of the mercury to be developed in this treatment save in a single instance, and then very slightly.

No one at all conversant with the rapid progress of medical science during the last half dozen years, can help feeling that many other important additions might been introduced, especially in the early portion of the first volume, devoted to *pathology as it relates to medicine*. A considerable portion of the lectures on "the morbid alterations of the fluids," "on inflammation," and "on cancer," is especially deficient in this respect, although we see occasional attempts on our author's part (as for instance in p. 147, lecture eleven) to remedy the evil. We are, however, well aware that he had an apology to offer which we cannot gainsay. The excellence of his work in its original form, has been one great means of placing him in a position leaving little time or probably inclination for any thing beyond the practical daily duties of a profession of which he is one of the brightest ornaments. G. E. D.

Leçons sur les Phénomènes Physiques des Corps Vivants. Par C. MATTEUCCI. Paris: 1847.—*Lectures on the Physical Phenomena of Living Beings.* By CARLO MATTEUCCI, Professor in the University of Pisa. Translated under the superintendence of JONATHAN PEREIRA, M.D., F.R.S. London: 1848.

Elements of Natural Philosophy; being an Experimental Introduction to the Study of the Physical Sciences. By GOLDING BIRD, A.M., M.D., F.R.S., F.L.S. London: 1848. (*Continued from* p. 406.)

WE now proceed to consider the physiological effects of dynamic electricity on the living tissues of the body, setting aside for the present the chemical changes caused by the passage of the current through its fluids.

It is impossible to obtain a galvanoscope of greater delicacy than the motor nerve of an animal recently killed, the smallest disturbance of its electric equilibrium being attended with contractions of the muscles to which it is distributed. Frogs are generally employed in galvanic researches, from their possessing, unluckily for themselves, a greater power of resisting the tortures to which they are exposed, and of retaining their excitability for a longer period after death than warm-blooded animals. If, in the amputated extremity of a frog just killed, a galvanic current from a single pair of plates is passed through the sciatic nerve, which has been carefully removed and insulated from the muscles of the thigh, it generally happens that a contraction or jerk of the limb takes place both on the closing and opening of the circuit, while in the interval, during the continued passage of the current, the muscles remain at rest. When, however, the parts have somewhat lost their excitability, contractions only occur on the closure of the circuit, when the current is made to pass from the spinal to the peripheral parts of the nerve, and, on the opening of the circuit, when it is flowing in the contrary direction. In order to facilitate the study of these phenomena, M. Matteucci has named the currents "direct" or "inverse," as they pass in the same or in the opposite direction to that in which the motor nervous influence is transmitted. When the uninjured sciatic nerve of a living animal is experimented on, not only are there observed contractions in the leg, but also movements of a reflex character in the muscles of the back, and symptoms of severe suffering, attendant on the commencement and cessation of both the "direct" and "inverse" currents,—effects due to the electrization of both the motor and sensory filaments of the nerve. These phenomena rapidly disappear with the frequent repetition of the experiment, and the consequent exhaustion of the nervous excitability, and the animal presently becomes insensible, unless to a more powerful galvanic stimulus; a second period in the action of the electric current may, however, be distinctly recognized before its effects entirely cease, in which the *contraction of the leg* occurs only with the commencement of the "direct" and the cessation of the "inverse" currents, while the *reflex movements and cries of the animal* accompany the commencement of the "inverse" and cessation of the "direct" currents. Such are the statements of M. Matteucci, hence he considers that there are two periods in the action of the current on the nerve; but the researches of Ritter and Pfaff indicate that, in the nerve of the frog, there may be observed three different stages of excitability;—the first, which occurs directly after the removal of the limb from the animal, being marked by contraction only with the commencement of the "inverse" and cessation of the "direct" currents—the second, by contractions both on the opening and closing of the circuit, whether the current is "direct" or "inverse"—and the third, by phenomena the reverse of those occurring in the

first stage, viz. by contractions with the commencement of the "direct" and cessation of the "inverse" currents. MM. Matteucci and Longet, when experimenting on nerves of purely motor function, have observed effects identical with those occurring in the first stage of Ritter and Pfaff, and we have occasionally obtained similar results on galvanizing the sciatic nerves of frogs killed while under the influence of chloroform.

We have seen that contractions are only produced by galvanic stimulation at the opening or closing of the battery circuit; but it has been observed by Pfaff and others, that a remarkable change is effected on the excitability of the nerve by the passage of the continued current, its action being either exalting or depressing as its course is "direct" or "inverse." If the "direct" current is allowed to circulate uninterruptedly through the nerve of a prepared frog's leg for a period of twenty or thirty minutes, it will be found that no contractions occur on afterwards opening or closing the circuit. But the excitability of the nerve thus exhausted may, according to Matteucci, be again restored by subjecting it for a short time to the continuous passage of the "inverse" current; and, by alternately reversing the direction of the current, the appearance or disappearance of the phenomena in the muscle may be obtained at will. On the other hand, if the fresh nerve is traversed by the continued "inverse" current for ten or fifteen minutes, such a high degree of excitability is produced that the interruption of contact with the battery is followed by violent tetanic contraction, lasting for several seconds, or, as we have observed, minutes, afterwards; it is, however, instantly arrested by again closing the "inverse" circuit, after the occurrence of a momentary increase of contraction. The artificial state of exaltation or depression of the nervous energy, caused by the continued current, gradually ceases under the influence of repose, and it has been remarked by M. Matteucci that this occurs with a degree of rapidity proportionate to the natural irritability of the nerve.

It has been observed by Pfaff, that the muscular contractions are always energetic in proportion to the length of nerve subjected to the influence of the battery. This may be readily proved if, with a frog's leg in the third stage of excitability of Pfaff, we apply the positive pole to the middle of the nerve, and with the negative pole touch points of it each time more distant from the periphery; the contractions will be found to diminish as the poles approach each other, and to cease altogether when they are brought opposite at the same point of the nerve. If, now, the negative pole is continued in its course to the spinal extremity of the nerve, the contractions reappear and increase as the poles recede from each other; we first have the decreasing influence of the "direct" current, then the point of no action where the current is passing transversely through the nerve, and afterwards the increasing influence of the "inverse" current.

There is no evidence whatever to induce us to believe that the nerves have a higher conducting power for electricity than the other tissues of the body; that in the natural state they ever act as conductors of that force from the brain or spinal cord to the muscles; or that the contraction of muscle under the galvanization of its nerve is other than the result of the irritation of the nervous principle, whatever that may be; that the two forces, electricity and the nervous principle, however intimately correlative, must be considered distinct, is proved by the effects of a moistened ligature passed round the nerve, which prevents the transmission of its peculiar influence to the muscles while the passage of the electric current remains uninterrupted.

During the course of his experiments, M. Matteucci observed a class of very remarkable phenomena resulting from the application of the nerve to a muscle during its contraction; to these he has given the name of "induced contractions." It appears that if the nerve of the frog galvanoscope is laid upon the thigh of another animal denuded of its integuments, each contraction of the muscles of the thigh, caused by the application of stimulus to the lumbar plexus, is accompanied by a simultaneous contraction of the galvanoscopic claw. If a second and third galvanoscopic leg be placed, the nerve of the second on the muscles of the first, that of the third on the muscles of the second, contractions will occur in the whole of the three legs each time that the muscles of the thigh are stimulated to contraction. When the inducing contraction of the thigh is tetanic, the induced contractions in the legs have a similar character. The interposition of lamina of paper, leaf-gold, mica, or selenite, between the inducing muscles and the galvanoscopic nerve equally arrest the transmission of the influence; while, on the other hand, the phenomena occurred with various non-conductors, such as layers of filtering paper soaked in oil, varnish, or Venice turpentine, placed between the muscle and the nerve. M. Matteucci is unable to decide whether these induced contractions are due to nervous induction, or to the effect of an electric discharge occurring during contraction.

M. Matteucci could scarcely observe any effect on galvanizing the nerves of animals killed by hydrocyanic acid, or repeated discharges of large batteries, though the application of the same stimulus to the muscles readily caused their contraction, from which he infers that the muscular fibre possesses a property of inherent contractility independent of the nervous system; in those poisoned by sulphuretted hydrogen, both muscle and nerve were alike insensible. On the other hand, he could not observe any modification produced in the excitability of animals which had perished in hydrogen, azote, carbonic acid, or chlorine.

Little need be said of the action of the current on the nerves of sense. Volta, Humboldt, and Ritter, have determined that it excites in each of them the sensation proper to it.

On bringing the poles of a battery of several pairs in contact with the tubercula quadrigemina, the crura cerebri, or the medulla oblongata, violent convulsions throughout the body and signs of suffering are observed; but their application to the cerebral hemispheres or cerebellum (?) is not attended with any result.

The muscular fibres of the heart, stomach, and intestines, are all thrown into action by the application of electricity to the ganglionic nerves, by which they are supplied; it seems to be a disputed question at present, whether those of the womb are affected by the same stimulus. The contractions caused by the application of galvanism to the nerves or muscles of organic life, continue during the passage of the current, and remain persistent for some time after its interruption.

The passage of the voltaic current through a voluntary muscle, is attended with the momentary shortening of all its fibres, both on the opening and closing of the circuit, "whatever may be the direction of the current relatively to that of the muscular fibres;" but the convulsion occurring on the closure of the circuit is more persistent than that which takes place at the opening of it. The discharge of a Leyden jar across a muscle, causes it to assume a continued state of contraction.

Effects of the Interrupted Current.—M. Matteucci observes that a much more rapid depression of the excitability of the nerve is effected by repeatedly interrupting and establishing the current, than by its continued passage, even when "direct." In describing the apparatus of Masson, used by him for that purpose, and which is no other than an electro-magnetic coil, with a rachet wheel to interrupt the battery circuit, he falls into the mistake of supposing that the current derived from it is that of the battery by which it is excited, whereas it is the induced current of the coil. There is little room for surprise at the death of a rabbit in five seconds, whose brain was made the path of the induced current from an electro-magnetic coil, excited by "only ten elements," or that Masson should succeed in wresting from an unlucky cat the whole of its nine lives in five or six minutes, by similar treatment.

The effect of the reiterated pulsations of currents produced by magnetic induction, in different organs, has been stated by MM. Edward, and E. H. Weber, Budge, Volkmann, and others, from which it appears, that the passage of this form of electricity through the nervous centres, is attended with a tetanic state of the muscles receiving their nervous influence from the part experimented on, which continues for some time after the removal of the stimulus (Volkmann)—that when the stream of shocks is transmitted through the medulla oblongata or the vagi, the peristaltic movements of the stomach and intestines are increased, while the contractions of the heart are immediately arrested, though they commence again under the influence of repose (E. Weber, Budge),—when the currents are transmitted through the substance of the heart itself,

its contractions are at first increased in frequency, but presently afterwards arrested (E. Weber). Budge, on the contrary, observed, that they ceased the moment the conductors were applied to the organ. According to our own observations, induced currents of low intensity have the effect of accelerating the pulsations when applied to either auricles or ventricles; while those of very high intensity, more or less quickly throw the part of the heart which they traverse into the most powerful tetanic contraction, from which it never recovers. The application of the conducting wires to arteries of small diameter (1-7th to 1-17th of a line), is followed by the gradual contraction of that part of the vessel through which the currents pass, until the channel is almost obliterated; the artery regaining its natural calibre on the removal of the stimulus. The larger arterial trunks and the capillaries, do not undergo the same change in size; but the circulation of the latter is retarded during the passage through them of weak induced currents, while by stronger ones it is altogether arrested from the coagulation of the blood.

Our space forbids us to follow M. Matteucci in his account of the therapeutical effects of electricity; but we hope to find other opportunities of considering this branch of the subject. In the mean time, we would impress upon our readers the necessity of attending, in their reports of cases treated by electricity, to the detailed description of the apparatus employed by them. Under the term "galvanism," it is the general custom to include the influence generated both by the voltaic battery and the coil machine; whereas the difference in the operation of the two instruments is almost equal to that of two distinct forces. As to the term "electro-galvanism," frequently used of late, it is altogether destitute of meaning.

Ueber die bisherige Beurtheilungs, und Anwendungsweise der ableitenden Methode. Von KARL FRIEDRICH HEINRICH MARX. Göttingen: 1848.—[*On the Derivative Treatment, and when and how to Employ it.* By K. F. H. MARX.] 4to. Pp. 99.

THIS is a memoir which perfectly defies analysis. It consists of seventy-seven distinct sections, in which are included the history of the derivative treatment from the times of the early Greek physicians to the present day; the consideration of the natural derivative action, sometimes established spontaneously by the kidneys, intestinal canal, or skin, for the relief of other organs; notices of the different materials used in the derivative treatment; and general rules and cautions to be borne in mind in the application of these materials. It exhibits signs of great research on the part of its author, and we can cordially recommend it to all who wish to make themselves acquainted with every thing that has been written (in Greek,

Latin, Arabic, German, French, Italian, Danish, and English) on a subject that is too much neglected in the therapeutics of the present day.

London and Provincial Medical Directory. 12mo. London: 1849.

WHEN the first volume of this work appeared, we noticed it with approbation, and suggested several alterations, which, we were glad to observe, were duly attended to in the subsequent issues. There is still, however, room for amendment. "London and the provinces" do not monopolize all that is important in the medicinal art of this country; and the value of the publication would be materially enhanced were medical lists for Scotland and Ireland introduced, as an addition to the present contents of the Directory. Should this hint be acted on, some party who knows about Scotland should revise the northern department. In the work we are informed that there are six places in Scotland where degrees are granted; but, of the six, the names of the teachers of only one university are given, and as it (St Andrews) is the only one of the whole that has no medical school, the propriety of giving it, in preference to the rest, is questionable. But, not content with this, the compilers, as if determined to emulate the functions of gazetteer-writers, give, in addition, the professors of St Mary's College, although it is a purely theological seminary—which information is so thoroughly gratuitous, that we question if it will be of the least service to any one of the purchasers of this Directory. As might be expected, no justice is done to Ireland—not one teacher's name in connexion with the Emerald Isle having been deemed worthy of record.

Some practitioners continue to be unmercifully lengthy in enumerating their monographs and periodical writings; as the work professes to be an enumeration of names of persons, and not lucubrations, free excision should be practised in this department. There are also some autobiographical memoranda that might be suppressed with advantage; such as, that one gentleman "was appointed physician of the Russian fleet, and for his services received the *thanks* of his imperial Majesty." The obituary, too, appears to be entirely a paste-and-scissors work, the whining eulogiums of the provincial newspapers being cut out and inserted *en masse*. Thus we have some deaths recorded as "greatly deplored," "regretted by all," &c.; while others are more elaborately set down as "universally esteemed and lamented," "highly respected and deeply regretted," "deeply regretted and universally lamented," &c. &c. There is no classification here, and it would be far better to insert the provincial like the metropolitan deaths, without note or comment.

Notwithstanding these drawbacks, the Directory has been of essential service to the medical profession, and we gladly recommend it as a useful work so far as it goes, and hope that, in 1850, we shall have it in our power to speak of it with unqualified pleasure.

A Treatise on the Diseases of the Heart and Great Vessels, and on Affections which may be mistaken for them; comprising the Author's view of the Physiology of the Heart's Action and Sounds. By J. HOPE, M.D., F.R.S., &c. Fourth Edition. 8vo. London: 1849. Pp. 609.

IN this new edition of a work, so well and so favourably known to the public as Hope on the Diseases of the Heart, it is not necessary for us to do more than mention its existence, in order to secure for it a place in the libraries of a large section of the medical profession. This volume contains at a reduced price, and in a portable form, all of Dr Hope's researches that is now important to the practical medical man or the physiologist; the only parts omitted from the text of the third edition, being the acrimonious personalities and controversies with which its pages were so plentifully strewed, and which, however interesting to the gossips, ought, we think, to be as soon as possible buried in oblivion, both from regard to the dignity of the profession and the credit of the different parties engaged in them. We need not say, that the work is one without an intimate knowledge of which the education of no physician can be considered at all complete; and to the student, in particular, we have no hesitation in recommending this edition, as being equally in virtue of its form, price, and omissions, better suited to his purposes than any previously published.

Medical Jurisprudence. By ALFRED S. TAYLOR, F.R.S., Licentiate of the Royal College of Physicians, &c., Professor of Medical Jurisprudence and Chemistry in Guy's Hospital. Third Edition, 12mo. London: 1849. Pp. 849.

THE present edition of Mr Taylor's celebrated and successful work has been in great part re-written; its table of contents has been made much fuller, and its index also forms a better guide to the subjects treated of than in any former edition. We regret that the pressure of other matters prevents us at present from entering at length into an examination of the added and altered portions of this work; but the review of the recent work of Dr Taylor on Poisons, will perhaps excuse us in the eyes of our readers. We can state, however, that we have examined with some care the additions in the sections on Wounds, Infanticide, and Legitimacy; and have been astonished at the care and diligence displayed in accumulating all the data bearing on the subject. Few, if any one in these kingdoms, have equal opportunities with Dr Taylor for becoming acquainted with the proceedings of the courts in important medico-legal cases; and we feel bound to say, that he has made the fullest and most impartial use both of his own experience and that of others; and has produced a work which, notwithstanding a recent decision of the very college of which he is a licentiate, we believe to be "the best English work on (Medical) Jurisprudence."

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVIII.

MEETING V.—*Wednesday, February* 7, 1849.—JAMES SYME, Esq., President, in the Chair.

CHLOROFORM IN SURGICAL OPERATIONS.

A COMMUNICATION from Dr William Keith, surgeon to the Aberdeen Infirmary, was read, "On the safety and suitableness of chloroform as an anæsthetic agent in some of the more complex and serious operations in surgery."

[This paper will be published in our next Number.]

Mr Syme did not agree with the author in recommending the high operation even for the removal of large calculi. He feared, also, the consequences likely to result from the performance of lithotrity upon *insensible* patients, by operators less expert than Dr Keith. If pain was given by the operation of crushing the stone, he believed some mischief was being done; but the mere abolition, on the part of the patient, of the power of expressing suffering, could not be held as evidence that the operation was well conducted. He agreed with the author in all that regarded the use of chloroform in lithotomy. He had himself used chloroform in every operation in which it could conveniently be given, and without any bad consequence fairly attributable to its use. He felt bound now to add, to what he had formerly said and written on this subject, that by the use of chloroform the shock of very severe operations was lessened; and particularly instanced a case of amputation at the hip-joint, recently performed by him at the Infirmary, upon an exhausted subject, who, he believed, owed his recovery to the employment of chloroform during the operation.

Dr Simpson expressed the pleasure with which he had listened to the observations made by the chairman; he considered them the highest possible evidence in favour of the use of chloroform in surgery. A conversation then ensued, in which Dr Simpson, Mr Syme, Professor Miller, Mr Roberts, and Dr J. A. Robertson took part, regarding sexual manifestations, cursing, obscene language, and other alleged effects of chloroform. None of the speakers had observed cases of sexual excitement; but the use of improper language had been frequently noticed during operations in the Infirmary and elsewhere. Mr Miller thought that these unpleasant manifestations were most frequent among patients from the lower classes, addicted to the abuse of ardent spirits; and that they might often be prevented by inducing sleep rapidly, and without unnecessarily agitating the patient by noise, motion, or surgical examination, while administering the chloroform.

ON THE AIR-TRACTOR, AS A SUBSTITUTE FOR THE MIDWIFERY FORCEPS. BY PROFESSOR SIMPSON.

If the opposed and adapted surfaces of two bodies have the air removed from between them, then the external atmosphere presses these bodies together, and consequently keeps them united with a force equal to about 15 lbs. upon each square inch. A round disc of two inches in diameter, would thus (if the exhaustion of the air were complete) adhere to any proper surface to which it was adapted with a force of 47 lbs.; or, in other words, it would require a power of traction equal to a weight of 47 lbs. to separate it from the surface to which it was attached. A round disc of two and a half inches would adhere with a power amounting to 73 lbs.; a disc of three inches, with a power

amounting to 105 lbs.; and one of three inches and a half would sustain a dragging force of 143 lbs. before it separated.

In physiology, more especially in comparative physiology, nature frequently employs this power of atmospheric pressure for the purposes of prehension, locomotion, adhesion, &c. She has provided, in other words, various animals with suckers or organs by which they are enabled to create a more or less perfect vacuum between the surface of these suckers, or cotyledons, and any body or surface on which these suckers rest, so as to produce a pressure of the atmosphere on the external surface of the suctorial organ, and thus cause it to adhere firmly to the body on which it was placed. The suckers on the head of the *remora*, on the feet of the *gecko*, on the legs of the *dytiscus*, in the mouth and adhering acetabulum of the *distoma*, on the arms of the *loligo* and *octopus*, in the disc of the *patella*, &c. &c., are illustrations of nature taking advantage of the principle of atmospheric pressure for the performance of various physiological functions. The human infant, the common medicinal leech, &c., provide themselves with nourishment by availing themselves of the aid of the same principle.

In surgery, the principle of atmospheric pressure is converted to practical purposes in the operation of cupping; in one mode of fixing artificial teeth, &c.

Several of the older surgeons, from Ambrose Parè downwards, proposed to elevate depressions of the skull, particularly in children, by applying a sucker, or kind of cupping-glass or cupping-horn, over the seat of the depression. Between twenty and thirty years ago, Dr Neil Arnott of London suggested the possibility of applying the same principle in obstetric surgery.

The power that an air-tractor of 2½ or 3 inches in diameter, when fixed on the head of the child, should give us, is *theoretically* as much as is required in most, if not in all cases of tedious labours in which the forceps are at present employed. In trying various experiments upon the power of traction usually employed by the forceps, Dr S. found it not in general to amount to more than from 25 to 35lbs. Some practitioners thought the common power of traction used by the short forceps to be below this. In a few rare cases, especially in long forceps operations, and where the obstruction was great, the power employed might rise to 40 or 50lbs. or more. The experiments upon which these results were founded, were made by traction with forceps fixed upon a Salter's spiral balance, used as a dynamometer.

In attempting to construct a proper obstetric air-tractor, a great variety of forms had been tried by Dr Simpson. The form which he found most effectual, consisted of a slender short brass syringe (1½ or 2 inches long) worked by a double-valved piston, like a breast-pump, having attached to its lower extremity a cup of half an inch in depth, and 1½ inch broad at its edge. Over this inner cup was placed a second cup formed of vulcanized caoutchouc, and so deep as to overlap the edge of the inner by six or eight lines. The mouth of the inner cup was covered by a diaphragm of very open brass wire gauze, and over it a piece of thin sponge, flannel, or the like, was placed, with the view of preventing injury to the scalp, and not allowing it to be elongated and drawn up into the vacuous space in the manner which we see occurring with the skin in the common operation of cupping. The parts thus applied to the child's hand consisted of caoutchouc and sponge.

Such an instrument, when fixed to the palm of the hand, lifted readily without detachment a weight of 30 or 40lbs. This Dr Simpson showed by experiments performed before the Society. An air-tractor, with a caoutchouc cup of three inches in diameter at its mouth, lifts, when applied to and stretched upon the hand, a weight of 60 or 80lbs.

The inner cup might be round, oval, &c., and vary in form and size. The outer caoutchouc cup would admit of much further improvement. A double cup of caoutchouc seemed to render the instrument stronger. Of course, with all this, the vacuum, however great, was still always more or less imperfect.

The air-tractor seemed to possess various advantages over the forceps. It was far less dangerous to the mother, as it was attached to the exposed part only of the scalp of the infant; the forceps were required to be passed high up between the head and maternal passages, and in incautious and inexperienced hands were apt to injure one or both. The materials of the air-tractor (caoutchouc and sponge) were safer to the mother and child than the material of the forceps (steel). The forceps always took up a certain amount of space between the head and passages; the air-tractor did not. The air-tractor was greatly less in size, and consequently far more portable. It could be applied with sufficient firmness and power to enable us to rotate the head, or change its *position*; as from, for example, an occipito-posterior to an occipito-anterior position, (the form of rotation most frequently required in practice.) It probably could be applied also to change the presentation—which the forceps could not effect—as, for instance, to bring down the occiput, when fixed there, in frontal presentations. Perhaps it may be found as useful, or more so, when the head is at the brim as when it is at the outlet. It may be made so as to fix upon the breech—in cases in which the forceps cannot be very readily or safely used. If found perfectly easy of application, it may enable the accoucheur, by adding a few pounds to the strength of each pain, to bring to a safer and speedier termination cases that would otherwise go on tediously, hour after hour, and in which we would still not choose to use so formidable an instrument as the forceps; for it is to be recollected that the danger of parturition to both mother and child increases in a ratio progressive with its duration. In cases of inertia of the uterus—the most common cases for the short forceps—it will probably be found specially applicable. And in such instances it is surely better to extract the child by a safe force, thus applied *ab anteriori*, than to effect its expulsion by the ergot of rye—which produces its result by forcing the uterus to push and press its parietes with renewed power and violence against the opposing body of the fœtus.

In conclusion, Dr Simpson stated that he had now used the air-tractor which he had constructed in several cases of labour, and with results answering his best expectations. But it doubtlessly admitted of much further improvement in construction, in mode of application, in working, and other details.

Mr Syme had seen, in cases of lacerated perineum, &c., deplorable evidence of the mischief which might be inflicted by rash hands working in the dark with such instruments as the obstetric forceps. He sincerely hoped, that Dr Simpson's invention would be found so efficacious as to supersede more dangerous means of delivery.

Mr Harvey and *Dr Margulies* had witnessed Dr Simpson's experiments in the Maternity Hospital, and expressed themselves quite satisfied that the instrument was safe and efficient. Drs Macaulay and Marr likewise made observations on Dr Simpson's communication.

MEETING VI.—*February* 21, 1849.—JAMES SYME, Esq., President, in the Chair.

OBSERVATIONS ON THE ASIATIC CHOLERA AS IT APPEARED IN ST PETERSBURGH IN 1848. BY DR MAURICE MARGULIES, PHYSICIAN IN ORDINARY TO HIS HIGHNESS THE GRAND DUKE ALEXANDER, PRINCE OF HESSE.

(*Translated from the French by Dr J. M. Duncan.*)

Ever since the year 1845 there has been an extraordinary prevalence of epidemic diseases in St Petersburgh, and especially of typhus fever, influenza, dysentery, scarlatina, and intermittent fever. This circumstance is the more worthy of remark, as cholera seems every where to be preceded, for a longer or shorter time, by a series of epidemic diseases, which prepare the atmosphere, and so affect the individual constitutions of some persons as to render them susceptible of being taken with the new epidemic which is on the march to visit them. In the years 1830 and 1831 the same extraordinary prevalence

of epidemic diseases was observed in Russia, France, and Germany, to precede and afterwards give place to the cholera, which in these years swept across Europe.

On the 4th of June 1848 there arrived at St Petersburgh a vessel from Ladoga. It was on board this vessel that the first case of cholera occurred. A man, the subject of it, had come from a district where cholera was raging. During the voyage his health was good, but he was seized with the disease on the day of his arrival, and before he left the ship. On the same day several individuals in the most different and distant parts of the city were also suddenly seized with the malady, and that without apparently having had any communication with any one arrived from an infected district, or with any one already affected with the disease.

The manner in which Cholera thus appeared in St Petersburgh, and its subsequent progress, have confirmed me in the opinion that it is reproduced in two very different ways. Firstly, by a certain atmospheric constitution, produced by telluric or planetary influences, and by the epidemic diseases which prevailed before its outbreak; in other words, by a miasma developed under these influences. Secondly, it may appear in a place, after having gradually approached it step by step, from localities where it has raged, and having individuals or fomites for the instruments or means of its diffusion. In this second case it is said to spread by contagion.

Having this double mode of reproduction and development, cholera must be regarded as a contagious and miasmatic disease.

In considering a disease whose progress is so alarming, and whose attacks are as much to be feared as those of an assassin, the first question which naturally suggests itself is—How can immunity from it be obtained? For this purpose there are general and individual prophylactic means—the former intended to secure communities, the latter to secure persons, against the disease.

In St Petersburgh nothing whatever was done with the view of preventing cholera from reaching the city, either because the government thought it impossible to stop all communication from without with a city so large and important, or because several of the physicians of the Board of Health regarded the disease as not contagious, but as one originating epidemically and spontaneously, and whose progress no system of quarantine, however strict, could have any influence in arresting. In regard to the prophylaxis of individuals, it is scarcely necessary to say, that very few could avail themselves of the most efficient means—namely, to quit St Petersburgh, where the disease was raging, and seek a residence distant from its ravages.

Reasoning from the observations very generally made during the epidemic of 1831, that individuals living in a city where cholera prevails have their blood less arterialized, or more highly charged with carbon than in the normal condition, I have been in the habit of prescribing, as a preservative against cholera, the regular use of small doses of sulphuric acid, believing it to be a substance which has the power of changing venous into arterial blood; and, in the cases of persons of a nervous temperament, I add to each dose of the sulphuric acid a few drops of the tincture of nux vomica, as a stimulant to the nervous system, without affecting the sensorium, and as the best remedy against diarrhœa. I encourage the continuance of old habits of diet and regimen, excepting that salads and unripe fruits be abstained from. Further, I recommend the wearing of flannel over the abdomen, and by all means to encourage an active state of the skin. I may further in this place take occasion to mention, that the only effect traceable to the influence of the variations of the weather has been this, that the number of persons taken with the disease increases when the temperature rises considerably above the ordinary point.

The poor and dissipated were attacked by the disease in a very much larger proportion than the rich, a circumstance easily accounted for when we consider their manner of living, their privations, and the want of prompt medical aid at the supervention of the first symptoms of the disease. Poor patients

were generally required to be taken to some distance to an hospital, in very bad and ill-constructed carriages, and arrived generally in a moribund state, and died soon after in spite of the best and promptest medical aid. The prostration resulting from the disease is in general so much increased by the improper mode of transportation to the hospital, that I do believe they would better be treated in their own homes. And if the system of treating patients at their own houses can be carried out in London, as is proposed, it may be safely tried in St Petersburgh, where physicians are more numerous than even in the English metropolis.

At the same time with the cholera, there have prevailed in St Petersburgh acute diarrhœa, intermittent, rheumatic, and bilious fevers. In the year 1831, during the former epidemic, the presence of cholera seemed to banish and exclude all those diseases during its continuance. In 1831 a larger proportion of young than of old people was taken with the disease; but in the present epidemic a larger proportional number of old people than of children has been seized and carried off. I may add, the cases among children generally have ended fatally. It has not been determined whether, in St Petersburgh, males were more frequently affected than females with the disease.

From 80,000 to 100,000 of the labouring population left the city, either driven away by fear of the epidemic, or to seek for work and occupation, which could not then be found in the capital. In the month of October, when I left St Petersburgh for Great Britain, 20,000 had already perished from the epidemic, and certainly this number is not exaggerated. During the months of its greatest prevalence, June and July, the whole city bore an aspect so melancholy that I could have wished entirely to have escaped from it! The busiest streets were now almost empty, Scarcely a single person was to be seen except now and then a domestic bearing a prescription to a druggist, or returning with the medicine. No vehicles were to be heard except an occasional carriage bearing either a physician or a corpse, and the dead were very frequently unaccompanied by the ordinary train of relatives; either because they were themselves so sick as to be unable to render this last homage to their departed friends, or because they remained at home to care for other relatives lying sick, or because all the relatives had already succumbed to the epidemic. For my own part, it was when I was in the streets that I felt most depressed and sad, and this disagreeable feeling ceased only when I was engaged with the sick; for I have a profound conviction that the medical art can generally effect as much for those struck with cholera as for those labouring under any other serious acute disease.

During the months June, July, August, and September, I was entrusted with the care of a very large number of cholera patients, in hospital and in private practice. Some of the results of my observations I shall now attempt briefly to detail, premising that, and during the first eight days of the epidemic, it was impossible either to observe or watch the disease, or attempt its treatment, because during that time its intensity was so great, that in a town so large the physician seldom, if ever, arrived to see his patient before he was either past all hope or already dead. It was only at a later period, when the public in general became frightened by the sad experience of others, that individuals began to take precautions as to their manner of living, and considered it expedient to send for a physician as soon as any suspicious symptoms supervened.

The general character and circumstances of the attack varied much in different cases. They may be classified, in my opinion, under four principal groups or forms.

FORM I.—Characterized by vomiting and purging of a whitish serous fluid, sometimes mixed with a reddish matter, the bile being altogether absent. Pulse not to be felt. Coldness of the superior and inferior extremities, of the abdomen, and particularly of the face and the tongue. Cold perspirations. The respirations very difficult, yet scarcely perceptible, and sometimes, but rarely, accelerated. During all this time the functions of the kidneys are en-

tirely arrested, and yet the patient constantly seeks for cold drinks. Subsequently, spasms of the inferior extremities and of the abdomen. After, and sometimes also before, the coming on of these symptoms, the patient complains of a feeling of oppression in the epigastric and hypogastric regions. When they first commence, he is so restless as to be unable to keep the same position many minutes; this state does not continue long, but gives place to an apparent apathy, which is the more astonishing, for the patient retains all his mental powers, and has a strong premonition that his last hour is come.

Form II.—In this form, a person in good health finds himself suddenly seized with spasms of the inferior extremities, and of the abdomen, causing intolerable pain. The respiration is oppressed. The face becomes blue and pinched, the eyes, sunk into their orbits, are surrounded by a dark circle, and bear an impression which inspires a spectator with fear. The skin is covered with a cold sweat. The whole body is cold, even in the mucous cavities, and yet the patient generally complains of a feeling of heat, and seeks for cold drinks. The function of the kidneys is completely suppressed. The pulse can be felt neither over the heart nor elsewhere. The voice is scarcely audible, and is changed into the characteristic *vox cholerica*. In this form, as in the first, the patient retains his mental powers, but does not exhibit the same apathy and indifference. There is neither vomiting nor purging, but merely uneasiness of stomach, hiccough, and desire to vomit. The patient dies after some hours of suffering.

Form III.—Two or three days before the attack, premonitory symptoms present themselves. The digestive functions are deranged. After eating or drinking, the patient feels as if his stomach were distended with fluid. There is constant noise and rumbling in the abdomen. The stools are irregular, somewhat loose, and mixed with an extraordinary quantity of bile. The tongue is yellowish white. If, when in this state, the patient neglects medical treatment, there supervenes diarrhœa, followed after some hours by vomiting, continual thirst, and a train of symptoms exactly as in the form first described.

Form IV.—In this, the malady commences with general indisposition, vertigo, and slight feverishness. The tongue is warm and red. The pulse is frequent, and not small. After some hours, or even a day, the patient begins to vomit, and, if the disease is not successfully combated, it quickly passes into the second or third form already described. At a later period, if the patient survives, this form has a tendency to merge into an affection, resembling in many respects the ordinary typhus fever.

Several months before the cholera appeared in St Petersburgh, a great number of the inhabitants complained of indigestion, constipation, inclination to sleep, unaccountably low spirits, and general irritability of the nervous system, symptoms which are all explained by the peculiar highly venous or carbonized state of the blood, of which I have already spoken, and which doubtless, in my opinion, exerts a great influence during the prevalence of cholera. The cholera may, in fact, be regarded as a disease which primarily affects the blood, chemically changing it in a manner that remains yet to be discovered and explained by organic chemistry. In consequence of this morbid or altered composition of the blood, there come on the symptoms of cholera, varying according to the special organ or system upon which this changed blood exerts its injurious influence. In any individual, the organ or part attacked is that which offers the least resistance (so to speak) to the poisonous influence of this changed blood.

If the sympathetic nerves are the parts first and most readily affected, then the symptoms take the *form first* described.

If the medulla spinalis is most affected, then we have the *second form*.

If the mucous membrane of the stomach is affected, then we have the *third form.*

If the mucous membrane of the great intestine is chiefly affected, then we have the *fourth form.*

If in the course of cases of the third and fourth forms, the medulla spinalis or the sympathetic nerves become affected, directly or by way of reflexion, then the symptoms are changed from those of the one form to those of another. This passage from one species to another is the more easily understood when we consider that the blood, which is primarily affected, acts directly at the same time upon the nervous system, as well as upon all the other parts of the body.

Stages of the Different Forms.

Form I.

1st Stage.	Vomiting and purging.
2d „	Coldness of the body.
3d „	Pulse imperceptible.
4th „	Recovery; merging into another disease; or death.

Form II.

1st Stage.	Spasms.
2d „	Pulse imperceptible.
3d „	Recovery or death.

Form III.

1st Stage.	Premonitory symptoms.
2d „	Vomiting and purging.
3d „	Passes into the 1st form.

Form IV.

1st Stage.	Premonitory symptoms.
2d „	Vomiting.
3d „	Passes into the 2d or 3d form, or into typhoid fever.

In all the forms of cholera observed at St Petersburgh, the secretory functions appeared to lose their activity, as if the various organs of secretion found the blood presented to them altogether unfit or unsafe for the performance of their functions. The urine and bile were no longer secreted, and in nurses the supply of milk is stopped. The evacuations from the stomach and rectum were not the secretions of those organs as in cases of ordinary vomiting and diarrhœa, but consisted of the morbid liquids which escaped easily from the blood in consequence of its changed composition. The blood seemed to lose a large portion of its serum by the mucous membrane of the stomach and large intestine, while the solid parts remained in morbid excess. Sometimes with the serum thus discharged from the blood, there is mixed a small quantity of its solid constituents in the form of a clot.

The cholera has been observed at St Petersburgh to end in three different ways:—

1st. In complete cure in two or three days.

2d. In an incomplete cure, some organ remaining affected. Thus there may remain liability to vomiting after eating or drinking, or constipation, or insufficient action of the kidneys, or continued diarrhœa.

3d. In death by paralysis of the heart or of the lungs, their nerves becoming affected, or else by coma, the result of the anæmic state into which the patient is reduced in consequence of the loss of the serum of the blood.

Death may take place within four or five hours, or in four or five days.

Sometimes the cholera was seen to pass into another disease, characterized by symptoms referrible to the brain and the intestines. This has generally been termed cholera typhus. After one or five weeks, this state ends either in death or recovery.

The results of *post-mortem* examinations made in St Petersburgh have been altogether unsatisfactory, nothing characteristic having been discovered. The traces of inflammation sometimes found in the membranes of the brain and spinal marrow, have been regarded as accidentally coexistent with the cholera, rather than as having any essential connexion with it.

Sometimes patients have recovered from the most violent attacks, where every symptom presented in its highest intensity. Patients even have recovered, in whom, for many hours, no pulse was to be felt at the wrist or in the

carotids, and no pulsation of the heart could be discovered. It is true that such cases were rare; but, nevertheless, the few remain as examples of what medical aid can do in this disease, where the powers of nature are so completely annihilated as to afford in themselves no chance of restoration. It has always been considered necessary to watch patients very carefully during their convalescence, as a relapse of the disease has almost invariably proved fatal.

Treatment.

It would be tedious to enter at length into the discussion of the merits of all the various drugs which were tried in St Petersburgh in the various stages of cholera as it appeared among us; I shall merely mention those which have appeared to me to be most efficacious and successful.

In treating the premonitory symptoms, I was in the habit of administering small doses of nux vomica and ipecacuanha; and after having put the patient to bed, I ordered frictions with spirits of wine, in order to bring out free perspiration.

In treating patients during the attack, I have always guided myself according to the form which the disease takes on.

The remedies administered may be divided into external and internal.

External Applications.

In all the four forms this part of the treatment is the same, except where the spasms and the general nervous symptoms were very distinct, and then the external applications were resorted to with more than usual diligence. The liniment which I most frequently used was composed of camphorated oil, oil of turpentine, and liquor ammoniæ. To be rubbed over the inferior extremities and the spine for five minutes every second hour.

When the skin appeared still to retain all its functions, and to be capable of absorption, I usually ordered a liniment compounded with nitric and sulphuric acids.

Sinapisms should be applied to the extremities, and, if it can be had at the time, a vapour bath should be tried. This means has often been followed by a favourable critical perspiration.

When I discovered local irritation in any organ, the cupping-glasses or a few leeches were applied on that organ, with apparent relief and success.

Medicines taken Internally.

The internal treatment was varied, as I have said, by the form or type of the malady.

In the first form. For the vomiting, small doses of fuming nitric acid in distilled water (ten drops of a mixture of half a drachm of acid to an ounce of water) was most successful. For the diarrhœa, small lavements, with five or ten drops of the tincture of opium, were very generally successful. After the vomiting has been stopped, medicines are given to arrest the diarrhœa, and for this purpose I generally gave a few drops of tincture of nux vomica and of opium, every quarter of an hour.[1] When the pulse became feeble or imperceptible, then resort was had to a mixture, compounded with phosphoric ether (1 grain of phosphorus to ʒj of ether). Five to ten drops to be taken every quarter of an hour. If the pulse returned, then all specific treatment was given up.

In the second form. For the spasms, ipecacuanha was used in large doses, to excite vomiting. When the pulse became imperceptible, then small doses of nux vomica and camphor were administered.

[1] I may add, that I believe nux vomica to be one of the best sedative astringents which we possess for the treatment of other obstinate forms of non-inflammatory diarrhœa. When with the Russian army of the Caucasus in the summer of 1845, I used nux vomica with great and rapid success in a bad type of miasmatic dysentery that appeared among us.

In the third form. The treatment is conducted as in the commencement of the first form, characterized by vomiting and purging. If the nitric acid does not succeed in arresting the vomiting, then ice pills often proved very effectual.

In the fourth form. The treatment of the vomiting and diarrhœa was conducted as in the other forms. If the diarrhœa became bilious, it was necessary to add to the drops of tincture of nux vomica and opium, some drops of the tinctura rhei aquosa.

The diseases which were observed by me at St Petersburgh to follow the cholera, were affections of the membranes of the brain, of the intestines, and of the liver. They demanded an attentive treatment, in which it was found necessary to keep in mind that they were merely secondary affections, and had a tendency to assume a nervous character.

In general these secondary affections were treated according to the symptoms and organs affected. If vomiting was found to continue, it generally yielded to aromatic tonics. If the diarrhœa persisted, then nux vomica, with ipecacuanha or opium, was usually successful. No special treatment was required for constipation of the bowels, and feeble action of the kidneys, which often remained, but which very generally disappeared, after the functions of the skin had been perfectly restored.

In a letter from St Petersburgh, dated 20th January, I learn that the cholera has again increased, as might be expected to arise, from the great influx of strangers to the city during the winter. The winter is the season at St Petersburgh, as spring and summer are at London. It is to be hoped that the intense colds of the winter will have the effect of completely driving away that epidemic.

Mr Wood, after alluding in highly complimentary terms to the observations of Dr Margulies, and expressing his sense of the advantage that the Society derived from communications which were, like the present, the result of large experience in foreign countries, proposed that the thanks of the Society be conveyed to Dr Margulies by the President, and that he be requested to allow his communication to be published along with the report of the Society's proceedings in the Journal. This motion was seconded by *Dr Robertson*, and being carried unanimously, the *President* addressed Dr Margulies, and offered him the thanks of the Society for his interesting communication.

[The remainder of this meeting of the Society was occupied by communications regarding individual cases, and exhibitions of morbid parts. We are obliged to defer the report of it till next Number.]

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

(From the Medical Gazette, Feb. 9, 1849.)

ANEURISM OF THE AXILLARY ARTERY. COMPARATIVE ADVANTAGES OF LIGATURE OF THE SUBCLAVIAN AND AXILLARY ARTERIES.

Mr Hancock, after detailing an interesting case of aneurism of the axillary artery, in which the subclavian was tied in the usual situation, and suppuration of the sac occurred after the wound had almost healed, with fatal hemorrhage ten days afterwards from the abscess,—proceeded to give a statistical account of the result of operations for the cure of traumatic and idiopathic axillary aneurism, in confirmation of his impression that the ligature should be placed on the axillary artery itself "in all cases where the mischief can be traced to a direct injury, and where the size of the tumour admits of our reaching this vessel:" the risk of hemorrhage would, he considers, be thus materially diminished, if not altogether obviated, in case of the sac suppurating, as in the present instance.

Mr B. Phillips said, that in cases requiring ligature in this neighbourhood,

the axilla frequently presented such appearances that the surgeon would scarcely feel justified in performing any operation below the clavicle ; and this would explain, perhaps, the reason why this operation was so little resorted to. It was one, however, that surgeons usually recommended, when practicable; and Mr Hodgson, if he had not said so much in words, had, by implication, given it his sanction. Desault and Pelletan had also adopted the practice ; therefore Mr Hancock was in error in supposing he was recommending any novel mode of proceeding. The operation on the axilla was the rule, and not the exception, as Mr Hancock had stated.

Mr B. Cooper expressed an unfavourable opinion of the operation below the clavicle, as being of great difficulty, and presenting no compensating advantage.

Mr Hancock had not formed his opinion solely on his own experience, but on a number of cases collected from the various European journals and works on the subject. These statistics were embodied in the paper. He was surprised at the remark of Mr Phillips respecting the opinions of Mr Hodgson and other surgeons in regard to the operation in question. He (Mr Hancock) had no recollection of any such statement in the works of these authors. The cases enumerated in the paper were under three heads :—First, those in which the subclavian artery was tied for idiopathic axillary aneurism ; secondly, those in which the subclavian artery was tied for traumatic axillary aneurism ; and thirdly, those in which the axillary itself was tied for traumatic aneurism of that vessel. Of the first, there were twenty cases—ten recovered, ten died ; of the second, eighteen cases—eight recovered, and ten died ; of the last, there were four cases, all of which recovered.

Mr Erichsen had paid some attention to the subject under discussion, and the result of his inquiries had been the formation of the very opposite opinion to that promulgated by Mr Hancock. He might say that he agreed with the author in his definition of aneurism, and excluded all cases which did not come under that category. Now, in eight cases in which the subclavian had been tied above the clavicle for traumatic axillary aneurism, seven had recovered ; and in four or five cases tied below the clavicle, all had recovered. He said it was necessary in these inquiries to distinguish between spontaneous and circumscribed false aneurism, as in the first the mortality was greater in an equal number of cases when the subclavian was tied. In the operation recommended by the author, it must not be forgotten that you run the risk of cutting off altogether the collateral circulation of the arm, and the probable consequence would be, gangrene of the extremity—a consequence which was not unusual. As to the relative difficulties of the two operations, there could be no comparison between those of the former and those of the latter.

Mr Hancock denied the validity of Mr Erichsen's statistics, and said that, as he had before remarked, he had drawn his conclusions from a much larger number of cases. With regard to the mortification, Mr Erichsen had proved how futile was such an objection, for in his own four cases they all recovered.

Mr B. Cooper would mention a case embodying a point of interest, not that it bore strictly on the paper, but because no writer on surgery had alluded to the accident he was about to detail. He had secured the subclavian external to the scalenus in a thin subject. The operation offered no difficulties whatever, but immediately the vessel was secured the patient was seized with a constant and continued short cough, which went on till he died. On examination, the phrenic nerve was found to be uninjured, but highly inflamed, as was its neurilemma. He said that unsuccessful cases were often more instructive than successful ones.

Mr Hancock took the liberty of observing, that the case just related was an additional argument in favour of the position he had assumed, inasmuch as in his operation the phrenic nerve would have been out of the way.

WESTMINSTER MEDICAL SOCIETY.

(*From the Medical Gazette, Feb.* 2, 1849.)

CHLOROFORM IN MIDWIFERY.

THE Westminster Medical Society has been occupied during its last three sittings with a discussion on the above important subject. Unusual interest was excited by the occasion, the Society's rooms being nightly crowded with fellows and visitors.

Dr Snow, in opening the subject, said that the chief objections that had been made to the use of anæsthetics in midwifery were of an *à priori* kind, but that the question ought to be decided by the result of experience. Some objections had arisen from the supposed necessity of inducing a deep state of insensibility, and he was of opinion that if it were requisite to cause the same amount of insensibility in midwifery as is required in operations in which the knife is used, that would be a valid objection, as he considered that this state could not be continued for two or three hours without injury to the patient. But this amount of insensibility was not required in obstetric practice unless to arrest or diminish strong uterine action for a few minutes to facilitate turning the child. The suffering attendant on labour might often be greatly relieved, or even altogether removed, without suspending the consciousness of the patient. Towards the conclusion of labour it was usually necessary to carry the effect of the vapour a little further, but even then it should not exceed the second degree, or that condition of the patient in which the mental functions are not altogether suspended, but in which there is a dreaming or wandering state of the mind, the patient, nevertheless, usually remaining silent if not spoken to. In labours unassisted by manual or instrumental aid, the auxiliary action of the respiratory and even the voluntary muscles, continued without interruption when the chloroform was well managed. He considered that a medical attendant acquainted with the action of chloroform, and the mode of applying it, might administer it with propriety in all cases in which the pain was either severe or protracted, whether they fell within the division called natural labours or not. Dr Snow related two cases in illustration of the beneficial effects of chloroform. He used an apparatus in administering it, and gave a little at the beginning of each pain. He disapproved of Dr Simpson's plan of giving chloroform on a handkerchief, and more particularly of his practice of putting three or four drachms on the handkerchief to begin with. To show the danger of this practice, he alluded to the fatal cases published, and read some notes, furnished to him by Mr Henry Smith, of a case in which the patient very nearly lost her life from chloroform given in this manner preparatory to an operation. Chloroform was of great service in removing rigidity of the os uteri and of the perineum, and it had relieved puerperal convulsions in two cases on record. He concluded by remarking, that as all medical men were actuated in their views only by a desire for the well-being of their patients, the difference of opinion in the profession concerning the employment of chloroform in midwifery ought not to be attended with any acrimonious feelings.

Mr Greenhalgh related two cases in which he had given the chloroform previous to the use of the forceps. He had no doubt of the great value of the agent in these instances. In two cases of turning he had also employed this medicine with the best effect. He had used chloroform altogether in thirty or forty cases, and had never seen any ill effects result from it.

Mr I. B. Brown had employed chloroform in fifty-four cases. In all it had been of service. It had been objected to the use of this agent, that it caused the death of the child; but in no instance had this occurred in his own practice.

Dr Merriman mentioned a case in which the pulse had fallen to forty in a patient who had taken chloroform in the Westminster Lying-in Hospital.

Dr E. Murphy spoke at great length on the subject. He proceeded to argue that many of the objections to chloroform had been founded, not so much on

the real merits of the agent, as on our ignorance of its true action. Doubtless, further experience was required by many to test its merits, but it must not be supposed by the opponents of this most valuable medicine, that those who employed it used it indiscriminately, or gave it a character to which it was not entitled. What, then, were the cases in which it was to be employed? Certainly not in every natural labour, if it proceeded in that tranquil manner which did not call for interference. In those cases, however, in which there was great mental excitement, arising from extreme suffering, and which interfered with the progress of parturition, chloroform was a great boon; for it must not be supposed for an instant that these were true physiological pains, for they were due to causes of which civilisation formed the chief. It was only in savage life that real labour-pains were present. He summed up the cases in which chloroform was advisable, as those in which it allayed excessive irritability, and mitigated unusual pain—in the operations of midwifery, in which it not only facilitated the proceedings of the obstetrician, but relieved the intensity of the suffering consequent upon such interference.

Mr Gream objected altogether to the use of chloroform in obstetrics. The first objection he mentioned was the very uncertain quantity required to produce its effects: it had, indeed, a cumulative property which might suddenly develope itself and destroy the patient. He believed that a very short time only would elapse before chloroform would cease to be heard of in midwifery. Dr Rigby, it would appear, from an extract from a letter lately written by him, had already lost much of his confidence in it, and declared that it should be used only in the "latter stages of labour." Mr Gream then entered into a very lengthy and elaborate statement of his objections to this medicine. They were of two kinds, moral and medical. Under the first head, he quoted a humorous description of Professor Miller, of the first discovery of chloroform as an anæsthetic agent; this description he did not hesitate to consider as having the authority of Dr Simpson, who was not only present at the "convivial board," but the intimate friend of the author. The effects of the agent were very decided, at first simulating the "victorious" nonchalance of "Tam O'Shanter," and ending with the spectacle of the worthy Edinburgh professor kicking and sprawling under the tables and chairs. Mr Gream then alluded to several cases in which women had, under the influence of chloroform, made use of obscene and disgusting language. This latter fact alone he considered sufficient to prevent the use of chloroform among Englishwomen. The medical reasons consisted of the fact of his having heard of twelve cases of convulsions arising from its use; and five cases of mania following its employment. In addition to these serious results, he declared that it had already been attended with fatal effects in thirty-two instances. He alluded to the cases recorded by Dr Simpson, in one of which he (Mr Gream) attributed rupture of the uterus entirely to the chloroform; and he considered that in Dr Haartmann's cases the ruptured perineum was attributable to this cause.

Dr Reid was not an opponent to the use of chloroform in obstetric practice. He had, however, never advocated its employment, and his experience of late had tended to make him think less favourably of it than he formerly did. He entered at some length into three cases in which he had employed it. In the first instance, a lady who had always suffered severely during her first five confinements, in which he had attended her, but in all favourably recovered,—indeed, without a bad symptom,—had in her sixth confinement taken chloroform. He did not believe that it had at all diminished her sufferings, and from that hour to this, a period of three months, she had been a dreadful invalid, suffering from a train of nervous symptoms such as he had never witnessed, and of which Dr Merriman only recollected one parallel instance in his vast practice, and that was the result of intense drunkenness. The chief symptoms in this case were most distressing sickness and headach. He believed a somewhat similar case had occurred to Dr Fergusson. In other cases the chloroform had not relieved the pains of labour.

Mr Wade has employed chloroform pretty extensively in private and dispensary practice in all kinds of persons, and various kinds of operations; he had never seen any "immoral" effects produced, and thought this a poor argument against chloroform.

Dr Snow said that there was no uncertainty in the action of chloroform; the quantity required could always be ascertained exactly by observing the effects; and it was attended with no risk whatever in the hands of medical men who had paid attention to its effects and mode of administration. He thought that Mr Gream had not given due consideration to all the circumstances of the case to which he had alluded, but had hastily attributed injurious and fatal effects to chloroform, which were quite unconnected with its use. He considered that there was no foundation for the imputation that the use of this vapour in labour could be attended with immoral effects. "*In vino veritas*" was as true of chloroform as of alcohol, and therefore respectable patients would never make use of immoral expressions. It was true that in a state of partial insensibility, there might be dreams of various kinds, as in natural sleep; but if the patient were not injudiciously questioned or spoken to, she would nearly always remain silent. He had never seen any effects which caused the least unpleasantness to the relatives or friends of a patient; and if it should happen in some rare instances that expressions should escape which the patient would not utter when exercising her usual reserve, they could cause no uneasiness to the judicious attendants of the lying-in room, who would feel only rejoiced that severe pain was prevented; nor to the patient, who would never be told of them. Sickness and headach occasionally followed the use of chloroform in surgical operations, and lasted a few hours, but he had not seen these effects after its use in midwifery, where a much less deep insensibility was required; and in two instances in which sickness attended labour, it was removed by the chloroform; if the distressing illness in Dr Reid's case were really due to the vapour, it must be a very unusual result. The discussion which had taken place impressed the opinion strongly on his mind, that if the body of the profession had had the same opportunities of witnessing the effects of chloroform in obstetric practice, when administered by a person accustomed to its use, that the operating theatre afforded with respect to its use in surgery, medical opinion would long since have been settled in favour of the practice of anæsthesia in all labours in which the pain is either very severe or protracted; for in such cases he found the benefits of it not less than in surgical operations.

SURGERY IN CHINA.—(*From a Correspondent.*)

WE have received the fourteenth report of the missionary hospital at Canton, superintended by Dr Parker, containing an account of the practice there from the beginning of July 1845, till the end of December 1847. In the course of these two years and a half, 8247 patients were prescribed for, of whom a considerable number were treated in the house. The hospital seems still to be resorted to chiefly by persons affected with diseases of the eye, of which 5767 cases are reported. The following is a list of the diseases occurring most frequently, with the number of cases of each, as given in the report:—Entropion, 762; lippitudo, 300; acute ophthalmia, 365; chronic ophthalmia, 1633; pterygium, 456; nebula, 941; cataract, 329; amaurosis, 155; staphyloma, 120. Probably the large proportion of cases of diseases of the eye presenting themselves at this institution is owing, in a great measure, to its having been at first established as an ophthalmic hospital, and to the deserved celebrity as an oculist which Dr Parker has acquired amongst the Chinese; but there can be no doubt that diseases of the eye are peculiarly frequent and severe amongst the inhabitants of the south of China, and that the native surgeons have not attained to much success in their treatment.

Of his cases of other diseases, Dr Parker reports the following as amongst the most frequent:—Deafness, 78 cases; otorrhœa, 14; salivary fistula, 14;

dyspepsia, 35; fistula in ano, 20; chronic bronchitis, 53; hemoptysis, 12; icterus, 26; ascites, 210; anasarca, 33; worms, 39; enlargement of spleen, 46; urinary calculus, 31; hydrocele, 32; headache, 18; opium-mania, 14; rheumatism, 146; abscesses, 82; ulcers, 146; scrofula, 105; hip-joint disease, 12; curvature of spine, 12; nasal polypus, 19; tumours, 156; scirrhus of breast, 19; harelip, 22.

In considering the whole list as contained in the report, we remark with interest the proofs which it supplies, both of the great difference existing between the climate of China and that of the west of Europe, and of the dissimilarity in the habits of the people. Thus, how remarkably small is the proportion of 4 cases of pulmonary consumption, 1 case of laryngitis, or even 53 cases of chronic bronchitis, to the 2380 cases of all other kinds of disease, exclusive of those of the eye! How strange to us to find only 12 cases of paralysis, and 5 cases of hydrocephalus, occurring in the same list! And in so hot a country as South China, we wonder to learn that only 10 cases of hepatitis and 2 of enlargement of the liver presented themselves in so extensive a sphere of practice.

Dr Parker reports in detail three interesting cases in which he performed lithotomy. One of the patients had laboured for eighteen years under the stone, which, when extracted, measured seven inches in circumference, and weighed two ounces and one drachm. "Very slight inflammation of the bladder followed the operation; with this exception there was not an unfavourable symptom, though, from the extent of the incision, the wound was longer in healing than had been the case in some previous instances. Nevertheless, in five weeks, the patient was discharged perfectly well." In the second case the stone was sacculated, and was also large (six inches in circumference); it was removed with great difficulty, but the patient did well, notwithstanding an attack of cystitis. The third case proved fatal. The patient was forty-one years of age, a butcher, had suffered long, and was much emaciated. "A fatal termination, and that speedily, seemed inevitable, unless, perchance, prevented by an operation. The state of the man's case was distinctly explained to him, and the bare possibility of the success of an operation. He, as well as his family, chose it as a *dernier resort*." The stone, although nearly as large as in the other two cases, was extracted easily. Much venous blood was lost during the operation. The patient sank in the course of the fourth day afterwards.

A very painful case is given of gangrene of both feet, from a cause peculiarly Chinese,—namely, the pressure exerted by tight bandages, applied, according to the barbarous fashion long prevalent in China, for the purpose of compressing the female foot into small dimensions. The patient was a little girl, seven years of age. The bandages had been applied in the ordinary manner, and occasioned much suffering, which, in the course of a fortnight, became so insupportable, that the parents were compelled to remove the bandages; the toes were then seen to be discoloured. A fortnight later the child was brought to the hospital, when it was found that both feet were in a state of gangrene; they were perfectly black, shrivelled, and dry. The line of demarcation formed at the ankles. The left foot came away in a few days; and subsequently the right also, leaving good stumps.

In the autumn of 1847, Dr Parker made several trials of the inhalation of ether as an anæsthetic, and found that the Chinese are as susceptible of its effects as Europeans. One patient was so much pleased with it, that, on the day following that on which he had been subjected to it during an operation, he requested a repetition of it, and represented the operation of the preceding day as a pleasurable dream.

It is very satisfactory to contemplate the beneficent operations of such institutions as the missionary hospital of Canton; for it is most obvious that they are fitted, not merely (although chiefly) to confer incalculably precious bene-

fits on the people who enter them as patients, but also to supply us, as well as other civilized and Christian nations who engage in such labours, with many important facts; and, at the same time, with boundless fields for the exercise of whatever talents and acquirements, even of the highest grade, may be devoted to them.

The Canton hospital, under Dr Parker, owes its establishment and support to American benevolence. The example set by it has been followed by similar, although smaller, institutions, at Shanghai, Ningpo, and Hong Kong; in some of which there are British surgeons, who lend their aid, and devote whatever influence they may acquire through the successful exertions of their professional skill, to spreading amongst the people sound principles of religion and morality.

CASE OF THE NAVAL ASSISTANT-SURGEONS.

THE following petition has been placed in our hands, in reference to a subject which is understood to have been frequently under the consideration of the Admiralty, and has recently attracted a large share of the attention of the periodical press, as well as of every one interested in the medical department of the navy. We are satisfied that it expresses the view of the entire medical profession on this subject, and that, unless a speedy remedy be found, the position of naval assistant-surgeons will cease to be sought by medical men of education, and the navy will no longer be enabled to procure medical officers of even ordinary qualifications. That this has been the case with licentiates of the Royal College of Edinburgh is evident, from one of the paragraphs of the following petition; and we understand it to be the universal sentiment of the Fellows of the College, that the utter failure of their attempt to procure a competition of candidates has rendered altogether nugatory the privilege with which they were recently honoured by the Lords Commissioners of the Admiralty, on the recommendation of Sir William Burnett and of the Navy Board, of presenting, once in three years, one of their most distinguished licentiates as a candidate for a navy appointment:—

"*To the Honourable the Commons of the United Kingdom of Great Britain and Ireland, the Petition of the Royal College of Surgeons of Edinburgh,*

"Humbly sheweth,—

"That the assistant-surgeons of the Royal Navy, many of whom are licentiates of this Royal College, are subjected to certain grievances, to which, in the opinion of your petitioners, a remedy ought to be applied by the Legislature.

"That, early in the present century, an Order in Council was made, which your petitioners understand has not been carried into effect, and in virtue of which the medical officers of the navy were placed on the same footing in all respects with officers of corresponding rank in the army.

"That the naval assistant-surgeons are the only officers of adult age who are excluded from the ward-room or general officers' mess; that they are consequently compelled to mess and associate with midshipmen and naval cadets, gentlemen much their juniors, engaged in the acquisition of an elementary nautical education; that their position as gun-room or junior officers makes them ineligible to possess cabins, the only places of *bona fide* retirement for purposes of study; and that these circumstances are the sources of much vexation, discomfort, and sense of unmerited degradation to this most important class of her Majesty's servants.

"But the College are far from wishing to found the present petition solely or even principally on the attention which is due to the feelings of the assistant-surgeons, inasmuch as its importance will appear far greater when considered in relation to their efficiency, and, consequently, in relation to the interests of the whole naval service. The assistant-surgeons usually enter the

service at twenty-two or twenty-three years of age, and look forward to the higher responsibility of the office of surgeons at the end of seven or eight years. Their capability, not merely in their present rank, but in the higher rank to which they aspire, will depend in a great degree on the devotion of a large portion of their time to purposes of self-improvement. They may at any moment be called upon to treat the most difficult cases, to combat the most formidable epidemics, to dress the most serious wounds, or to perform the most unusual operations, without those advantages which the medical gentlemen of the army for the most part possess of unlimited access to books, and of obtaining the advice and assistance, in all difficulties and emergencies, of the most distinguished medical men, military and civil. Thus, placed in situations of unwonted responsibility, and at the same time left to their own resources in a much greater degree than any other class of professional men, they are by the present arrangements deprived of the society of the educated part of the ship's company, forced to associate with those whose inferior age and education render them unsuitable and unimproving companions, and deprived of the necessary facilities for uninterrupted study. In these circumstances, your petitioners do not hesitate to convey to your honourable House their fixed conviction, that not only is it impossible for these gentlemen to keep pace with the rapid march of the medical sciences, but it is not even possible for them to retain unimpaired the varied knowledge which they had acquired in the schools; and that no system of checks, or of re-examinations, on passing to the higher grade, will compensate to them for that self-culture of which, during a long term of years, they are thus deprived.

"The Royal College lately received from the Lords Commissioners of the Admiralty authority to recommend, once in three years, one of their licentiates as a candidate for a navy medical appointment. In 1847, which was the first year after they received this privilege, no candidate presented himself. In 1848, only one such candidate came forward. On both occasions extensive publicity had been given to the intention of the College to recommend. These facts, together with others well known to them, have satisfied your petitioners that a disinclination exists on the part of their licentiates, and especially of those of them who are distinguished by superior talent and education, to enter the navy medical service; and they cannot doubt that their reluctance proceeds in a great measure from the causes above assigned.

"May it therefore please your honourable House to take the premises into your early consideration, and to remedy the evils set forth in this petition, by granting to the assistant-surgeons of the Royal Navy the rank and the privileges of ward-room officers."

AMATEUR INTERFERENCE WITH MEDICAL PRACTICE.

THERE is a folly of which, strange to say, not a few otherwise sensible people are too often guilty—we allude to clandestine interference with a disease already committed to the charge of a medical practitioner. This folly is at times chargeable on the sick man himself; at times on his officious neighbours and visiters; at other times on both. The wonder is whence the hardihood springs for such dangerous meddling; and nobody wonders more than the medical practitioner, who with difficulty figures to himself an amateur passion seeking to revel in the anxieties of a responsibility involving the life of a fellow-creature—a responsibility, the sense of which, as the dangerous crisis of a disease approaches, so often deprives him of his rest. And his wonder does not subside when it occurs to him, that his amateur rival is denied some sources of relief to which, amidst such anxieties, he can resort with, at any rate, a temporary satisfaction. The conscientious well-educated medical practitioner feels, on such occasions, that he knows his duty—that he has not been wanting in the performance of it—that the issue is in the hands of Providence. His

doubts are not the doubts of ignorance or of repentant rashness; he has studied the disease; he has recalled his former experience in it; what he has done he has done deliberately and with the requisite forethought; why then should he be painfully anxious as to the result? It is the solemnity of the moment, while a human being continues in the uncertain balance between life and death, that suggests apprehensions however groundless; he fears something may have escaped him; that there may be some peculiarity in the instance of disease before him which may exempt it from ordinary rules. Surely, when not carried to timidity, this is a wholesome state of mind for one whose acts must, like those of a medical practitioner, be regulated by ever-varying shades of circumstances—a jealousy ceaselessly watchful lest some little perceptible occurrence should arise to vitiate the soundness of his judgment in the application of the rules of medicine. Common sense must here approve. And yet how different must be the mental cast of many well-meaning people who act daily on a very opposite principle? They think the practice of medicine comes as naturally to a man as driving a gig or tilling a farm, occupations held, though not always with impunity, to need no apprenticeship. Our Quixotic friend falls in to-day perhaps with an acquaintance suffering from colic, prescribes a glass of brandy, and receives thanks for the relief it affords; but to-morrow his ill-luck may guide him to another acquaintance who has been just freely blooded for a dangerous inflammation of the bowels. We have an actual case in view—there are the same pains—our amateur discerns no difference—he insists on the relief his friend of yesterday obtained from a bumper of brandy. The victim yields; and the medical man doomed to disappointment, on his return in the evening full of hope, finds that hope blasted, and a life irrevocably sacrificed to the silly vanity of beating the doctor in his own craft.

We would earnestly recommend to all who sincerely wish to keep their consciences free from the burden of having sent a fellow-mortal prematurely into eternity, to leave the treatment of diseases in the hands of those whom a public authority has pronounced to be qualified; and in proof of the risk they run by acting otherwise, we have the following very distressing case to recount. Though the names and other particulars, for obvious reasons, are suppressed, we believe we may pledge ourselves for the general accuracy of our statement.

Not many weeks ago, in a thriving Scotch town, a man, previously in strong health, between thirty and forty years of age, in a humble occupation, was attacked with acute rheumatism, and fell under the care of a well-educated medical practitioner. Several joints were affected,—there was very considerable attendant fever, and latterly great frequency of the pulse. When he had been ill between two and three weeks, a person in a respectable line of life, altogether unconnected with the medical profession, but an amateur hydropathist, went to see him without the knowledge of the medical attendant. He returned repeatedly, always at times between the medical visits, contenting himself at first with bathing the affected joints with cold water, which was done on several successive days, notwithstanding that the patient said he sometimes shivered in bed. At length, when the case had lasted fully three weeks with no abatement, and when, owing to the frequency of the pulse and other unfavourable symptoms, the medical man had begun to regard the disease as assuming a very serious character, our amateur resolved to employ the cold wet sheet. Accordingly, avoiding interruption from the doctor, he one evening wrapt the patient in a sheet wrung out of cold water, and having packed him in blankets, left him to the care of his wife, being unable to remain owing to some engagement of his business. When the patient had been in the cold sheet for some time, probably something more than an hour, the doctor called, and discovering, to his surprise, what was going on, told the man's wife that he thought it would cause his death. The sheet was kept on, however, some time longer, most likely altogether about two hours, and soon after it was removed, the doctor was sent for, owing to the state into which the man had fallen, and on his arrival he considered him to be dying. Brandy

and water was administered, but with no effect, and life was extinct in four or at most five hours after the cold sheet had been applied. The treatment in use at the time he was subjected to this heroic remedy was calomel and opium.

A careful inquiry was made into the whole circumstances of the case by the authorities—the amateur was apprehended, examined, committed to prison till liberated in due course of law, and afterwards admitted to bail. He was thereafter kept, while the case was under further investigation, for some weeks in a state of uncertainty as to whether he was to be brought to trial. Ultimately, it would appear, the intention to try him has for the present been relinquished; probably in consequence of some hesitation in applying to his case, where there was no pecuniary motive, the law which is understood to be clearly applicable to ordinary quack practitioners.

It is, however, obvious that, in other respects, the case was one peculiarly adapted for selection, as an example to officious persons of the risk they run in interfering with the medical treatment of diseases. The treatment was adopted without any pretension to the knowledge of diseases, the amateur believing all the while that he was treating a fever. Death so speedily followed the use of the cold wet sheet, as to leave no room for doubt as to the cause of that result. To apply a cold wet sheet to a person exhausted by the long continuance of febrile action is, we believe, hardly more consistent with the precepts of intelligent hydropathy than with those of regular medicine. Acute rheumatism is almost the only disease in which it could have been pronounced with certainty, that a single application of the cold wet sheet had been the cause of death, this disease being almost singular as one in which extensive inflammation, with great febrile excitement and extreme exhaustion, may last for many weeks without any tendency to a fatal event. The uniform answer in the witness-box by intelligent medical men, would have been that acute rheumatism does not prove fatal unless inflammation of internal organs arise in its progress. The post-mortem examination proved that no such internal disease had arisen; the heart was free from disease; so was the brain and the other internal organs, with the exception of a considerable congestion in the vessels of the brain, spinal cord, lungs, liver, and kidneys, such as might be anticipated where death took place under such circumstances. It is proper to add, that the body was exhumed for the purpose of being inspected twelve days after burial. The narrow escape of the amateur (if he has escaped) from prosecution, ought to be a warning to such persons, that they may have to answer at the bar of a criminal court, as well as at the bar of conscience, for such unauthorized and uncalled for interference in matters of which they have no proper knowledge, and in regard to which they have no right to interfere.

PROGRESS OF CHOLERA.

We have received a large number of most interesting communications from different districts of the country, with regard to the origin and communication of cholera in the various localities in which it has arisen. Some of these we here publish; others, which have come too late into our hands, we are obliged reluctantly to defer inserting till next Number. To the authors of these communications we beg to return our best thanks; and at the same time we would take the opportunity of reminding our readers in general of the importance of procuring at this time evidence on the origin and transmission of cholera, especially in country districts, and where the cases are not in overwhelming numbers. Indeed, it may be said, that the smaller the *number* of cases in any locality, the more important is their thorough examination in reference to this subject. We therefore request all of our readers who have opportunities of observation upon this subject, or who are in communication with others having such opportunities, to procure for us early and authenticated statements as to

the arrival of the epidemic in particular localities, and its propagation thereafter. We would respectfully request their attention to the following topics, in regard to which precise statements are especially desirable :—1. The situation of the locality and the condition of the inhabitants. 2. The degree of cleanliness, supply of water, drainage, and ventilation of the locality, and the houses attacked. 3. The prevalence of diarrhœa, fevers, endemic or other epidemic diseases, before the accession or during the continuance of cholera. 4. The personal habits of those attacked. 5. The details of the first cases, and more particularly their communication, known or suspected, with infected districts or individuals, and with one another. 6. The occurrence of attacks in *classes* of persons in particularly close communication with cholera patients, as, for example, medical attendants and nurses ; the *aggregate numbers* of the individuals composing such classes ; and the *number* of those attacked. 7. The comparison of such details with those of similar classes of individuals in the same locality, but *not* engaged with cholera cases. (This and the preceding heads chiefly refer to places where the disease has been severe, and the patients collected in hospitals). 8. The localities of cholera in the present epidemic as compared with that of 1832-3, and with the localities of other epidemic diseases.

We have now so repeatedly stated our conviction, that the questions connected with the origin and transmission of cholera, can only be definitively settled by precise and accurate details of the kind above alluded to, that we need scarcely again urge upon our readers the necessity of accumulating these in a concise form apart from vague and general statements, which can be of no avail except to produce a momentary bias in the mind of the inquirer.

HOLYTOWN, LANARKSHIRE.—(*From Mr David Angus, L.R.C.S., Edinburgh*).—Cholera appeared in Baird's Square, Holytown, on the 18th December 1848. On the 22d a nephew of the first patient was seized, and on the 23d the mother of the nephew. The last two had been freely visiting the first mentioned ; in the course of another day or two several other families in the square were attacked, and among them another nephew of the first patient, and who (the nephew) had not visited any of his relatives when they laboured under the disease.

Cholera appeared at Woodhall Colliery Square on the 2d of January 1849, when three children in one house were seized betwixt the hours of five and six in the morning. It afterwards spread over about a dozen families.

Cholera commenced in Chapelhall on the morning of the 8th of January 1849, when a carter and his wife were affected with the disease about the same hour. Again, in Chapelhall, after five days' absence, a man and a woman living in one room took ill.

At Legbranoch, on the 19th of January, a workman was seized, and five hours afterwards his landlady formed the second case in the district.

At Stevenston Square cholera appeared on the 21st of December—spread slowly—continued a few weeks—left for a fortnight, and re-appeared ; several individuals being attacked on the first night of its return.

On the 1st of January 1849, a blacksmith from Carnbroe, then an infected district, died of cholera at Salsburgh ; and no cases have as yet followed in that village.

In none of the cases above enumerated, except the last, was there any connexion, direct or indirect, with any district where cholera prevailed.

All the districts mentioned are miles separated from each other, with the exception of Stevenston and Baird's Squares, which are a few hundred yards distant.

In all, I have attended about 100 cases, seventy of whom have died.

STIRLING.—(*From Mr Moodie, Surgeon*).—Previous to January cholera had been prevailing in a severe form in Falkirk, Grangemouth, and the neighbouring villages, about twelve miles south-east from Stirling ; on the 8th January

it broke out in Doune, eight miles north-west; on the 11th January a farmer living two or three miles east of Stirling was attacked; he had not been from home for some time, and no circumstance to give rise to the suspicion of contagion could be ascertained on inquiry; he had a severe attack of British cholera about four years ago. A few days after, several cases occurred near Denny, seven miles south; a female had come from Glasgow ill with diarrhœa —she recovered, but three of the inmates of the same house were seized with cholera and died. It will thus be seen that cholera was prevailing in all directions round Stirling before its breaking out in the town.

On the 19th January a case occurred at the Abbey, a small village close to Stirling on the east, but separated from it by the Forth, and not two miles from the farm where the case already mentioned occurred; the patient was a woman of rather irregular habits, whose husband lived in Glasgow. It was ascertained that there had been several cases of cholera in the house where her husband lodged in Glasgow; that he had come to see her and brought some clothes to be washed; and that she had been washing them the day before she was attacked. Several cases occurred in the village immediately after, one of them being a woman who had washed the clothes of the one first attacked; and two children near the river on the Stirling side were also attacked.

On the 22d January a case occurred at the Old Bridge, a part of Stirling situated on the banks of the Forth to the north-east; it is inhabited chiefly by the poor, and suffered severely from cholera in 1832. It is about a mile from the Abbey, and no communication with it or any other infected place could be ascertained; the patient was a woman who had been drinking for a week; two of her family were attacked in a day or two after, and several other cases occurred among her neighbours. In a few days after this it spread gradually over various parts of the town. Nothing satisfactory as to its mode of communication, and contagious or non-contagious nature, can be ascertained after this. It has hitherto been confined almost entirely to the very poorest and the dissipated—the exceptions being few; and here, as in other places, it has chiefly occurred in houses and localities where fever has been known to prevail.

Both before and since the outbreak of cholera, diarrhœa has been very prevalent among all classes.

The farm where the case mentioned occurred, is alluvial land near the Forth; the Abbey and the houses at the Old Bridge are built on similar land, and close to the river. Almost all the other cases, however, have been in the higher parts of the town, but in houses occupied by the very poorest class; the lower parts of the town, inhabited by persons of a better class, have hitherto almost entirely escaped.

HADDINGTON.—(*From Dr Howden*).—For some time previous to the 12th November *diarrhœa had been unusually prevalent*, but there occurred no case of cholera until that date, when a man of worn-out constitution was seized, and died on the 13th. He had come to Haddington upon the 8th November from Leith, where the disease then prevailed. He presented no sign of the disease for three clear days after leaving that place. From him, no one who lived in the house with him, and no one who waited upon him, took the disease.

On the 17th November a child, who had had no connexion or intercourse with the preceding case, became affected, and died on the 18th. None of the members of this family became affected until the 10th December, when his mother was seized, and when the disease was more generally prevalent.

From the 18th to the 30th there was no case,—when a man, in a different quarter of the town, who had had no connexion with any person or place affected, took ill and died. Neither his wife, nor any other who waited upon him, took the disease.

December 2d.—A woman, without communication with any one, took ill, and died on the 3d. She had been engaged, all the evening previous to her at-

tack, in washing in a damp cellar. Her husband continued to sleep on the damp ground floor, and was seized on the 6th December. All his children *slept* on the floor above, but was not otherwise separated from their parents; and none of them took the disease, nor any one who waited upon either father or mother.

On the 14th, in the quarter of the town where the case on 30th November occurred, a mother and daughter died, without the help of medicine; and from them no case could be traced.

From this date to December 29th, twenty-seven cases occurred—all of them single cases in families, with the one exception of a son and mother; and all of them, so far as they knew, or it could be discovered, having had no communication with the sick. Then, after the groups of houses became affected, it was less possible to trace that the cases stood unconnected; and therefore I did not mark individual cases further; but, that I may show to how trifling an extent it spread in individual families, and therefore how futile is the evidence in favour of its contagious character, I may state, that up to this time there have occurred 102 cases in 69 families, and 62 separate houses.

There was no case in the country part of the parish until the 3d January, when a man was seized at the Abbey—a collection of about a dozen and half of cot-houses, about a mile and a half to the east of Haddington, and on the bank of the river. He had been repeatedly in the town. He died on the 7th, and his daughter was seized on the 12th. She, however, had also been in Haddington on the two previous days. Not one of the other dwellers in the place took the disease.

Upon January 8th, a man, who had repeatedly been in Haddington, took ill in one of six or eight houses about three miles to the west of the town. He died, and there was no second case there.

A man residing in Athelstaneford, about three miles to the north-east of Haddington, passed the nights of the 20th and 21st January at Rosehall, about half a mile to the west of the town. He was then well—returned to Athelstaneford—took ill on the 24th, and died on the 25th. Upon the 25th were reported the two first cases of cholera at Rosehall. Up to this date, 17th February, there has been no second case at Athelstaneford, which is a village principally occupied by labouring people.

These are the only cases that have occurred in the rural districts. There has not been a case in the west half of the town, which is drier and more airy than the east portion, and which has not in it any sewer; while the latter has them of very imperfect nature, and has the floor of some of its houses on a lower level than their filthy drain.

There has not been a case in any lodging-house in the town, which are at all times well filled with vagrants. No family, removed from any house in which a patient had died, has carried the disease to the district to which it has been removed.

No nurse, no doctor, no parish-officer has taken the disease. Such are the facts. They at least do not strengthen the contagion theory—they are too small an array very much to weaken it.

The papers of Dr Keith, Dr Simpson, and Dr Jenner, are in the hands of the printer, and will be inserted next month. We are obliged also to postpone the report of the Obstetric Society, and a great many important articles of "Medical News."

Communications respecting cholera have reached us from the following gentlemen:—Drs Craig, Weir, Maclatchy, Logan, Cruickshank, Turnbull, Hamilton (Falkirk), Hamilton (Dalry), Thomson, Blacklock, Ritchie; Messrs Angus and Mitchell.

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*The Attitude and Positions, Natural and Preternatural, of the Fœtus in Utero, acts of the Reflex or Excito-Motory System.* Part II. By J. Y. Simpson, M.D., F.R.S.E., Professor of Midwifery in the University of Edinburgh.—(*Continued from* p. 441.)

Section III.—Reflex or Excito-Motory Muscular Movements the Cause of the Attitude of the Fœtus.

Let us now attempt to point out the means or mechanism by which the special position of the infant in the latter months, with the head downwards, is assumed and maintained through the instrumentality of the fœtal reflex or excito-motory movements. In fulfilling this object, we shall speak first of the position of the fœtus at the full term of pregnancy, and the mode in which it is *maintained* by reflex action; secondly, we shall inquire how and when reflex action enables the fœtus to *assume* this position; and lastly, we shall offer a few observations on the appropriate stimuli calling the reflex motions of the fœtus into action, the parts of its surface most susceptible of the external impressions leading to its reflex motions, and the period of life at which reflex muscular motions are first and principally remarked.

1.—*Mode of Maintenance of the Position of the Fœtus towards the end of Pregnancy.*

At, and (as we have already seen) for some time before parturition, the human fœtus is rolled up into an ovoid-formed mass, with its cephalic extremity placed lowest or over the os uteri. When thus placed, it stands, so to speak, upon its head, when the mother is in the erect posture. To comprehend the mode in which this peculiar

position is maintained during the latter period of utero-gestation, it is necessary to attend to the relative shapes or forms of the uterus and of the fœtus at that period. For it is the relation in shape of the fœtus to the uterus—of the contained to the containing body—that regulates this position. And the maintenance and restoration of the position is effected by reflex movements, when its continuance is from time to time threatened to be interrupted by movements of the mother's body, or by other circumstances affecting the conditions and relations of either the uterus or infant.

The form of the uterus at the full term of utero-gestation is ovoid. See the outline of it from Hunter's most accurate drawing, in Plate II., Fig. 1. "The general figure of the uterus at this time is," says Dr Hunter,[1] "oviform; the fundus answering to the largest extremity of the egg, and the cervix and os uteri to the small end; but the fundus is larger and more flat, or less pointed, in proportion to the lower extremity of the uterus, than one end of an egg is to the other; and the whole uterus seems more or less compressed, so as to be broader from right to left than it is from the fore part backwards."

In the pregnant uterus represented in Plate I., and belonging to Professor Goodsir, the whole length of the cavity of this organ is twelve inches and a half. The broadest part of the cavity is four and a half inches from the fundus, where it measures eight inches across. From this point the organ gradually diminishes in breadth, and tapers downwards towards the cervix. Across the cervix, about three inches above the os, it is about four inches in breadth.

The form of the fœtus as it lies rolled up in utero at the full term of pregnancy is ovoid, like that of the uterus itself. "The fœtus," says Velpeau,[2] "forms an ovoid mass, whose large end, turned towards the fundus of the uterus, is represented by the pelvic extremity of the trunk, and the summit (small end of the ovoid) is represented by the cephalic extremity, which rests upon the cervix of the uterus." In Mr Goodsir's preparation (see Plate I.), the broadest part of the larger or pelvic end of the ovoid mass of the fœtus measured nearly eight inches, and ran in a line from the lumbar region of the child across nearly to the point where the sole of the foot was applied to the placental surface. The breadth of the smaller or cephalic end of the ovoid (formed by the occipito-frontal diameter of the head) measured about four inches. In other words, the lower end was nearly a half narrower than the upper and broader end of the fœtal ovoid.

The figures 1 and 2, given in Plate II., represent in outline the ovoid form of the uterus, and the ovoid form of the fœtus at the

[1] Anatomical Description of the Human Gravid Uterus, p. 3.
[2] Traité des Accouchemens, tom. i. p. 333.

full term of pregnancy, according to the preceding description of the relative shape and configuration at that time.

Towards the end of pregnancy the fœtus is situated in the uterus, so that the two ovoids which the fœtus and uterus form are relatively adapted to each other; that is, the broad or pelvic end of the ovoid of the fœtus, is placed towards the broad or upper end of the ovoid of the uterus; while the narrow or cephalic end of the ovoid of the fœtus, is placed towards the narrow or lower end of the ovoid of the uterus. The narrow and broad ends of the ovoid mass of the fœtus, are relatively adapted to the narrow and broad ends of the ovoid cavity of the uterus. The figure of the contained body (the fœtus) thus comes to correspond with the figure of the containing body (the cavity of the uterus), and is, as it were, fitted into it. Fig. 3 in Plate II. represents the one ovoid (Fig. 1) thus adapted to the other ovoid (Fig. 2).

At the advanced time of pregnancy of which we are speaking, when the uterus and fœtus are normal in shape and size, the fœtus cannot be placed transversely in the uterine cavity, so as to form a transverse or arm presentation, as there is not room for it to lie in that position. For the long axis of the ovoid mass of the fœtus is about twelve inches; the greatest breadth of the uterus is only eight inches; so that the latter could not receive the former. For the same reason, when once the position with the head downwards is assumed, it comes to be difficult or impossible for the child near the full time to move round in utero, so as to turn its pelvic, instead of its cephalic extremity downwards, and towards the os. In fact a compressible body of twelve inches in length, such as the rolled up fœtus is, cannot readily move through a space of only eight or nine inches, even though that space, like the transverse axis of the uterine cavity, be capable of dilatation and forcible alteration of shape.

At, and towards the full term of utero-gestation, the position of the fœtus with the head lowest, is thus greatly maintained by the relative *physical* adaptation of the ovoid shape of the rolled up mass of the fœtus, to the ovoid shape of the interior of the cavity of the uterus. But this particular adaptation and position of the fœtus in the uterine cavity, would be often lost if no other additional and *vital* means were in operation; as we see indeed often happen when the child dies. The other additional vital means, by whose influence this special position is still further, rigorously and carefully sustained, consists of the restoring influence of reflex motions on the part of the fœtus itself. For, let the relative adaptation and position of the fœtus be partially disturbed (as readily happens by any movements of the mother, as, for example, by her suddenly assuming the upright or supine postures, or other such causes), and immediately the fœtus, by a series of reflex movements of its extremities, particularly of its lower extremities, acts till it regains its former perfect position. It moves, in other words, till it has retaken that position in which it is most free

from any marked excitations or compressions upon the external surface of its own body.

It is perhaps unnecessary to add, that seeing the fœtus is placed in the liquor amnii, a fluid medium of high specific gravity, and is consequently subjected to an uniform amount on all sides of hydrostatic pressure (considerably neutralizing the force of gravity upon its mass), its existing position is easily altered by changes of position and motions on the part of the mother, and consequently on the part of the containing body—the uterus. But for exactly the same physical reason by which the position of the fœtus is thus rendered easily capable of change and alteration, the physical powers and means necessary for the restoration of it to its proper position are correspondingly simple. Floating as it does in a liquid medium, the motions of its extremities in that medium, and against the uterine walls, are easily and perfectly adequate, as a mechanical means of motion, to enable it to swim back to, and resume that normal and adaptive position in which it lies most free from extraneous excitations, and from the irritation of disturbing impressions or stimuli upon its own surface. In some mothers the position is apparently easily maintained; the infant and uterus are perfectly adapted to support each other; and consequently the child moves little. In other mothers, or in the same mother in other pregnancies, the movements are nearly incessant. In those cases in which the movements of the infant are great and continuous, the umbilical cord is generally found long, and hence liable to compression; or the cavity of the uterus is large and distended from an excess of liquor amnii, and almost constant movements are required on the part of the fœtus to prevent its almost constant tendency to displacement or compression. The nearly uninterrupted constancy of the fœtal movements in some mothers, exists to a degree greater than is observed in any voluntary muscular movements. But the excess of fœtal movements corresponds in this respect with the law of constancy, which regulates other corresponding excito-motory movements in the animal economy. Voluntary muscular movements always become temporarily arrested and interrupted from time to time; because, from time to time, volition, like other cerebral acts, must temporarily cease for the purpose of rest and renovation. Fatigue, and its antidotes repose and sleep, necessarily belong to the cerebral system; but the spinal system never sleeps, and the reflex or excito-motory muscular movements over which it presides, are not susceptible of fatigue, and do not necessarily require any intermission in their action. It is on this principle that respiration in man is unattended by fatigue, and that "the long flight of birds in their migrations, and of flies, which are all the day long on the wing, is sustained."[1] Some of the reflex or

[1] Diseases of the Nervous System, p. 26; "I suspect, indeed, that the migratory traveller is frequently actually visited by nature's sweet rest, even during its ærial transit." New Memoir, p. 32. The young infant often evidently con-

Plate II.

Fig. 1.

Ovoid form of Uterus.

Fig. 3.

Adaptation of Fœtus to Uterus.

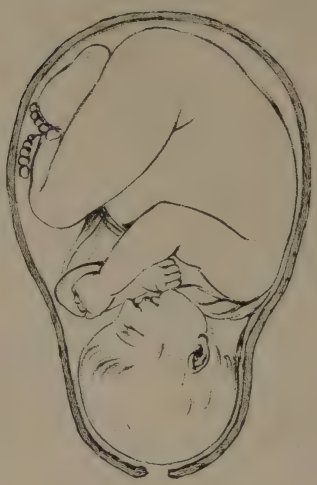

Fig. 2.

Ovoid form of Fœtus. at full time.

Fig. 4.

Fœtus and Uterus at fifth month

Fr. Schenck lithog. Edin.r

excito-motory movements of animals which show this law of constancy, have the same object and effect as the reflex movements of the fœtus; viz., the maintenance of position. Thus, the fish holds and maintains its position in the stream by constant reflex movements of the fins and tail.

2.—*Mode of Assumption of the Natural Position of the Fœtus with the Head downwards.*

I have already stated (p. 428) that the position of the fœtus, with the head over the os uteri, is not observed till the latter months of utero-gestation; that in fœtuses expelled before the end of the sixth month, presentations of the feet or pelvis are as frequent as presentations of the head, and presentations of the shoulder by no means infrequent; and that the position of the head lowest, and towards the os uteri, comes to be taken with more and more certainty from the end of the sixth month, onward to the full term.

From the observations collected by Professor Dubois at the Maternity Hospital of Paris, I have constructed the following table in evidence of the preceding remarks. The respective results of the presentations among children born before the end of the sixth month, during the course of the seventh month, and at the full time, include, it will be observed, all cases whether the child were alive or dead at the time of its expulsion The return for the eighth and ninth months, refers only to children born dead. He does not give any return relative to the children born alive during these two months.

Table of the Relative Proportion of Head Presentations, as varied by the date of the Pregnancy or Labour.

Period of Pregnancy.	Total Cases.	Presentations of			Per centage of Head Presentations.
		Shoulder.	Breech.	Head.	
Before end of sixth month,	121	5	51	65	52 in 100
During seventh month,	119	6	31	82	68 in 100
During eighth and ninth months, ...	96	2	22	72	76 in 100
At full term of gestation,	100	1	3	96	96 in 100

The facts in this table sufficiently prove that the position of the fœtus with the head lowest and over the os uteri, does not begin to be assumed till about the end of the sixth month, and that it is taken up with increasing frequency and certainty from that period onward to the full term of pregnancy.

The double fact of the position not being assumed before the period in question, and of its being more and more positively and

tinues the act of sucking (an act of the spinal or reflex system) while the cerebral system is asleep.

steadily assumed after this period, is simply, I believe, ascribable to the circumstance, that before the sixth month there is no adjusting correspondence of figure between the uterus and fœtus; while, after the sixth month, the figure of the one comes to correspond more and more to the figure of the other, and consequently requires more and more the containing body to be placed in the containing cavity in that kind of relative adaptation which has been already described.

All anatomical authorities allow that the uterine cavity before the sixth month[1] is spheroidal, and not ovoid, the neck of the organ not yet being developed and taken up to form part of the cavity. The fœtus, up to this time, is not folded up so compactly as afterwards, or moulded into its ultimate ovoid form. Further, at the same period the uterine cavity is not only roundish in form, but is relatively much larger in regard to the fœtus—for it contains a large quantity of liquor amnii.[2] After the sixth month these various circumstances are gradually altered. The cavity of the uterus becomes more and more ovoid in form, from the development of the cervix; the quantity of liquor amnii becomes less and less, relatively to the increasing size of the uterus and infant; the infant itself becomes in a greater and greater corresponding degree deprived of space—is forced to alter its shape and position according to the shape and form of the uterine cavity—becomes more folded and compacted together, because its containing space is less—and ultimately becomes comparatively fixed in the position which it is thus forced to assume. "In the last two or three months of gestation, the child," observes Dr Hunter,[3] "is commonly so much straitened for room, and so compactly adapted to the oblong figure of the uterus, that it cannot change its general position either by its own efforts, or even by accidents happening to the mother." "When," he elsewhere remarks,[4] "there is a considerable quantity of liquor amnii, the child takes the advantage of room, and the composition of its parts is not so close or globular. In proportion as there is less room, its figure is more compacted and moulded to the shape of the cavity of the uterus."

These changes, however, in the physical shape and size of the

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1 See Plate II. Fig. 4, sketched and drawn from a case of five or six months pregnancy, delineated by Breschet and Velpeau.

2 "The relative proportions," says Ramsbotham, "between the quantity of fluid and size of the embryo, differ much at different stages of pregnancy, being considerably greater at the early periods, and less at the advanced stage. Thus when the embryo is scarcely visible to the naked eye, there is from half a drachm to a drachm of water collected within the membranes. Where the embryo is not so large as a small kidney bean, there would be an ounce or more of liquor amnii; while at the end of gestation, when the fœtus weighs, on an average, nearly seven pounds, the amount of fluid seldom exceeds a pint. The quantity, therefore, though positively increasing with the growth of the ovum throughout the whole of gestation, is relatively to the size of the fœtus gradually diminishing."—*Obstetric Medicine and Surgery*, p. 69.

3 Anatomical Description of the Gravid Uterus, p. 67.

4 Ib. p. 63.

uterine cavity, relatively to the physical form of the fœtus, would not produce of themselves the position with the head over the os uteri. The dead child does not take this position. If the child has been alive up to the seventh, or eighth, or ninth months, and has in consequence assumed the position in question, but then dies—the position is liable to be lost from the death of the child. At pp. 432 and 433, I have given two tables proving this fact, and showing how much the assumption, as well as the maintenance of the position, with the head downwards, is a vital act, and connected with the existence of life in the child. The regulating vital power guiding it to the assumption of that normal position in which its figure corresponds as exactly as possible to the figure of the uterine cavity, consists of a succession of reflex or excito-motory movements of an adaptive kind on the part of the fœtus, excited by impressions made on its external surface. In order fully to understand their action, we shall next briefly consider the origin and effects of those adaptive reflex movements, which thus contribute not only to the original assumption, but to the continued maintenance of the position of the fœtus,—and force it to take up, retain, and restore itself to that special position in the uterine cavity in which it can lie with its form more perfectly adjusted than in any other position to the form of the uterus, and with its own excitor surfaces more removed than in any other position from the action of external impressions and stimuli.

3.—*Nature of the Physical Impressions, or Stimuli, exciting Reflex Movements on the part of the Fœtus.*

"All animals," says Harvey, "while they are at rest or asleep, fold up their limbs in such a way as to form an oval or globular figure. . . . . So, too, the infant in utero is generally disposed after this manner. In such a position is the embryo usually found as that which we naturally take in sleep."[1] This folded up state of the limbs and body of the fœtus is primarily the effect of the well known greater muscular tone and contraction of the flexor than of the extensor muscles. Of the power and preponderance of the flexor over the extensor set of muscles, we have evidence remaining for some time after birth, in the almost constant state of flexion which may be observed in the extremities, and particularly in the lower extremities of the new-born child, when unirritated and at rest. It is not in general for some considerable time after birth that the child acquires the full power of extending the lower extremities. And, no doubt, this superior tone and contraction of the flexor muscles is one of the means by which the necessary ovoid form of the fœtus in utero is maintained. But while this phy-

[1] Harvey's Works (Sydenham Society Edition), p. 522.

siological principle [1] accounts for that form and figure of the fœtus which best adapts it for the form and figure of the uterus, we must have recourse to the acts of the reflex system in another view, in order to account for the adaptive position, or rather adaptive movements of the fœtus. These movements are reflex. And it is laid down as an established physiological law, that "the reflex nerves are in every case, without an exception, excited to action by the impressions of *physical* agents acting on the peripheral extremities of incident nerves;" [2] and "that the reflex power is never exercised without the excitement caused by the application of a physical agent either to the external or internal *surfaces* of the body." [3]

In relation to the movements of the fœtus in utero, let us next, then, inquire what physical agents produce the required excitor impressions, and on what surface, or the peripheral extremities of what incident nerves, these impressions act?

Various physiologists have pointed out that, when in the lower animals the head is removed, or the spinal cord is divided, and sensation thus abolished, the mere touching or irritation of the *skin* is followed by more or less complex movements in the *muscles* of the extremities and trunk. Whytt, for example, long ago "remarked that, after decapitation, frogs and serpents performed movements of the limbs or entire body, consequent upon the application of a stimulus to the skin." [4] "In the frog," says Mr Grainger,[5] "in the rabbit, kitten, &c., after all feeling and volition is lost, the *limbs* are moved when the *skin* is touched." The movements thus excited under these conditions (decapitation and division of the cord) are evidently reflex or excito-motory, and the facts prove how powerful

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[1] Dr Hall, it is well known, holds that the reflex function, in its natural state, constitutes the principle of equilibrium and tone of the whole muscular system.—*Memoirs on the Nervous System*, 1837, p. 38.

[2] Grainger's Observations on the Structure and Functions of the Spinal Cord, p. 50.

[3] Ibid, pp. 119, 129. [4] Ibid, p. 3.

[5] Ibid, p. 49.—"Every physiologist," says Mr Grainger, "who has divided the spinal cord in living animals, and has then pricked or otherwise irritated the *skin*, has remarked the free motion which is thereby caused in the muscles which are called voluntary,"—p. 100. When the spinal cord is divided, "the foot of the rabbit is not only drawn away from the irritation, but the whole limb, occasionally even both legs, are thrown backwards, with all the complicated motions that occur when the creature runs. In the frog, if the cord be divided sufficiently high, both hind legs often strike out when one is touched, not in the manner of the rabbit, but with the very action that occurs in swimming. The chicken, in the experiment of Flourens, when deprived of all volition, flew when thrown into the air. Lastly, in the human body, the legs may be incited to motion, when all voluntary power is destroyed, by tickling the sole of the foot. It is thus proved, beyond the possibility of a doubt, that motions similar to those performed in the progression of the animal may be excited by touching *the skin*, when volition and sensation are destroyed,"—p. 150.

an excitant surface *the skin* is, and how easily irritations of it act in calling up or producing reflex muscular movements. "No part," says Müller, "equals the skin in the property of exciting reflex motions; the slightest touch applied to the surface, in animals in a state of narcotization, is frequently sufficient to give rise to strong spasms, while the reflex actions excited by irritation of the nerves themselves are much slighter." [1]

I have already stated (page 440) that movements of the limbs of the child are readily produced *after* the period of its birth, by irritation of the sole of the foot and other parts of the cutaneous surface; that *during* birth, and while labour is going on, similar movements can be produced by similarly irritating the presenting part of the child; that *before* birth, or, in other words, during pregnancy, the same kind of movements is excited by irritating the limbs or body of the fœtus through the abdominal or uterine walls by the hand, stethescope, &c.; and that in all these cases the movements thus produced are reflex or excito-motory, and not voluntary, inasmuch as they occur in anencephalous fœtuses entirely wanting a brain, as distinctly as in those that are normal in structure.[2]

Cutaneous impressions, or physical irritations applied to the peripheral extremities of the incident nerves distributed to the surface of the skin, are thus capable of producing reflex-motory actions on the part of the infant, and are, perhaps, the most common cause of excitement of the muscular movements of its extremities, and consequently of its changes of position. If the fœtus be temporarily thrown out of position by any movements on the part of the mother, and if the surface of its body thus become irritated at any point or points by mere pressure from or against the opposing uterine parietes, or other resistant substances, this pressure (like the pressure of the hand or stethoscope, alluded to in the preceding paragraph) will act as an excitor stimulus, calling up reflex motions calculated and empowered to move away the irritated and compressed limb or body from the agent or source of the irritation. Different postures of the mother, when suddenly assumed, by both displacing the fœtus and altering the shape of the uterus so as to make its parietes press upon the fœtus, are followed for a time by a succession of movements on the part of the infant.[3] Thus the acts of suddenly rising or

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[1] Müller's Physiology (Baly's Translation), vol. i., p. 322; see also Longet, Anatomie et Physiologie du Systéme Nerveux, vol. i., p. 318.

[2] Unzer, whose remarkable works contain such clear and comprehensive views, in many points, of the physiology of the nervous system, makes a remark upon this subject which is too striking to omit in evidence. "A living anencephalous monster, when born, draws itself up, if you puck or burn its limbs, and that plainly through a reflex operation of the nerves—[durch eine blosse nervenwirkungzurück]—as a natural new-born child would do, if you made the same external impressions upon it."—*Erste Gründe einer Physiologie*, p. 552.

[3] "That the different attitudes of the mother's body should produce altera-

suddenly lying down excite almost constantly, in certain women, motions on the part of the child. When some mothers stoop or lean forward, they alter the shape of the uterus, and compress the child so greatly as to call up instantly such strong fœtal motions as to oblige them immediately to assume the erect posture. And the fœtus sometimes throws itself into positions which excite new movements on its own part. In the latter months, "it is frequently evident," says Dr Hunter,[1] "that some awkward straightened position of the child being produced by its own motion, and pressing or stretching the uterus unequally, gives the mother much uneasiness for a time; and then, upon the child's stirring in some remarkable way, it gets to be more at its ease, and the mother feels instant relief from the pain, the stretching, or whatever the complaint was. This will happen to the same woman again and again."

In the various cases and conditions which I have mentioned,[2] the *cutaneous* impressions and stimulations call up reflex *motions* in the fœtus according to the laws laid down by all those physiologists who have written upon the subject of excito-motory phenomena. "An impression is made," to use the language of Dr Marshall Hall, "upon the extremity of a nerve (or nerves); this impression is conveyed, not to the cerebrum, but to some part of the medulla oblongata or medulla spinalis, whence it is reflected upon certain muscles destined to be excited into consentaneous action."[3] "The external impressions," as Prochaska[4] writes it, "that are made upon the sensorial nerves, are most swiftly propagated along the whole length of

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tions in the figure of the uterus, needs not now a particular explanation or proof. The weight of the uterus itself, and of the adjacent viscera, being differently directed, must produce some change of posture, especially as the parts against which the uterus will rest its weight in the different postures of the body are of such different natures."—*Hunter's Anatomical Description of the Gravid Uterus*, p. 4.

[1] Anatomical Description of the Gravid Uterus, p. 67.

[2] All the conditions I have spoken of in the text relate to cutaneous irritations and impressions acting as stimuli to reflex motions. In cases of prolapsus of the umbilical cord, compression of the prolapsed funis by the fingers or otherwise seems generally to lead to a series of reflex movements of the body and limbs of the fœtus. During pregnancy this may be one of the means employed by nature to prevent, more frequently than otherwise would happen, the position of the cord between the child's head and the interior of the uterine cervix. It is certainly remarkable that we do not meet with this position of the cord more frequently, when we consider its length and its greater specific gravity than the liquor amnii. If, when so placed, and consequently compressed, reflex fœtal movements were always excited, this circumstance may possibly be the means of leading to, or producing, one other result—viz., the ultimate course and twisting of the cord around the body, neck, or limbs of the fœtus; in other words, such convolutions of the cord around these parts may be the result of sanitary reflex movements on the part of the fœtus—these sanitary reflex movements originating in, or being excited by, some unsafe or uneasy compression of the cord itself.

[3] Memoirs on the Nervous System, p. 70.

[4] Annotationes Academicæ, fasc. iii., p. 114.

the nerves to their origin, where, when they have arrived, they are reflected according to a certain law, and pass on to certain and corresponding motor nerves, through which, being again very swiftly propagated to muscles, they excite certain and determinate movements." The views which Prochaska took of the general object and aim of such reflex or excito-motory movements are precisely those which are attained by the aforesaid movements of the fœtus in utero. "The general law," he observes, "by which the common sensorium reflects sensorial impressions into motory, is the preservation of the individual (*conservatio nostri*); so that when external impressions act that are hurtful to our body, there follow certain motory impressions producing motions combined for the purpose of removing and driving away this irritation from our body; and, on the contrary, when external or sensory impressions act that are grateful to us, there follow internal or motory impressions productive of motions, having for their object the further maintenance of this grateful condition." [1]

All the so-called voluntary or motory muscles of the fœtus are not excited to an equal degree, or with equal facility, by the action of the physical impressions or stimuli to which it is subjected. The muscles of the trunk and upper extremities are much less moved under these stimuli than the muscles of the lower extremities are. Various authors have remarked incidentally, and without a view to any theory, that apparently the lower extremities move in utero much more than the upper.[2] Indeed, for some time after birth the movements of the legs continue to be greater in degree and extent than the movements of the arms. In the fluid medium in which it lies and swims in the uterine cavity, the motions of the lower extremities of the fœtus are more useful and powerful in enabling it to maintain and resume its position than any movements of the upper extremities could be.—And even in the adult, when the influence of the mind and cerebral system is withdrawn, reflex movements are far more easily excited as in the fœtus, in the lower than in the upper extremities. When a man receives an injury, producing fracture or displacement of one or more of his cervical vertebræ, and paraplegia in consequence is produced, "we find," says Dr Todd, "the patient presenting the following phenomena. His trunk and extremities appear as if dead (except the movements of the diaphragm); while the head lives. In full possession of his mental faculties and powers, he is, nevertheless, unconscious, save from the exercise of his sight, of any changes which may affect the parts below his head, nor is the utmost effort of his will sufficient to produce a movement of any, even the smallest, of these parts. If the stunning effect of the accident have passed off,

[1] Annotationes Academicæ, p. 117.

[2] Thus, for example, Professor Dubois observes, "Il ne faut pas oublier que les grands mouvemens du fœtus, et les plus ordinaires sont ceux qu'executent les extrémités abdominales."—*Memoires de l'Academie*, vol. ii., p. 285.

tickling the soles of the feet will be found to cause movements, of which, as well as of the application of the stimulus, the patient is unconscious. The limbs may be irritated in various ways, but without exciting any effects which the patient can perceive, excepting movements, and these he is aware of only from his happening to see them. It is *important* (adds Dr Todd) to notice that, in cases of this kind, movements are difficult of excitation in the *upper* extremities, *while they are aroused with great facility in the lower.*"[1]

4.—*Are there any parts of the Cutaneous Surface peculiarly susceptible of the appropriate physical impressions or stimuli leading to the Reflex Motions of the Fœtus?*

In the fœtus after birth some parts of the cutaneous surface, when stimulated, give rise much more readily than others to reflex muscular motions. In the adult the skin of the soles of the feet, of the knees, and of the sides, are thus specially susceptible. In other words, these parts are more sensitive than other parts to the irritation of touching or tickling. This over-susceptibility in the cutaneous nerves of these special parts is of no apparent use in the adult; and yet there is no property without its utility. It becomes generally more and more diminished and lost as life advances, and is greater in degree the younger the person, and greatest of all in the infant. Has this property not *its* special utility in the infant before birth? The parts of the fœtal surface most exposed to external irritation, the soles of the feet, knees, and sides, as seen in Plate I. and Plate II., are exactly those parts which are super-sensitive after birth to cutaneous irritation, and which most readily produce reflex movements of the limbs, in consequence of their cutaneous irritation. In a new born infant irritation of the sides of the chest is followed by more or less strong movements of both lower extremities. Rubbing or tickling the soles of the foot immediately excites a movement of change of position and retraction. "Certain localities," says Dr Hall, "are more susceptible than others to the effects of the excito-motory stimulus—the sole of the foot is especially one of these. Dr Little," he continues, "has published an interesting case of distortion of the foot, which only, but uniformly, occurred when it was placed upon the ground. Dr Dieffenbach met with a similar case"[2]

In anencephalous infants the same special parts of the cutaneous surface are principally or solely those through the irritation of which muscular movements are excited, when such infants survive after birth. In his Croonian Lecture on Muscular Motion, Sir Gilbert Blane describes some experiments upon a kitten which he decapitated, and upon another in which he divided the cord; and he states that after these injuries of the nervous centres, when the hind paws were irri-

[1] Physiology of the Nervous System (Encyclopedia of Anatomy), p. 19. See also Todd and Bowman's Physiological Anatomy, vol. i., p. 334.

[2] Diseases and Derangements of the Nervous System, p. 117.

tated, "the muscles belonging to the posterior extremities were thrown into contraction, so as to produce the motion of shrinking from injury." He subsequently adds, "In an acephalous monster the like phenomena were observable. It moved up its knees when the *soles* of its feet were tickled, &c." [1]

The results of pathology strikingly prove that there are some portions of the cutaneous surfaces which are much more susceptible than others, of producing reflex muscular movements, under the application of appropriate excitants. This is particularly seen in some cases of complete paraplegia depending on disease or division of the spinal cord,—and where all power of sensation and volition is abolished, as far as the paralyzed limbs are concerned. In instances of this kind, described by Drs Hall, Barlow, Budd, and others, the muscles of the paralyzed limb, though in no way obedient to volition, were capable of being excited into temporary action by various irritations of the cutaneous surface of the limb, but particularly by tickling of the sole of the foot.[2] Thus, in a case described by Mr Grainger, "a girl about fifteen years of age, was affected with angular curvature of the spine, producing insensibility and paralysis of the lower extremities. On tickling the *soles* of her feet, which, as an experiment, was often done, the legs were immediately slightly retracted, although the patient said she felt nothing; it was further remarked, that on touching the *other* parts of the feet or the legs in the same manner, no effect was produced." [3] "The results," observes Mr Grainger, "noticed in these cases are full of interest. They prove, first, that in parts of the body indisputably deprived of all feeling and power of voluntary motion, contraction may be excited in the so-called voluntary muscles, by impressions made on the skin; secondly, that this capacity of exciting muscular contractions, is not equally possessed by all parts of the external surface of the body, but that the *sole* of the foot is that precise part in which the action is excited in the most energetic manner." [4]

In cases of hemiplegia, particularly when the paralysis is complete, the same phenomena may be frequently observed. "In such cases," observes Dr Todd, "it is wonderful how easily movements may be excited in the palsied leg—very rarely in the arm, by the application of stimuli to the *sole* of the foot, or elsewhere with less facility. The patient, who acknowledged his utter inability to move even one of his toes, is astonished at the rapidity and extent to which the *whole* lower extremity may be moved by *touching* the sole of the foot, even with a feather." [5] The development of these excito-motory phenomena in paralytic cases is frequently (adds Dr Todd), "in the inverse pro-

[1] Select Dissertations on Medical Science, p. 262.

[2] See Diseases of the Nervous System, p. 233, &c. &c.

[3] Observations on the Spinal Cord, p. 94.—[4] Ibid, p. 94. See also Dr Budd's paper in Mad. Chir. Trans., vol. xxii., p. 187: "Impressions on the *soles* of the feet were more efficient than any that were tried on other parts of the skin."

[5] Physiology of the Nervous System, p. 19.

portion of the withdrawal of the power of the will."[1] And in the same way, and on the same principle, the *excito-motory reflex* muscular movements of the fœtus are probably only the more marked in degree and extent, in consequence of the absence during intra-uterine life, of all volitional and other mental influences. This remark leads us to a new subject, viz. :—

5.—*The degree of Reflex or Excito-Motory Movements manifested by the Fœtus in Utero, and the Period of their Commencement.*

In adult life the influence of, and susceptibility to, reflex movements in the so-called voluntary muscles of the trunk and extremities, is masked by the predominating power and effects of the mind and cerebral system over these same sets of muscles. Their excito-motory activity, however, is only obscured, not obliterated. Disease, by sometimes separating the influence of the cerebral system from the influence of the spinal system over these muscles, proves to us the persistance of their excito-motory activity, as in the cases of paraplegia and hemiplegia, adverted to in the two preceding paragraphs. These cases shew us by a kind of pathological analysis, that even in the adult, when the motory muscles of the trunk and extremities are withdrawn from the psychical nervous influence of the cerebral system, they are still found remaining subject to the physical nervous influence of the reflex or spinal system. "The truth," remarks Dr Hall, "is that the intellectual functions are daily developed during the first years of life, and obscure those of the excito-motory; but the latter are not enfeebled during this change, which is one of superaddition, not of substitution."[2]

All modern physiologists, however, admit that reflex or excito-motory movements are more distinctly and more powerfully developed, and are entrusted with the performance of more and more important functions in the animal economy according as we descend in the scale of animal life, and consequently also in the scale of individual life. In other words, the younger the individual is, the more evident and marked, as a general rule, is the power and influence of the reflex movements. "It is worthy," says Dr Hall,[3] "of special observation, that, in the very young animal, and in the cold-blooded animal, the phenomena of the excito-motor power are far more vividly manifested than in the older and the warm-blooded. In the very young kitten, even when asphyxiated to insensibility, every touch, contact, or slight blow—every jar of the table, any sudden impression of the external air, or that of a few drops of cold water—induces at once energetic reflex movements and acts of inspiration. The nostrils, the tail, the soles of the feet, the general surface, are all extremely sus-

[1] See also Dr Budd, in Medico-Chir. Trans., vol. xxii., p. 186 : "The reflex movements varied in extent and force, inversely with the degree of voluntary power."

[2] Diseases and Derangements of Nervous System, p. 138.

[3] New Memoir on the Nervous System, p. 29.

ceptible, and in degree in the order in which I have mentioned." "Phenomena of this nature," observes Professor Todd, "may be produced in all vertebrate animals. They are, however, especially marked in the cold-blooded classes. In the young of warm-blooded animals they are more manifest than in the adults of the same class."[1]

In accordance with the principle stated in the preceding paragraph, we ought to expect the manifestations of reflex movements to be well marked in the fœtus. And the whole of the present inquiry is intended to show that they are so, in particular sets of muscles, viz., in those connected with the general movements of the body. Other sets and combinations of muscles subject to excito-motory action after birth, as those of inspiration, suction, deglutition, defecation, &c., &c., are still in the state of absolute quietude during intra-uterine life, because the appropriate physical stimuli calculated to excite these reflex movements, are never applied till extra-uterine life is commenced, and because their action during intra-uterine life would be dangerous and even fatal to the fœtus. On the other hand, we have seen that the appropriate stimuli, calling into exercise the motory muscles of the trunk and extremities are applied during intra-uterine existence; and the action of these muscles is necessary on the part of the fœtus, in order to enable it to assume and maintain its position with the head downwards, an action which is in itself characterised by being intimately connected, as other excito-motory actions are, "with the preservation of the individual, and the propagation of the species."[2]

In several parts of his works, Dr Hall speaks of the extent of the phenomena referable to the nervous system that are manifested in fœtal life. But in consequence of not adverting to some of the points which have been attempted to be stated in the preceding pages, he takes too limited, and consequently, as I am inclined to believe, an erroneous view of the extent of the excito-motory or reflex muscular movements and actions that occur during intra-uterine life. "The fœtus in utero seems," says he, "restrained to a sort of ganglionic life. Every thing consists in nutrition and growth, or development. The contact of the liquor amnii is sufficient to keep the eyelids, the lips, and the sphincters closed."[3] "The fœtus in utero is," he again observes, "so little exposed to the influence of stimulants, or excitants applied to the cutaneous or mucous surfaces, that the excito-motory property is comparatively little called into action in the form of the reflex function. The contact of the liquor amnii may preserve the lips or the larynx, and the sphincter ani, closed. In this manner,

[1] Physiology of the Nervous System, p. 18.

[2] The "functions" of the "true spinal excito-motory system" are, as repeatedly explained and laid down by Dr M. Hall, those functions of the economy "on which depend, 1, the preservation of the individual; and 2, the continuance of the species."—*Diseases of the Nervous System*, p. 39, &c; *New Memoir*, p. 51; &c.

[3] Diseases and Derangements of the Nervous System, p. 131.

the reflex function, or the function of exclusion and of retention is in activity; but, as the agent in ingestion and egestion, it is as if it did not exist."[1]

In these, and the other passages in which he alludes to the state of the nervous system of the fœtus, Dr Hall nowhere, as far as I am aware, adverts or alludes to the strong and well marked reflex movements and actions performed during intra-uterine life by the motory muscles of the trunk, and particularly of the limbs of the fœtus, and which constitute, as I have endeavoured to show, the mechanism by whose instrumentality nature ultimately effects and produces in the human uterus, the normal and salutary position of the child with the head downwards and over the os. At what period of intra-uterine life reflex movements first commence in these so-called voluntary muscles, we have no power of precisely determining. The exercise of these reflex movements is not essential for the principal object which they seem intended to accomplish (namely, the proper position of the fœtus), till the latter period of pregnancy. But the fœtus is usually felt by the mother moving as early as the middle term of utero-gestation; and stethescopic observations show that they are often present to some extent before they come to be perceived at the time of quickening. "During the early stage of pregnancy," says Nægele, "while the fœtus is very small in proportion to the size of the cavity which contains it, and while the free movements of its limbs are consequently unrestrained, sounds produced by the movements of the fœtus may be occasionally distinguished as gentle taps repeated at intervals, and continued uninterruptedly for a considerable time. These sounds may sometimes be distinguished *several weeks before* the mother becomes conscious of the motion of the child, and also earlier than the pulsations of the fœtal heart, or the uterine souffle."[2]

In comparative physiology, we may have even *ocular* proof of the early commencement of reflex or excito-motory movements in the limbs, &c., of the embryo. In marsupial generation, the embryo, as is well known, leaves the uterus very early, and becomes immediately affixed to the nipple protruding in the interior of the marsupial sac, where it continues its development and growth. In the kangaroo, the embryo is expelled from the uterus as early, according to the observations made by Professor Owen, as the thirty-ninth day. An embryo of the kangaroo expelled at that period, was watched and examined by Mr Owen, and to use his own words, it "resembled an earth-worm in its colour and semi-transparent integument," and in the degree of development, its brain corresponded to that of the human embryo at the ninth week. Yet, in this early stage of development, the existence in it of reflex actions and movements was most distinct, for it

[1] Diseases and Derangements of the Nervous System, p. 243; see also p. 113, &c.

[2] Treatise on Obstetric Auscultation, p. 50.

breathed, *moved its limbs when touched*, and fixed upon the marsupial nipple. "It adhered freely," says Mr Owen, "to the point of the nipple; breathed strongly but slowly, and moved its fore legs when disturbed."[1]—*(To be continued.)*

ARTICLE II.—*On the Safety and Suitableness of Chloroform as an Anæsthetic Agent in some of the more Complex and Serious Operations in Surgery; with Illustrative Cases and Remarks on Lithotrity, and on the Lateral and High Operations of Lithotomy.* By WILLIAM KEITH, M.D., M.R.C.S.L., Surgeon to the Royal Infirmary, Aberdeen; and Lecturer on Clinical Surgery.

(*Read before the Edinburgh Medico-Chirurgical Society, February* 7, 1849.)

ETHER had been tried very fairly at the Aberdeen Infirmary without affording the satisfaction which many professed to derive from its use by inhalation, and without obtaining the confidence of the medical officers sufficiently to induce them to continue to employ it; so that when Dr Simpson's "Account of a new Anæsthetic Agent as a Substitute for Sulphuric Ether in Surgery, and Midwifery" was published, ether had by them been wholly laid aside. The new agent, *chloroform*, was speedily subjected to a similar trial, and the issue is unquestionably most triumphant. It has been used in all the ordinary operations of surgery, in amputations, excision of tumors, reduction of dislocations, &c., with the happiest effect, and without one attendant drawback. It has been employed at all ages with equal immunity from danger. On the 29th January, 1848, I subjected an infant aged four months to its influence, while the painful process of subcutaneous ligature of a large nœvus on the head was undergone, and that without a movement on the child's part, and with entire safety. But in two cases of lithotomy, violent convulsive struggling came on after the patients were secured for operation, which not only deterred the writer himself, but led him to discourage others from using chloroform in like cases.

Having, however, very recently enjoyed the advantage of being present while Professors Simpson, Syme, and Miller operated on various patients in a state of anæsthesia, and having been privileged to hear Professor Miller's admirable lecture "On the Surgical Experience of Chloroform," introductory to his course for the present session (1848), and which he has since published, it became quite apparent that the mode of administration, and not the chloroform, had been in fault in the two cases above referred to. Four cases of stone having offered

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[1] Cyclopædia of Anatomy—article Marsupialia—Vol. III., p. 322.

immediately on the writer's return to Aberdeen, each was subjected to the operation best suited to the case, and all to the anæsthetic influence of chloroform, and that with effects so entirely satisfactory, that he feels in fairness bound to make the issue of the trial known, with the view of abating prejudice, and of at least removing such doubts as he himself may have helped to create.

Not to weaken the evidence of their authenticity, the cases are given in detail; but it is trusted that the interest attaching to a class of cases, at all times so seriously involving life, will hinder the communication from seeming tedious.

Case of Stone in the Bladder treated by Lithotrity, the Patient being at the time under the Anæsthetic influence of Chloroform.

November 14, 1848. John Smith, farmer, aged fifty-six, from the parish of Glenbucket, admitted, labouring under the usual symptoms of stone in the bladder, which had continued without remission for a period of sixteen weeks; at the commencement of which the symptoms suddenly supervened after an attack of severe pain in one of his loins. States that he had suffered in a similar manner several times, and had on each occasion subsequently passed a calculus by the urethra of an oblong shape, one of which weighing twelve grains, and composed of lithic acid, he produced; but that he had been exempt from such for a period of five years prior to the present attack.

Nov. 15. Chloroform was administered freely. After a struggle of half a minute's duration, requiring his limbs to be steadied by four assistants, and then a good humoured indulgence in a display of celtic eloquence, which lasted five minutes, he went into a state of complete anæsthesia, breathing deeply, but without stertor. When in this state, the bladder was explored, and a stone of small size readily detected—the operation of sounding being much facilitated by the absence of any tightening at the neck of the bladder around the instrument, the sphincter having been rendered entirely passive.

Nov. 17. The bowels having been cleared out by a dose of castor-oil, and having retained his urine for two hours, he was at 10 A.M. put under the influence of chloroform. The same scene, though of shorter duration, was again enacted—the sleep ensuing calm, easy, and profound.

The screw lithotrite was introduced, the stone immediately seized, and crushed, and four fragments subsequently picked up and reduced to powder. The lithotrite was then withdrawn, and the screw scoop twice passed into the bladder, and each time brought out having its jaws filled with debris (sand and gravel). The operation, from the introduction of the lithotrite, occupied two minutes and a half. The sphincter of the bladder was quite relaxed during the whole proceeding, but the bladder was itself so active that the urine had to be restrained by the thumb and finger of the left hand, embracing the penis and shaft of the instrument.

This patient, during the operation, was still as a sleeping child, manifesting no signs of suffering, but in a degree conscious of the presence of the operator and students, and even capable of replying intelligently to a question put to another, which he had understood as addressed to him.

This patient never had the slightest bad symptom. For two days after the operation, he passed with his urine a large quantity of sand and fragments. On the fourth day after, he returned home a distance of fifty-seven miles.

Case of Stone in the Bladder in a Child on whom the Lateral Operation of Lithotomy was performed while under the Anæsthetic influence of Chloroform.

November 3, 1848.—Alexander Donald, aged three, a fine healthy-looking child, was admitted, presenting the following symptoms:—Unable to retain

his water for any length of time; agonizing pain after making it; sudden stoppages occurring during its flow; urine scanty, and high-coloured.

His mother states that he had fever when a year old; since which he never has been well or free from suffering, often passing gravel with his urine, and having great pain in so doing.

November 6.—He was put under the influence of chloroform, and that with the greatest facility, one drachm of chloroform setting him soundly asleep in one minute. On proceeding to sound him, a congenital phymosis was detected, the aperture exceedingly small. Circumcision was, therefore, at once performed, and the glans penis fairly exposed by peeling back the adherent remains of prepuce saved—an operation usually attended by considerable pain. The stone was felt, but the final sounding delayed until November 8, when again, under chloroform, the operation of sounding was conducted with an ease, comfort, and precision, that such an agent could alone secure. A stone, of small size, was readily detected.

November 25.—Having been brought fully under the influence of chloroform, while in his own bed, he was then carried to the operating theatre, held in the proper position by two assistants, and cut by the lateral mode. After some little delay, occasioned by the situation of the calculus behind the pubis, and its small size (weighing ten grains), it was extracted; a small tube introduced by the wound into the bladder, and the little patient, still sleeping soundly, conveyed to his bed. The whole proceeding occupied eight minutes. During the whole time, from his removal till his return to bed, he lay quite still, and wholly exempt from suffering; and awoke about an hour afterwards contented and happy, and to all appearance unaware of anything extraordinary having happened to him.

This little patient progressed in the most favourable manner, and was dismissed cured on the nineteenth day after the operation.

Case of Stone in the Bladder of an Adult in whom the Lateral Operation of Lithotomy was performed, while under the Anœsthetic influence of Chloroform.

November 14, 1848.—Alexander Beattie, aged forty-six, farmer, was admitted with all the usual symptoms of stone; was sounded, and a stone of some size, at once detected. States that, about a year and a half ago, he suffered from severe pains in his left lumbar region, the pains extending towards the symphisis pubis, attended by scalding heat along the urethra when voiding urine; that he had ever since had irritation about the bladder, occasionally passing gravel; was sounded by Dr Milne of Banff, eight months since, who found a stone, and urged the propriety of his submitting to lithotomy. In compliance with his advice, he presented at the hospital soon after; but his health being at the time feeble, his urine loaded with phosphates and slightly albuminous, he was put under a suitable course of treatment, and sent home until his health should be re-established. After an absence of nearly three months, he returned on the 14th November much improved in appearance, though still very spare of flesh. Urine light brown in colour, specific gravity 1017, free of deposit, whether treated by heat or by nitric acid, and neutral when tested by litmus paper. His appetite good, his pulse natural, and he sleeps well, his bladder being at present comparatively quiet.

November 25.—Having, by order, retained his water for half an hour, he was, while still in his bed, put under the influence of chloroform. This, though the first trial of that agent in his case, was accomplished with great ease and rapidity, simply by its copious administration (the handkerchief being well saturated with a pure chloroform). He was carried asleep to the theatre, the staff was introduced, and the limbs secured. While this was being accomplished, the stomach first and then the lower gut were emptied with considerable force, without awakening him or occasioning interruption to any one: Dr Pirrie, who was assisting at the time, remarked, however, that at the same moment the bladder was contracting spasmodically, and would, but for the

restraining pressure of his finger on the penis, have discharged its contents also. The incision was then made, the forceps introduced, a stone (weighing seven drachms) extracted, a tube inserted into the bladder, and the patient carried, still sound asleep, to bed; the whole operation having occupied two minutes. He slept for half an hour after, and on awaking expressed much gratification at finding the business over.

This patient made a good recovery, and, about a month after the operation, returned home to a distance in the country.

Case of Stone in the Bladder of an Adult in whom the High Operation of Lithotomy was performed while under the Anœsthetic influence of Chloroform.

October 31, 1848.—James Duncan, aged sixty-seven, saddler, admitted complaining of frequent calls to make water—of great pain along the urethra and beneath the glans penis, after voiding it—of the irritation being greatly augmented, and the urine tinged with blood after any exertion. Health in general good. Had no idea of stone being in his bladder. States that eleven years ago he was suddenly seized with pain in the region of the left kidney extending to the groin—was ill for a fortnight, and then gradually recovered. Three years since had a very similar attack in the same side, and from this period he dates the appearance of bloody urine on making any extraordinary exertion. He was subjected to chloroform to try its effects upon him preparatory to any operation; he struggled convulsively for three minutes, but when fully overcome lay perfectly still while being sounded, and for ten minutes after. Two stones of large size were in his bladder—so large as at once to suggest the propriety of extracting them above the pubis in place of risking the bruising of soft parts in attempting their extraction by the lateral mode. The urine was clear, free of sediment or deposit with heat or acid—reaction alkaline, sp. gr. 1017. Bladder dilatable by injection, and all the natural functions well performed for a man at sixty-seven. Pulse seventy-eight—full but soft. Tongue clean but tremulous.

November 25, Saturday, Eleven A.M.—The bowels having been well cleared out by castor oil and by injection, he was put under the influence of chloroform in bed, and that in half the time required on two former occasions on which its effects had been tried, and without a struggle. He was then carried to the operating table—laid on his back—the nates elevated—the lower limbs bent over the end of the table, and the knees secured by tapes to the legs of the table.

The urine in the bladder was allowed to escape through the injecting catheter, and the bladder then filled to distension with thin barley water, by means of a syringe—the attempt was made to throw in twelve ounces, but such was the resistance of the bladder, that the operation was proceeded with, only six ounces being retained, and that by restraint. An incision was made between the recti and pyramidales muscles, from a point about mid-way between the umbilicus and symphisis pubis to very near the root of the penis. The finger sufficed to separate these muscles—the abdominal fascia was scratched through, and the bladder reached; the duplicature of the peritoneum was very visible at the upper angle of the wound; it was pressed upwards and supported by the finger of the surgeon assisting; the wound was then extended upwards, the finger acting as a shield and directory to the knife until the recti muscles were separated nearly as high up as the commencement of the incision in the skin. The wound at the lower end was tight and insufficient until the tendinous connexion of both the pyramidal muscles with the pubis was cut through, when the freest possible access was then had to the bladder. The bladder was then seized with a spring or bull-dog forceps immediately behind the symphisis pubis, lifted up thereby, and a bistoury pointed downwards thrust through its coats. A finger promptly introduced hindered collapse of the bladder, and served as a guide to the knife in enlarging the wound in the bladder, a little way upward, but much more down towards the neck of the bladder. Before

withdrawing the finger a blunt hook was inserted and held by the assistant, so as to keep the wound patent and support the fundus of the bladder. Indeed, of so much importance was it deemed to prevent the possibility of the bladder falling down by collapsing into the pelvis, that the spring forceps allowed to retain its original hold, was confided, along with the blunt hook, to the assistant, and the means proved admirably suited. The forceps were then introduced, and in succession two stones of large size, each weighing two ounces, were extracted with extreme facility. A curved silver tube, with a shield at its outer end was inserted at the lower angle of the wound, the edges of which were then neatly closed by three stitches and isinglass plaster. Another tube with a sigmoid flexure was inserted per urethram, and both secured by an abdominal belt and perineal band. The whole operative procedure occupied six minutes, during which the patient slept soundly, and never moved a muscle. He was carried to bed, and continued to dose on with only sufficient interruption to enable him to take a drink, until 2 P.M., when he wakened up, and described the ease and comfort with which he went under the influence of the chloroform, and the entire absence of sensation, mental or corporeal, ever since the hour of eleven.

Eight P.M.—Has slept with little intermission. Feels easy. Pulse 76. Urine clear, and flowing freely by the suprapubic tube ; the other tube appearing to serve no good purpose is withdrawn, and the abdominal belt with the perineal band also laid aside. The wound neatly closed between the stitches with isinglass plaster, painted over with collodion, and a small square of oil-silk, with the tube-shield protruded through a slit in its centre, made to cover the whole, and convey the urine clear away from the abdomen.

November 26, Ten A.M.—Has slept well—urine copious. Pulse 80, soft. Skin and tongue both moist. The abdomen soft, easy, and in no degree tumid. The dressings guarded by the oiled silk are completely protected from moisture, and the skin all around sheathed by repeated smearings with lard. The urine finds a free and constant escape by the tube above the pubis.

Nov. 28, 7 A.M.—Continues to do well. Slept soundly all night ; pulse 88 ; tongue slightly furred, but moist ; bowels have not moved. To have half an ounce of castor oil for the second time ; urine copious. 10 A.M.—Removed the stitches from the wound ; union complete up to the tube : reapplied isinglass plaster. 2 P.M.—Belly rather tumid, but no complaint made on pressure being applied ; a purgative enema administered. 6 P.M.—The enema repeated. 9 P.M.—Bowels not relieved ; belly more tense ; admits now that there is some pain on pressure ; pulse 120, full and hard ; skin soft, but the tongue dry ; urine quite copious and limpid, distilling incessantly from the tube ; the tube removed ; bled to sixteen ounces ; repeat the enemata ; give five grains of calomel immediately, and one grain every hour after ; strong mercurial ointment to be rubbed perseveringly into his thighs. A warm cataplasm applied over the abdomen, to be renewed every two hours. 10 P.M.—The lower guts emptied by the enemata ; the belly felt somewhat relaxed. Pergat. 11 P.M.—Blood drawn at 9, very thickly buffed ; says he feels easy : the expression of his countenance is anxious ; breathing hurried ; pulse 130.

He continued the remedies until 4 A.M., his breathing becoming more and more hurried. At 5 A.M., on the 29th November, he expired.

At 11 A.M., the same day, an inspection was made. The abdomen appeared full, but neither tense nor much distended. The lips of the wound were easily separated ; the tract of the wound downwards to the bladder was brown looking, showing a few scattered points of commencing granulation. The duplicature of peritoneum lay entire in a loose fold, at the upper angle of the wound. A semicircular incision, passing from the crest of one ilium to the crest of the other, and above the umbilicus, afforded a flap, which, when thrown down over the pubis, allowed the viscera to come into full view. The intestines were distended by flatus ; slight traces of congestion were noticed, and it was thought that their peritoneal coat did not present its usual glossy appearance. There

was no agglutination of the intestines or other viscera to each other, nor any effusion of the usual products of inflammation, neither serum, lymph, or pus. The peritoneum lining the abdominal wall, and passing on to the fundus of the bladder, was then subjected to the most searching examination. The whole was healthy, glossy, and entire. The duplicature, stretched on the points of three fingers inserted at the wound, enabled all present to see, and be satisfied on these points.

On lifting up the intestines and looking down into the pelvis, the peritoneum, where it passes from the fundus of the bladder to the rectum, seemed thickened and whitened, as if from some effusion on its vesical side. The bladder was carefully detached from the pubis, and along with the surrounding parts, removed, when it became apparent that the cellular tissue in front of, and around the neck of the bladder, was loaded by infiltration with a plastic pus-like fluid. The same it was that gave the colour and thickened appearance to the peritoneum behind it. The pus was not collected in quantity, but evidently occupied the cells of the loose tissue around the neck of the bladder. It penetrated so low as to envelope the prostate gland on the left side, and on the same side permeated the tissue connecting the peritoneal and muscular coats of the bladder some way backwards. The fluid above alluded to, viewed through the microscope, was seen to contain the primitive filaments described by Bennett, and among these were numerous pyoid corpuscles, some of which bore a considerable resemblance to impure pus globules. The bladder was itself healthy, of course much contracted; the wound occupied the mesial line; measured an inch and a half in length; and extended from a point half an inch below where the peritoneum takes hold of the fundus, down to within one inch of the neck of the bladder.

From the above record it is therefore apparent, that death resulted from imbibition or infiltration of urine through the vesical wound into the cellular tissue around and below the neck of the bladder; and, humanly speaking, might have been prevented by the use of a lithotomy tube inserted into the bladder through a perineal wound. The syphon tube in the urethra completely disappointed expectation; and the suprapubic tube, though it answered admirably as a safety valve, was insufficient to secure the entire escape of the urinary secretion.

If asked why the high operation was in this last case selected, the reply would be:—Owing to the large size of the stones which had to be removed,—an element so influential on the results of the lateral operation for evil, that means must be devised for lessening the mortality after that operation, where the calculi come to exceed two ounces in weight.

Dr Simpson, in the Monthly Journal of Medical Science for November 1847, sets this matter in the strongest possible light, by the following table, composed from the data of Mr Crosse. It exhibits the mortality of lithotomy, calculated according to the different weights of the stone extracted:—

Weight of Stone.	No. of Cases.	No. of Deaths.	Ratio of Mortality.	Per Centage Mortality.
2 oz. and under.	648	65	1 in $9\frac{63}{65}$	10 in 100
From 2 to 4 oz.	46	23	1 in 2	50 in 100
From 4 to 7 oz.	9	5	1 in $1\frac{4}{5}$	55 in 100

In an experience, now extending over eighty-three cases of lithotomy, including hospital and private practice, the writer has arrived at results very strikingly similar. In fourteen cases, the weight of the stone ranged from two ounces up to four ounces; of these seven recovered, and the large proportion of seven died.[1] It need not, therefore, excite wonder, that while he seeks to recommend the adoption of Lithotrity for all stones weighing less than half an ounce, and the continuance of Lateral Lithotomy for all stones weighing less than two ounces, he should desire the revival of a mode of operating for the removal of such as are larger still, in which the soft parts escape bruising, and bulk in the stone ceases to be the same source of danger, because in the high operation the outlet is not *confined* by a solid arch of bone.—But, to return from this degression.

In the first case—where Lithotrity was performed, the influence exercised by the chloroform was altogether beneficial. Usually the sphincter of the bladder is so tightened by the patient's fears and struggles, that the movements of the instrument are very much impeded, the delicacy of touch, needful in detecting small fragments, quite destroyed, and a painful resistance offered to the withdrawal of the instrument, especially if that chance to be a scoop well filled with the debris of the broken stone. But under chloroform fears are at rest, the bladder insensible, and the sphincter quite relaxed. In such a case the operator has only to curb his own zeal, and, remembering that he is operating on a living being, suspend his manipulations before he has inflicted so much mechanical injury, as to excite after action in the vessels of the mucous membrane of the bladder.

Without chloroform, the straining is usually so great during the operation, that with every available means of restraint, much of the urine escapes by the sides of the lithotrite; and often, in consequence of the bladder thus becoming emptied, the operation is prematurely closed for the time. Under chloroform it was not

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[1] Lest the fact above stated should lead to a wrong estimate of the proportion in which large stones may be expected to occur in practice, it may be proper to state, that while these eighty-three cases offered for lithotomy, there were sixty-eight operated on by lithotrity, in which the stones averaged only two and a half drachms; and further, fourteen cases were refused as unfit, from age and decrepitude, in which the stones were all large.

The statistics of the writer's experience at this date, therefore, are—

68	stones under	½ oz.,	removed by	Lithotrity.
69	do.	do. 2 oz.,	do.	by Lithotomy.
14	do. from 2 oz.	to 4 oz.		do.
14	do.	do.	do.	unfit for any operation.
165				

giving of cases that offer with stones above 2 oz. in weight 1 in $5\frac{2}{8}$, and of such in which operative interference may be thought of, 1 in $11\frac{1}{14}$, so that in every twelve cases of stone that occur, five may be cured by lithotrity, five by lateral lithotomy, one by the high operation, and one rejected as unfit.

found so difficult to restrain the flow; but even were the bladder emptied, as there would be neither suffering nor resistance by the patient, the bladder could be again filled by injection with great ease, and the operation resumed. Usually, from the loss of consent between those parts necessary for the successful accomplishment of the effort, urine cannot be made for some hours after the operation, and accumulates so much to the discomfort of the patient, as often to require the catheter,—doubtless, owing to the painful stretching and struggle of parts connected with that important function. Under chloroform, the bladder is seen to contract itself promptly and completely, and continues to do so afterwards. It ought at the same time to be noted, that this active state of the bladder is in its way no less useful than is the passive state of the sphincter, in as far as, by keeping the bladder tense around its fluid contents, no loose folds can exist for fragments to lodge between.

In the second case,—that of the child subjected to lithotomy,—the operation was more than usually prolonged. Had it not been for chloroform, the struggles of the child might have added to the difficulties, and the shock to the constitution would have been proportionably severe. As it was, the child lay without a movement, slept without once awaking, and made an uninterrupted, a rapid, and a perfect recovery, having been dismissed cured on the eighteenth day thereafter,—the child's reminiscence of the whole proceeding being, that "the doctor gave him snuff, and took away his stone."

The observation made on these two cases, and the experience acquired, will induce the writer now to practise lithotrity on children also, an operation which he never did deem safe for them previous to the introduction of the use of chloroform.

In the third case,—another of lithotomy,—the whole proceeding occupied so brief a space, that there was little time to make observations. The emptying of the stomach, noticed so invariably if food has been taken recently, should induce attention to the precaution of making every patient fast for two hours at least before having chloroform administered. The quiescence during the operation was absolute; the comfort to the patient every thing that a total insensibility could secure; the recovery since was steady and progressive.

In the fourth case,—where the high operation was performed,—the comfort to the patient from the use of the anæsthetic agent, was immense, and little less so to the surgeon. The whole presented the semblance of a clean, calm, nice dissection; the patient suffering as little pain, and the surgeon as little interruption, as if what was realised had been only semblance. The favourable progress of the case for so many days after the operation, demonstrates the fact, that death did not result from any thing immediately connected with the operation itself. The infiltration of urine is supposed to have taken place on the night between Monday and Tuesday, when, being tired lying on his back, he turned on his left side, and

dislodged the tube for a time. Clearly, in another case, a more secure and efficient mode of exit for the urine must be ensured.

The chloroform used in these cases was very excellent and pure. It was prepared by Mr Robert Rattray, practical chemist, who manufactures it largely in Aberdeen, being apothecary to the hospital. It was by him also very efficiently administered, and its effects watched. The quantity consumed by the child was about two drachms; by each of the adults from six drachms to an ounce. The effect produced being more noticed than the measure as to quantity; the leading lights, in truth, being those two indicated by Professor Miller (see page 20th) in his published lecture,—"first, to make sure of a pure chloroform; and, second, when struggling commences, not to desist, but to go on with an increase of dose." The work just quoted from has so ably and so completely exhausted the subject of the "Surgical Experience of Chloroform,'' anticipating every conclusion that could be drawn in connexion with chloroform from the few preceding cases, that the writer, now they are finished, feels extreme diffidence in forwarding them to the Medico-Chirurgical Society. He does so in the belief, that his brethren in the provinces will feel much encouraged by the successful example of one of their own number; and in the hope that at a distance, *e.g.*, in our sister isle, where evidence emanating from the Scottish metropolis obtains credence with great difficulty, the testimony of an humble provincial may be more favourably received when he declares, after a patient and dispassionate trial, that chloroform, as an anæsthetic agent, is suitable, safe, and effectual in the severest operations in surgery.

ARTICLE III.—*On Typhoid and Typhus Fevers,—An Attempt to Determine the Question of their Identity or Non-Identity, by an Analysis of the Symptoms, and of the Appearances found after Death in Sixty-six Fatal Cases of Continued Fever, observed at the London Fever Hospital from January* 1847 *to February* 1849. By W. JENNER, M.D., London.

INTRODUCTION.

IT is beyond dispute that there are large groups of diseases arranged under the same heads in systematic works on medicine, of which the only bonds of union are, that their nature is unknown, and that they present in common certain pretty constant and striking symptoms.

Of these ill-defined anomalous groups, continued fever is one of the most remarkable and important. To trace the relations, and to detect the differences of the incongruous mass of diseases united together under this name, only the most careful analysis of daily recorded clinical observations can suffice.

The memory is too treacherous to be depended on in the science of medicine, in which the effects of primary are complicated by the effects of secondary lesions, and in which observation has to be corrected by observation.

For the elucidation of its obscurer points, no loose or general statements are of use—the most rigid induction can alone avail.

It would be a great point gained towards the right understanding of the true value of the differences observed in cases known as continued fever, if the question raised by Louis could be positively answered—no matter whether in the affirmative or negative—as to the identity of typhus and typhoid fevers; that is to say, have or have not British physicians confounded under the term continued fever, two *essentially* distinct diseases?—the one characterized by an anatomical lesion peculiar to it, and distinguishing it from all other acute diseases, the other exhibiting no certain anatomical character, and each presenting symptoms by which it can with more or less facility be distinguished from the other.

It will be necessary, before proceeding further, briefly to explain what is meant by the terms identity and non-identity, as applied to cases of typhus and typhoid fevers.

A given case of any disease always differs more or less from all other cases of the same disease, and therefore it may be said that no two are identical; but it is not with this kind of identity that we have here to do. It is not regarding differences in degree, but in kind, that question is here made.

Whether the primary affection is identical in typhus and typhoid fevers is the problem to be solved. Are the differences allowed to exist between them due simply to individual peculiarities, atmospheric differences, epidemic constitutions, or hygienic conditions, giving rise to local complications in the one which are absent in the other, and to variations in the symptoms as a consequence of these local complications? Or are they distinct diseases, as are scarlet fever and small-pox?—distinct as to their primary or exciting cause, their essential symptoms, and anatomical lesions. It may be as well, before proceeding further, to anticipate a little, and to state that the conclusion necessarily drawn from the analysis about to be submitted to the reader of sixty-six fatal cases of fever, is in opposition to the opinion of the principal writers on the subject of continued fever in this country. With few exceptions, British physicians have laboured to prove that typhoid and typhus fevers are identical. The results obtained by this analysis justify the assertion, that they are essentially distinct diseases. This difference from the opinion of others, I should be slow to express if I was not supported by a larger body of facts, than, so far as I am aware, has ever been offered in evidence to prove the identity of the two fevers. To those who delight to dwell on the imperfections common to all analysis of cases, who smile at any attempt to improve medicine by rigid induction, the value of the figures presented for their consideration may be increased by the

following general assertion, that for two years, in distinguishing the two diseases by the eruption alone, not a single error has been made, so far as could be proved by examination after death of the fatal cases, or by the progress of the non-fatal cases after their diagnosis was recorded.

Two cases cannot seem to differ more, to a superficial observer, than a slight sore throat, the result of exposure to the poison of scarlet fever, and a case of scarlatina anginosa, or than a mild case of scarlatina simplex and one of scarlatina maligna; yet no one now doubts the identity of the two, *i. e.*, as we here use the word, or that however much they may differ, it is in degree and not in kind, for we find every variation in the severity of the throat affection, and in the intensity of the rash uniting the two extremes; the latter may be present or absent, but if present is similar in character, though more vivid in the one case than in the other. The former is trifling or absent in the one, fearfully severe in the other; and it may be remarked that these two symptoms of the disease bear no necessary relation in severity the one to the other. In like manner, how different a mild case of modified smallpox from one of variola maligna; yet that they are varieties of the same disease is undoubted. No one, however, questions the non-identity of scarlet fever and smallpox. Although there are some symptoms common to the two diseases, and others which, more frequent in the one, are occasionally present in the other—*e. g.*, rigor, chilliness, followed by heat of skin, headach, delirium, somnolence or coma, hoarseness, sore throat, dysphagia—one or all of which symptoms may be present in either disease. In both there is an eruption of the skin, and though the usual period of its appearance differs, that of scarlet fever may not make its appearance till the third day, and the first symptoms of smallpox may have been so mild, that the patient will assert that he has been ill only two days when the skin affection appears.[1] The duration of the two generally differs; but the illness of the person attacked by either, may be prolonged by local complications to an indefinite period, and so the duration of the disease, which is the shorter, may in some cases appear to be the longer.

On what grounds, then, do we assert that they are two diseases?

1st, In the vast majority of cases the general symptoms differ.

2d, The eruptions, the diagnostic characters if present, are never identical.

3d, The anatomical character of smallpox (in this particular case also the eruption) is never seen in scarlet fever.

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[1] So like too is the rash, which often precedes by a day the eruption of the pustules in smallpox, to the rash of scarlet fever, that those conversant with the two diseases may be led to form an erroneous diagnosis. Cases are constantly sent into the London Fever Hospital, with certificates stating they are suffering from scarlet fever, the true nature of the case not appearing till the day after their admission.

4th, Both being contagious diseases, the one, by no combination of individual peculiarities, atmospheric variations, epidemic constitution, or hygienic conditions, can give rise to the other.

5th, The epidemic constitution favourable to the origin, spread, or peculiarity in form or severity of either, has no influence over the other, excepting that which it exercises over disease in general.

By the anatomical character of a disease I intend to signify that lesion, or those lesions of structure which are the invariable concomitants of a disease if it has continued for a given time, which time must be determined by a separate series of observations for every distinct affection. The anatomical character is not, be it remarked, and this is of immense importance, necessarily the cause of the disease, for it may be merely a symptom; but then, if the disease be not cut short by death, it is an invariable symptom. Thus the anatomical character of smallpox is the distinctive pustule; but the patient may die before that lesion of structure has been developed, and yet die of smallpox—the condition of the blood or nervous system, induced by the absorption of the poison, having been incompatible with a duration of life sufficiently long for the change of structure to take place. But the anatomical character may be the cause of death, and that before the primary disease has run its full course; or it, or its effects, may remain after the original disease has disappeared, and retard recovery, or even finally prove fatal to the patient. Again, the anatomical character being merely the invariable concomitant of a disease, and not necessarily its cause, it is evident that the extent of the lesion of structure constituting that character, need bear no relation to the violence of the essential symptoms of the disease. It must not be supposed that the question of the identity or non-identity of typhus and typhoid fevers is merely one of words—that its solution has no practical bearing—that it is a matter of indifference whether the latter be regarded as typhus with abdominal complication, or as a distinct disease. If continued fever be one disease, the *essential* treatment must be the same in every case, modified only by the presence of local complications; if two diseases,[1] then the essential treatment *may be* totally different for the one from that required by the other, and this without regard to local complications. The very groundwork of the treatment, so to speak, may for the one be diametrically opposed to that necessary for the other.

To illustrate the extreme practical importance of the point, we need only remember that, till a comparatively late date, scarlet fever and measles were viewed as the same disease. Of what value would any dissertation on the general treatment of the two have been, so

[1] From observations made on from 1000 to 2000 cases of continued fever, admitted into the London Fever Hospital, I think it probable that there are in this country at least four distinct continued fevers.

long as they were thus confounded, even though the local complication of scarlet fever—*i. e.*, the throat affection—had been taken into consideration? The cold sponging or douche, so beneficial in scarlet fever, might be death to a patient suffering from measles.

London alone, of all the cities of Europe, from the fact of fever with and without intestinal disease being almost constantly present within its bounds, afforded a field for observation capable of setting the vexed question at rest.

In Edinburgh, writes Dr Christison, the intestinal lesion is seen often enough only to prevent physicians being ignorant of its characters. In Dublin it appears to be equally unfrequent. While on the Continent fever without lesion of the agminated glands is so rare, that many eminent practitioners have doubted the existence of such an affection.

The London Fever Hospital, by collecting within its walls cases of continued fever from all parts of the great metropolis, offered peculiar facilities for its study; and I am indebted to the medical officers of that institution for permission to avail myself of the advantages it presented. To Dr Tweedie, the senior physician to the hospital, so long known and so highly appreciated by the profession for his researches on fever, I beg especially to acknowledge my obligations. To my talented friend, Mr Sankey, M.B., I am also greatly obliged for his readiness to assist me on all occasions, and for many valuable suggestions and observations.

In examining the subject of this paper—one beset with extreme difficulties, not so much from its own intricacy as from the mode in which fever has been treated of by the hosts who have chronicled its symptoms and its phases—I have endeavoured to bear the following rules constantly in mind:—

That the facts known with absolute certainty were to be kept distinct from those only approximatively ascertained.

That although, in order to settle a point of such moment, many cases were required, yet that a few observations, carefully and honestly made, were of more value than a multitude of general statements.

That the question at issue—*i. e.*, of the identity or non-identity of typhus and typhoid fevers—involving not only a consideration of symptoms but also of internal lesions, it was absolutely necessary that the latter should be demonstrated, and the former recorded, in every case that was to be admitted as evidence in determining the judgment.

I have during the last two years made notes of nearly 1000 cases of acute disease, chiefly of patients under the care of Dr Tweedie, at the London Fever Hospital—of these, sixty-six were cases of continued fever, which proved fatal, and were examined by me after death.

For the reasons before stated—*i. e.*, the absolute necessity of the condition of the intestinal mucous membrane being demonstrated,

and not surmised, with however great an amount of probability that might be—I have excluded from my present calculations all those cases which did not prove fatal; all those cases which, although they died, were not examined after death; and all those which I examined after death, but whose symptoms I had not noted during life.

These sixty-six fatal cases I have divided into two groups, placing in the one the cases in which disease of Peyer's patches existed; in the other, those in which, so far as the unassisted eye could determine, the agminated glands were in a healthy condition. Of these sixty-six cases, forty-three belonged to the latter group, or were cases of typhus fever; the remaining twenty-three being cases of typhoid fever.

I propose, in the first place, to take certain general conditions and the symptoms presented by either disease, and compare them with those which existed in the other; and then, in like manner, to collate the appearances found after death.

YEARS AND MONTHS IN WHICH THE SIXTY-SIX CASES HERE ANALYSED WERE ADMITTED.

Typhoid Fever.

Months.	Cases.
1847.	
January	1
February	1
August	4
November	1
1848.	
June	3
July	2
August	2
September	3
October	2
November	1
December	2
1849.	
January	1

Typhus Fever.

Months.	Cases.
1847.	
April	1
May	4
June	1
July	5
September	3
October	3
November	1
1848.	
May	5
July	3
August	1
September	3
October	2
November	5
1849.	
January	5
February	1

PREVIOUS HEALTH, GENERAL BODILY CONDITION, AND SIZE.

Typhoid and Typhus Fevers.—The patients, as a general rule, had enjoyed uninterrupted health before the attack from which they died, and were, in the large majority of cases, of robust bodily conformation, and, if admitted soon after the commencement of either disease, moderately stout.

The size of patients suffering from the two was nearly equal; thus, in *typhoid fever*—

Of the Males,	Of the Females,
The mean height was 5 f. 5½ in.	The mean height was 5 f. 1½ in.
The tallest ... ... 5 f. 10½ in.	The tallest ... ... 5 f. 5 in.
The shortest ... ... 5 f. 2 in.	The shortest ... ... 5 f.

In *typhus fever*—

Of the Males,	Of the Females,
The mean height was 5 f. 4½ in.	The mean height was 4 f. 7½ in.
The tallest ... ... 5 f. 10½ in.	The tallest ... ... 5 f. 6 in.
The shortest ... ... 4 f. 10 in.	The shortest ... ... 4 f. 6 in.

COMPLEXION.

Typhoid Fever.—Twelve out of fifteen of the patients, or 80 per cent., were noted as of fair complexion.

Typhus Fever.—Eight only out of twenty-six of the patients, or 30·7 per cent., of whom notes on the point were made, were fair; but here a fallacy exists, as will be shown when speaking of the aspect of the patients while suffering from this disease. Many, whose complexions were extremely dark when admitted with typhus fever, became comparatively fair as the disease disappeared. Of course this could only be observed in patients who recovered.

SEX.

Of the patients labouring under *typhoid fever* whose cases are here considered, thirteen were males; ten females.

Of forty-three case of *typhus fever*, twenty were males; twenty-three females.

AGE.

Typhoid Fever.—The average of the ages of the twenty-three patients affected with this disease was 22·08 years. The youngest was ten years old; the eldest thirty-six.

There were—

Between the ages of	10 and 19,	both years inclusive,	8	patients.
... ...	20 and 29,	... ...	12	...
... ...	30 and 36,	... ...	3	...

Typhus Fever.—The mean age of the forty-three patients suffering from this affection was 41·8 years. The youngest was eight years old, the eldest seventy.

There were—

Between the ages of	8 and 19,	both years inclusive,	5	patients.
... ...	20 and 29,	... ...	4	...
... ...	30 and 39,	... ...	9	...
... ...	40 and 49,	... ...	12	...
... ...	50 and 59,	... ...	6	...
... ...	60 and 69,	... ...	6	...
Of the age of	70,		1	...

MODE OF ATTACK, AND THE DATE OF THE DISEASE ON WHICH THE PATIENTS WERE FIRST CONFINED TO BED.

Typhoid Fever.—No data to determine the mode of attack could be obtained in six of the twenty-three cases. The disease, in seven only of the remaining seventeen cases, began so suddenly that the exact day of its commencement could be ascertained; six out of the seven took to their beds respectively on the 1st, 1st, 2d, 3d, 10th, and 16th days;[1] the last two patients, however, were obliged to lie down part of the day from an early period in the disease. No note was taken of the time when the seventh first kept his bed.

In the remaining ten of the seventeen cases, the disease began gradually. Of these, four were ailing for a few days, and then became suddenly worse; three of the four took to their beds respectively on about the 7th, 8th, and 11th days of the disease. The other six of the ten in whom the disease began insidiously, could fix on no particular day as that on which their illness began, but only stated that they became gradually ill from about a given day; of these, four took to bed severally on about the 3rd, 7th, 12th, and 17th days of the disease—when the others first kept their bed was uncertain. So that only 28·5 per cent. took to bed before the 7th day.

Typhus Fever.—Of sixteen of the forty-three cases no particulars as to whether the disease began suddenly or insidiously could be obtained.

Of the remaining twenty-seven cases, twenty-three were taken ill suddenly; nineteen of these twenty-three cases first kept their beds as follows:—

4 On the 1st day of the disease.	3 On the 4th day of the disease.
6 ... 2d day	1 ... 6th day ... ...
5 ... 3d day	

Thus all these patients were confined to their beds before the 7th day. Of these twenty-three cases, when four took to their bed was not learned. In four of the twenty-seven cases of which a correct history was obtained from the patients or their friends, the disease began insidiously, so that the day of its commencement could not be exactly ascertained, but they took to their beds on about the 2d, 3rd, 4th, and 6th days; so that, if this group be added to the first, every patient may be said to have been confined to bed by the 6th day; while, as we have seen, 28·5 per cent. only of the patients affected with typhoid fever took to bed before the 7th day.

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[1] In reckoning the day of the disease, both the day on which it commenced and the day on which the event referred to happened, whatever that may be, have constantly been included in the calculations. Thus a patient who entered the hospital on the 9th of a month, and whose illness began on the 4th, would be stated to have been admitted on the 6th day of the disease.

DURATION.

Typhoid Fever—Seven cases were received into the hospital respectively on the 4th, 5th, 6th, 8th, 10th, 12th, and 16th days of the disease, the average day of admission being the 8·7th.

Of thirteen patients, the exact duration of whose illness before they entered the hospital could not be ascertained—

1	was admitted on about the 7th day of the disease.			2	were admitted on about the 15th day of the disease.		
1	...	...	9th ...	1	...	...	19th ...
1	...	...	12th ...	2	...	...	21st ...
1	...	...	13th ...	1	...	...	27th ...
3	...	...	14th ...				

These thirteen cases were thus admitted on about the 14·5th day of the disease; one as early as about the 7th day, and one as late as about the 27th day.

The disease began gradually in these thirteen cases, so that the date of its commencement could not be accurately fixed; but four out of the thirteen cases became suddenly much worse, after from three to eleven days indisposition; and it is singular that, if the disease be reckoned to have commenced from the time of the sudden increase in the severity of the symptoms, and to which moment the patients themselves usually referred the beginning of their illness, the average day of the disease on which these four cases entered the hospital will exactly agree with the average day of the disease on which the patients were admitted, the precise commencement of whose illness was clearly ascertained—*i. e.*, both will have been admitted on the 8·7th day.

If the dates of the disease on the admission of the latter group of cases (being considered as nearly correct) are added to those of the first group of cases, it will be found that 23·5 per cent. only were admitted before the 9th day of the disease.

I omit one patient who had been ill, probably more than three weeks when admitted, and lived till after the 21st day from her entrance; and two cases in which the duration of the disease, prior to the day they first came under observation, was unknown. These patients remained in the hospital three and eight days before the fatal termination.

Of the seven cases, the exact date of whose illness on admission was known, three survived till after the fever had run its course. The other four died respectively on the 12th, 17th, 20th, and 27th days of the disease—the average day of death of these four cases being the 19th. This is probably below what a larger number of cases would give, because one case proved fatal on the 12th day from severe pneumonia.

Of the thirteen cases in which the duration of the disease before

the patients were received into the hospital was only more or less nearly ascertained, six lived some time after the termination of the typhoid fever, and the remaining seven died severally on about the 16th, 17th, 23d, 25th, 27th, 28th and 30th day of the disease. The average day of death, dating from the commencement of illness, being for these seven cases 23·7, this average probably, for the reason above stated, is nearer the truth than that obtained from the first group of cases, or those in which the exact date of the illness was ascertained.

This close relation between the average day on which the disease proved fatal in the two groups—*i. e.*, the group in which the exact date of the disease on admission was accurately fixed, and that in which it was only approximately ascertained—renders it probable that, in spite of the late date of the disease at which the second group entered the hospital, the mildness of their first symptoms, and then their sudden increase in severity, I have done right in dating the commencement of the illness from the moment when the patients first felt themselves ailing, and not from the time when they became suddenly worse.

If the two groups be added together, it will be seen that ten out of the eleven cases—*i. e.*, 90·9 per cent.—survived till after the fifteenth day, the only exception being the patient who died of pneumonia on the 12th day of the fever.

Twelve[1] of the twenty-three cases here analysed proved fatal during the progress of the fever. The average day of residence in the hospital on which these patients died was the 10·5th. One case proved fatal on the 3rd day; and one lived till the 20th day after admission.

Ten survived the fever; but the duration of the disease in one of these cases was too uncertain for it to be taken into calculation.

Dating from the commencement of illness, the remaining nine cases proved fatal, on an average, about the 47th day—one of the patients dying on the 32nd, and one as late as the 86th day; or, counting from their entrance into the hospital, one lived till the 20th day, and one till the 79th day—or they died, as an average, the 32nd day of their residence in the hospital.

Typhus Fever.—The exact day of the disease on which the patients were admitted into the hospital (omitting one case, that of a nurse who resided in the institution), was ascertained in twenty-two cases to be as follows:—

1	was admitted on the 5th day of the disease.			5	were admitted on the 9th day of the disease.		
2	... ...	6th	...	2	... ...	12th	...
4	... ...	7th	...	1	... ...	14th	...
7	... ...	8th	...				

[1] One case is here omitted, because I have not the data to determine whether it proved fatal before or after the fever had run its course.

The day of the disease on which nine of the forty-three patients were admitted, was only approximately learned. Of these—

1	was admitted on about the 4th day of the disease.			1	was admitted on about the 9th day of the disease.		
1	...	...	5th ...	1	...	...	10th ...
2	...	...	7th ...	2	...	...	14th ...
1	...	...	8th ...				

So that the twenty-one patients, the commencement of whose illness was clearly made out, were, on on average, admitted into the hospital on the 8th day of the disease—one entering as early as the 5th, and one as late as the 14th day; and the nine patients, the exact date of whose illness was not quite accurately ascertained, were, on an average, admitted on about the 8·6th day of the disease—one entering as soon as about the 4th day, and one as late in the disease as about the 14th day. Or, if the two groups be added together, it will be seen that as many as 61·29 per cent. were admitted before the 9th day of the disease, whereas only 23·5 per cent. of the cases of typhoid fever were admitted before the 9th day.

The duration of the disease, prior to the patient's entrance into the hospital, was entirely unknown in eleven cases. Of these, four survived after the fever terminated. Out of the twenty-three cases in which the exact day of the commencement of the disease was accurately fixed, five recovered so far as the fever was concerned. Of the remaining eighteen patients—

1	died on the 10th day of the disease.			1	died on the 15th day of the disease.		
2	...	11th ...	...	2	...	16th ...	...
2	...	12th ...	...	2	...	17th ...	...
3	...	13th ...	...	1	...	19th ...	...
3	...	14th ...	...	1	...	20th ...	...

The average day of the disease on which death in these eighteen cases occurred, was the 14·27th; one dying on the 10th day, and one not till the 20th day.

Of the nine cases in which the day that the disease began was only approximately ascertained, two died after the fever had run its course. Of the seven cases which proved fatal during the progress of the fever—

1	died on about the 9th day.		1	died on about the 15th day.	
1	...	11th ...	1	...	16th ...
1	...	13th ...	2	...	17th ...

The average day of the disease on which these seven cases died, being about the 14·4th day—one terminating fatally on about the 9th day, and one as late as about the 17th day. Or, if the two groups of cases be taken together, it will be seen that 36 per cent. only proved fatal after the 15th day of the disease, and not one after the 20th day; presenting in this particular a striking contrast to the cases of typhoid fever, of which, as we have seen, 90·9 per cent. proved fatal after the 15th day, and nearly one half after the 20th day.

Thirty-two cases proved fatal during the progress of the fever. The average day of residence in the hospital on which these patients died was the seventh—two dying on the third day after their admission, and one surviving till the sixteenth day.

The six patients (omitting one who died of smallpox) that recovered from the attack of typhus, and the date of the commencement of whose illness was ascertained, or nearly so, died respectively on the 22d, 23d, 24th, 29th, 44th, and 148th days, reckoning from the outset of the disease; the day of death of these patients being respectively the 19th, 18th, 17th, 17th, 31st, and 141st of their residence in the hospital.

The four patients, the duration of whose illness before admission was unknown, and who died of diseases that supervened during the progress of the typhus, and continued after that had run its course, or that began after its termination, died severally on the 11th, 12th, 16th, and 30th days after they were admitted into the wards.

ERUPTION.

Description of the Rose Spots peculiar to Typhoid Fever.—The eruption in the cases of typhoid fever here analysed, was papular. The papulæ, called by Louis *taches roses lenticulaires*, possessed the following characters :—

They were slightly elevated. To detect the elevation the finger had to be passed very delicately over the surface, as they had none of the hardness of the papulæ oftlichen, or of the first day's eruption of smallpox. Their apices were never acuminated, never flat, but invariably rounded—their bases gradually passed into the level of the surrounding cuticle. No trace of a vesicle or white spot of any kind was ever detected on them. They were circular and of a bright rose colour, the latter fading insensibly into the natural hue of the skin around. They never possessed a well-defined margin.

They disappeared completely on pressure, resuming their characteristic appearances as soon as the pressure was removed, and this was true from first to last, from their first eruption to their last trace. They left no stain of the cuticle behind; they never passed into any thing resembling petechiæ—the characters they presented on their first appearance continued till they vanished. Their ordinary size was about a line in diameter, but occasionally they were not more than half a line, and sometimes a line and a half in diameter.

The duration of each papula was three or four days; fresh papulæ made their appearance every day or two. Sometimes only one or two were present at first, ran the course above described, and then one or more fresh ones made their appearance, vanished in three or four days, and were followed by others to last as long.

The number of papulæ seen at one time on the surface was ordinarily from six to twenty—though occasionally there was only one, and sometimes more than one hundred.

They usually occupied the abdomen, thorax, and back, but were occasionally present on the extremities. One was frequently noticed on the thorax over the cellular interval, at the upper border of the pectoralis major on either side.

A *very pale and delicate* scarlet tint of the skin sometimes proceeded the eruption of the papulæ, but never lasted more than a day or two. The skin resembling in tint that of a person shortly after leaving a hot bath.

Rose spots were present in nineteen of the twenty-three fatal cases here analysed.

There was no eruption detected in four cases.

The date of the first appearance of the papulæ was ascertained in three cases, which commenced suddenly, and was the 8th, 12th, and 20th days; the patients being admitted respectively on the 4th, 5th, and 10th days of the disease.

In a fourth case, in which the papulæ first appeared on nineteenth day, the patient, a strong made butcher's man, left his work only eight days before the eruption appeared, but had been ailing for ten days before he left his work.

The papulæ were present in thirteen cases when the patients first came under observation—three of these cases being seen for the first time on the 9th, 13th, and 18th day of the disease; five on about the 8th, 10th, 12th, 16th, and 22d day; and two on about the 15th, and two on the 28th day. In one case no clue to the duration of the disease before the patient's admission could be obtained.

In two cases the date of the first appearance of the spots was not noted.

Of the four cases in which no spots were detected, three had been ill before they entered the hospital more than fourteen days, and one probably more than three weeks.

The longest time any given papula remained visible was six days, the shortest time one day. The average duration of each spot, calculated from repeated observations on nine of the cases here analysed, each observation including several spots, was 3·14 days.

In three cases which proved fatal on the 32d, 34th, and 86th days, fresh spots made their appearance as late as the 29th, 21st, and 24th[1] days. The first of these cases proved fatal from peritonitis, the other two died of erysipelas.

In seven cases which proved fatal on about the 23d, 25th, 28th, 30th, 44th, 46th and 71st days, fresh spots were observed to appear respectively, as late as about the 19th, 15th, 24th, 24th, 23d, 24th, and 30th days of the disease. Of these patients, the first three, or those who expired as nearly as could be ascertained before the 30th day, died chiefly from the abdominal lesion.

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[1] This patient had a relapse after convalescence was partially established, and then spots re-appeared and continued as late as the forty-ninth day.

The last four died respectively of pneumonia, sloughing, erysipelas, and pleuro-pneumonia and sloughing.

No second crop of papulæ was observed in seven cases, the patients dying on the 2d, 3d, 3d, 5th, 6th, 7th, and 15th days, after they came under observation.

In the two cases in which the date of the first appearance of the spots was not noted, the presence or absence of more than one crop of papulæ was not recorded.

To sum up—

1. Rose spots were detected during the progress of all the cases admitted before the fourteenth day of the disease.

2. The spots usually appeared between the seventh and fourteenth days after the first symptoms of the disease showed themselves.[1]

3. The ordinary duration of each spot was three days.

4. Fresh spots generally appear every day or two after their first eruption, till from the twenty-first to the thirtieth day.

5. One case relapsed, and in that fresh papulæ appeared every day or two till the twenty-fourth day, then no more were seen till the thirty-seventh day, after which fresh spots were again noted daily till the forty-ninth day, reckoning from the first outset of the disease.

6. The fever terminated by the thirtieth day, as proved by no fresh spots appearing after that day, unless in the case of a relapse, and by the presence of local complications sufficient to account for death in all cases that proved fatal after that date.

Description of the Mulberry Rash[2] *peculiar to Typhus Fever.*—In the cases of typhus fever, the eruption was never papular. Its characters varied with its duration. On the first appearance of the rash, it consisted of very slightly elevated spots of a dusky pink colour. Each spot was flattened on the surface, irregular in outline, had no well-defined margin, but faded insensibly into the hue of the surrounding skin, disappeared completely on pressure, and varied in size from a point to three or four lines in diameter. The largest spots appeared to be formed by the coalescence of two or more smaller, and the shape of the former accordingly was more irregular than that of the latter.[3]

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[1] I would here remind the reader, that as these particulars are deduced from a limited number of fatal cases, the exact days of the first and last appearance of the eruption seem to be a little more irregular than they really are. It is well known that fatal cases of smallpox and scarlet fever are not the best fitted for exhibiting the regular course of the eruption in the two diseases.

[2] I have ventured to use a new term for designating the rash peculiar to typhus, because I consider it unadvisable to retain any of the names now in use, limited as each of those names is to a peculiar modification of the one rash here described. In consequence of the eruptions in scarlet and typhoid fevers having received names from their colour, and the rash of typhus fever often resembling in hue the stain of the mulberry, I have been led to adopt the above term.

[3] In some cases it was doubtful if the spots on their *first* appearance had not the characters here assigned to them as their second stage.

Second Stage.—In one, two, or three days, these spots underwent a marked change; they were no longer elevated above the surrounding cuticle, their hue was darker and more dingy than on their first appearance, their margins rather more, but still imperfectly defined, and now they only faded on pressure.[1]

In this stage they were usually darker, less affected by pressure, and their margins more defined on the posterior than on the anterior surface of the body. In some cases the spots after this grew paler, passed into faintly marked reddish-brown stains, and then disappeared.

Third Stage.—In others a third stage was reached, the centres of the spots became dark purple, and remained unaltered by pressure, although their circumferences still faded; or the entire spots, the circumferences as well as the centres, changed into true petechiæ, *i.e.* spots presenting the following characters,—a dusky crimson or purple colour, quite unaffected by pressure, a well-defined margin, and total want of elevation above the level of the cuticle. This alteration was most frequently observed to take place on the back, at the bend of the elbow, and in the groin. At the bend of the elbow they were generally oval, their long axis lying in the direction of the long axis of the arm.

In a large majority of the cases, the spots were very numerous, close together, sometimes almost covering the skin. In a few instances, however, they were comparatively few in number, very pale, and situated at some distance from each other.[2]

The usual situation of the spots was the trunk and extremities, but occasionally they were limited to the trunk, and now and then were observed on the face.

Their number reached its maximum on the first, second, or third day, no fresh spots appearing after the latter date, and each spot remained visible from its first eruption till the whole rash vanished.

When very numerous, the whole of the spots seen together on the surface had not an equal depth of colour, many were much paler than the others, and had a dull appearance as if seen through the cuticle. In my notes I have been in the habit of distinguishing these collectively as the subcuticular rash. *It often, by its abundance, gave*

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[1] By the term "faded on pressure," I intend to signify, that however firmly the finger was pressed on the spots, they could not be entirely obliterated, but some trace of the spots or stain of the cuticle remained.

[2] In these cases, on the first day of their appearance they occasionally bore so close resemblance to the rose spots, that although they were never altogether identical with the best marked specimens of the latter, yet the most tutored eye might be in some doubt as to which order they belonged; and when the general symptoms were at the same time equivocal, the diagnosis was impossible till a day or two had elapsed, when some or all the spots passed into their second stage; whereas, if they had been the spots peculiar to the typhoid fever, they would have retained the characters they presented on the first day, till they disappeared altogether on the third or fourth day after their eruption.

a mottled aspect to the skin, on which ground the darker spots were seated.

Variations in the absolute or relative amount of the subcuticular rash, and of the spots, as well as in the depth of their respective colours, cause much difference in the general appearance of the rash. Sometimes it resembles measles so closely as to be distinguished from it with difficulty; at others, it presents that appearance which has been called the spotted rash; and again, it is sometimes so pale, that unless carefully looked for it might be passed over altogether. When the spots on the back were of a much deeper hue than those on the anterior surface of the trunk, the skin covering the posterior surface was generally considerably congested. Slight pressure of the finger leaving a white mark, which slowly returned to its previous dusky red colour.

The eruption mulberry rash, characteristic of typhus fever, was observed in all of the forty-three cases of that disease here analysed.

The rash appeared in two cases after the patients' admission into the hospital; the date of its first eruption in these two cases was the seventh and ninth days. In the remaining forty-one cases, the rash was developed before the patients came under observation.

Of these forty-one cases—

1	was first seen on the 6th day of disease.			1	was first seen on about the 5th day of disease.		
2	were ...	...	7th ...	2	...	...	8th ...
5	...	...	8th ...	2	...	...	9th ...
5	...	...	9th ...	1	...	...	10th ...
5	...	...	10th ...	2	...	...	15th ...
1	...	...	12th ...				
1	...	...	13th ...				
1	...	...	15th ...				

And in twelve cases it was uncertain how long the disease had existed prior to the presence of the rash being noted.

In eleven of the forty-three cases, the eruption disappeared before death, a mere trace of the spots only existing in four cases on the 14th day, in two on the 20th, and in one there was no trace of the spots on the 24th day, no note of their condition having been taken for some days preceding; these seven patients were first seen on the 7th, 8th, 9th, 9th, 9th, 10th, and 15th days of the disease, the eruption being present on all at these times.

The four patients from whom the rash disappeared on the 14th day, died on the 23d, 24th, 32d, and 148th days, after the first symptoms of fever showed themselves. Of these the first died of asthenic laryngitis; the second of phlebitis, which began after the patient had left his bed and was considered convalescent; the third of smallpox, caught during convalescence; and the fourth of erysipelas, sloughing of the back, and pneumonia. The diseases that caused death in the first and last, supervened during the progress of the fever, and continued after that had run its course.

Of the three patients from whom the rash disappeared on the 20th, and before the 24th days of the disease, the two former died on the 22d and 29th days of circumscribed gangrene of the lung, and of sloughing abscess and pleuro-pneumonia; and the third on the 44th day, of gangrenous abscess of the lung. These fatal complications also arose during the course of the primary disease, and caused the patients' death some time after the specific fever had terminated. It was uncertain how long the disease had existed prior to the admission of the remaining four patients (*i.e.* of the eleven from whom the eruption disappeared before death), but a mere trace only of the spots was to be seen on the 8th, 9th, 9th, and 13th days of their residence in the hospital, the patients dying on the 11th, 12th, 30th, and 16th days respectively, of pus in the joints, cerebral congestion, pleuro-pneumonia, and erysipelas; all commencing before the fever had finished its course, and continuing and proving fatal to the patient after its termination.

In one case, some difficulty was experienced in determining the nature of the spots, *i.e.* whether they were true typhus spots; they were in fact so imperfectly marked, that the diagnosis between typhus and typhoid fever could not in this case have been positively formed from the eruption alone.

In the remaining thirty-one cases, the rash was distinctly visible till death. In four of these cases, it became somewhat paler the last two or three days of life; in four others, no note of its condition was made for a day or two before the fatal termination—these eight patients survived beyond the 14th day of the disease; in twenty-three of these thirty-one cases, the rash continued to grow darker, and to be less affected by pressure, till death.

Of the thirty-one cases in which the mulberry rash was distinctly visible till death—

1	died on the	10th	day of the disease.	1	died on about the	9th	day of disease.
2	...	11th	... ...	1	...	11th	... ...
2	...	12th	... ...	1	...	13th	... ...
3	...	13th	... ...	1	...	15th	... ...
3	...	14th	... ...	1	...	16th	... ...
1	...	15th	... ...	2	...	17th	... ...
2	...	16th	... ...				
2	...	17th	... ...				
1	...	19th	... ...				
1	...	20th	... ...				

The date of the disease on admission in six cases was entirely unknown; two of the six died on the third day of their residence in the hospital, the other four respectively on the 4th, 6th, 7th, and 8th days. To sum up,—

1. The mulberry rash was present in all the cases.

2. The rash usually appeared from the 5th to the 8th day of the disease.[1]

[1] See note, p. 676.

3. Fresh spots never appeared after the 2d or 3d days of the eruption.

4. The duration of each spot was from its first appearance till the death or recovery of the patient from the attack of typhus.

5. The rash disappeared between the 14th and 21st days of the disease; when death ensued after the latter date, it was the result of local disease, which either complicated the progress of the fever, and continued after that had run its course, or sprung up anew, connected or not with the enfeebled state of constitution, the consequence of the fever.

6. In no case was there any return of the eruption, and therefore *no true relapse.*

ARTICLE IV.—*Cases of Syphilitic Periostitis, followed by Phagedenic Ulceration, occurring in the Lower Limb, with Practical Observations.* By G. CRITCHETT, Esq., F.R.C.S., Assistant Surgeon to the London Hospital, &c.

THREE cases have lately been under my care, which seem to throw light upon some of the stages through which syphilitic periostitis, or as it is commonly called "the syphilitic node," occasionally passes; and also upon the treatment best adapted for such a morbid condition; and I am induced to bring the subject before the profession, from finding a remarkable tendency in such cases to continue for an almost indefinite period, becoming gradually worse and resisting most of the ordinary methods adapted for their cure; and I may add, from the remarkable rapidity and certainty with which they yield to the plan I am desirous of recommending. I think I shall most clearly explain myself; by first briefly relating the cases, and then adding such inferences theoretical and practical, as may seem fairly deducible from them.

In the spring of 1848, I was called in consultation with a physician to see a gentleman, about thirty years of age, residing in the neighbourhood of Camberwell. I found him suffering from a deep foul ulcer, with rugged burrowing edges, and occupying a superficies of about two and a half square inches, on the surface of the left tibia, rather below its centre—the margin was of a reddish blue colour, considerably raised and uneven, and at one point a probe could be passed beneath the skin to the extent of about an inch. The surface was highly irritable, and the discharge thin. Considerable pain was complained of, aggravated at night, and by the depending position. The patient was of a bilious tempermant, feeble, and emaciated. It appeared from the history he gave me, that he was by profession a sailor; that about three years ago he contracted a primary syphilitic sore which had been healed without mercury; that he had been troubled with a cutaneous eruption, of which however no trace then remained; and that about a year ago he first perceived a hard swelling over the shin bone, in the situation of the present sore; which had been attended with severe nocturnal pains. As this occurred while he was at sea, but little attention was paid to it, and no treatment was adopted. At the end of about four months, a fluctuating tumor formed over the hard swelling, and

in about ten days this broke and discharged a small quantity of matter. The opening gradually enlarged and formed a sore, which was the commencement of the present ulcer. The treatment up to the time of my seeing him, had consisted chiefly in water dressing, *absolute rest*—the patient having been kept in bed about three months, rather a limited meat diet, no stimulus of any kind, and mild medicines such as the decoction of sarsaparilla. I recommended the application of the black wash in lint to the wound, to be retained by short pieces of strapping and a bandage ; a more liberal diet including some amount of stimulus, and I substituted the extract for the decoction of sarsaparilla—suggested also that the patient should endeavour to get up a little, as his health was suffering severely from prolonged confinement in bed. I did not see this patient again for about two months, when he came to my house. I found him much improved in health; and on examining his leg I found the sore much changed in appearance, the upper part and a large portion of the centre had healed, but there was considerable undermining at the sides and below, with an evident disposition to spread; there were also two or three small sores of the same character in the vicinity. The plan I had recommended, had been persevered in, and apparently at first with great benefit, but although the sore had appeared at one time to be nearly healed, the edges had always remained unsound. The case being now placed entirely under my own management, I adopted the following plan—taking small pieces of lint thoroughly saturated with black wash, and well covered with the black deposit, I not only covered the ulcerated surface, but with a probe I thrust pieces well underneath the overhanging edges, so as to get them in contact with the entire morbid surface. I then applied mechanical support to the limb from the toes to the knee, by means of adhesive bands, in the way I have detailed at page 417 of the last volume of the Lancet ; over this I put a bandage. The constitutional treatment the same as before, gentle walking exercise to be taken. When he came to me again, two days after, the wound was improved, the lint could not be pushed so far under, and the pain was much diminished ; there was, however, one point where the probe passed up to such an extent, that, thinking it impossible to get the wash applied to it, I introduced a director and laid it open. I continued to dress this case, every other day, for about six weeks, at the end of which time the leg was perfectly sound ; during this period, the patient did not lay by for a single day, but often took rather long walks. I heard a few days ago that he remains perfectly well.

The next case is that of a young man of a sanguine lymphatic temperament ; by occupation a light porter to a city banking house. I saw him at the request of one of the partners in July of last year. I found three large sores with ragged edges and highly irritable surfaces on the flat part of the right tibia, which, however, had lost its flatness and become, to a considerable extent, convex, a fact which was very noticeable when it was compared with the other shin. It appeared that about five years ago he contracted a chancre, for which he was under treatment in one of the metropolitan hospitals. Possessing one of those strumous constitutions in which syphilis commits its worst ravages, and mercury exercises its least salutary power, he seemed to have passed through the various stages of the disease in their more aggravated forms, and during the last nine months, severe nocturnal pains in various parts of the body and a large swelling over the right tibia, shewed that the disease had finally located itself in the periosteum. About six months before I saw him, the surface of the tibial swelling had become discoloured, and small wounds had formed, which gradually increased to their present size. He had been attending as an out-patient at the same hospital for some months, and up to the time I saw him, the principal treatment had been the administration of the iodide of potassium and the application of poultices. The unhealthy aspect of this patient, the traces of the syphilitic poison still shewing themselves ; the great size of the wounds, their extreme irritability, the inflamed condition of the surrounding parts, and the very questionable condition of the bony substratum, all conspired

to give me a very unfavourable impression of the case, and I commenced the treatment with little hope of success; I continued the same constitutional plan as before; I applied black wash freely to the wounds, and fearing from the hard condition of the parts surrounding the sores, and from the subacute character of the inflammation as evidenced by the bright red colour of the vessels, the facility with which they emptied on pressure, and the rapidity with which they refilled, that strapping could not be borne, I applied strips of wet linen in the same manner as I do strapping to the entire limb, after the plan first suggested to me by my friend Mr Ward, and recommended in Mr. Chapman's work on Ulcers. This treatment I continued for about a month with manifest advantage, the discharge becoming thick and abundant, the surface losing its irritability, the edges assuming an even and white aspect, and the surrounding parts changing their colour from red to a deep blue. At this stage the discharge gradually diminished, until the surface of the sore became perfectly dry and ceased to heal. I would here repeat what I have mentioned in a former publication, and what I have frequently noticed, that in ulcers of any size, the water dressing, after accomplishing a certain amount of improvement, produces a dry surface and ceases to promote the healing of the wound, producing a stationary condition, which I have found from direct experiments to continue for many weeks. Under these circumstances, I commenced the application of mechanical support by means of strapping, making but slight pressure in consequence of the hard unyielding nature of the surrounding parts; and as the sores had now entirely lost their specific character, I substituted for the black wash a solution of the nitrate of silver, twenty grains to the ounce of water. Under this plan, the ulcers recommenced the secretion of pus, and healed steadily, though slowly, the man all the time pursuing his accustomed avocations, with as much comfort, according to his own account, as if the leg were well. About two months after the commencement of the strapping, all the wounds had healed, except one, which was about the size of a shilling, and progressing very slowly. He had discontinued the constitutional treatment, and he began to complain of nocturnal pain in the head and arms; a periosteal swelling formed on the frontal bone, and another on the left humerus; the former suppurated, broke, and healed, and on resuming the iodide of potassium the latter rapidly subsided. At the end of another month, making altogether a period of above four months from the commencement of my treatment, the man left me apparently perfectly well, and I have reason to know he still remains so. I advised the medicine to be continued for some weeks after the wounds were healed.

The third and last example of this form of disease that I propose relating, occurred in a gentleman, about thirty years of age, connected with one of our public offices. When I was called to him about two months ago, I found four sores over the upper and anterior part of the right tibia. The central one was rather large, deep, and presented a slough at the bottom; there was a smaller sore below, and on each side was an ulcer of a crescentic shape, evidently spreading on its outer and convex edge, and apparently stationary at its inner border. These ulcers were imbedded in a diseased mass of skin and cellular tissue, and the raised condition of the bone shewed from whence they had originated. The history of the case is as follows. About ten years ago this gentleman was the subject of primary syphilitic disease, for which he took a considerable quantity of mercury, without, however, producing any effect upon the mouth. The sore healed slowly, and he believed that during the time he was taking mercury, having been much exposed to inclement weather, he caught a severe cold. About six years ago he began to suffer from nocturnal pains in the head and limbs, often of an aggravated character. About two years and a half ago he first perceived a hard swelling on the right shin bone, which was not attended with much pain, though the nightly sufferings in the limbs still continued. Having consulted one of our most eminent city sur-

geons, iodine and taraxicum were prescribed with marked benefit, and were had recourse to from time to time whenever the pains became severe. About a year ago a small soft swelling formed over the centre of the node, which soon broke, and became a troublesome sore, slowly extending both in depth and circumference. The same surgeon was again consulted, and strongly advised rest and some soothing application, for which the water dressing was subsequently substituted. Complete rest was now tried for six weeks, with some abatement of the pain, but without any material change in the sore. Being unable to absent himself longer, he again resumed his accustomed avocation, at the expense of severe suffering and aggravation of the ulcer. About five months ago the other wounds formed spontaneously, and had continued slowly to spread until the time I first saw him; when his sufferings may be said to have reached their acme, and it was a marvel how he continued to crawl about. His appetite was good—his days were made up of a fearful struggle with his malady—and his nights were sleepless; and nothing but indomitable courage kept him up. The only mitigating point was, that since his leg had ulcerated, his former pains in the limbs had entirely left him. Upon the strength of the two cases I have just related, and some few others I have met with, I ventured to promise him a favourable and speedy termination to his sufferings. He was using an opiate ointment, over which was placed a piece of moist spongio-piline; neither of which applications are, in my opinion, suitable for open sores. I at first ordered a lotion of a weak solution to the chloride of lime in distilled water, with the view of cleansing the wounds. This caused considerable smarting pain, but accomplished the desired object. I then commenced the water dressing, in the way I have before described, applying, at the same time, black wash to the wounds. This latter caused such intense suffering that I was compelled to relinquish it. The wounds slightly improved, but bled whenever the water dressing was removed, and were highly irritable. In ten days after the commencement of my treatment I applied strapping to the entire limb, renewing the black wash, as I was convinced, with this addition, it could be borne. I may say, from that hour, all pain ceased, and the wounds healed with a rapidity that quite astonished me; the patient's characteristic expression was, "that he was in heaven," and as much of happiness depends upon the force of contrast, this was hardly an exaggeration. I allowed him to take gentle exercise, and at the end of six weeks, from the time that I took the case in hand, all the wounds were healed. There were two slight underminings, one of which I got rid of by making pressure over it with a little pad of lint, the other by thrusting in the black wash. It was these sinuses that delayed the case—the two semilunar wounds having healed within three weeks; and I may remark, that I have before observed, that specific ulcers, when once in a healing condition, cicatrize far more rapidly than common sores. I still continue the constitutional treatment.

The first important point that seems to me to be fairly deducible from these three cases is, that the ulceration that supervenes upon syphilitic periostitis, is frequently not an extension but a *translation* of the disease from the one texture to the other; this point has an evident bearing upon the prognosis of these affections, and exerts an influence upon the question of treatment, in reference to the propriety of attempting a cure of these sores. If the disease had continued in the periosteum in the cases I have related, I cannot conceive that the ulcers would so readily have yielded to local means, or that the cure would have been premanent. Secondly, it seems evident that these specific sores, when they become located in the lower limbs, have no inherent tendency to heal; but on the contrary, if left to themselves

gradually spread, multiply, and involve contiguous textures, and seem to have no definite limits in regard to their duration, number, or size. The third inference that I deduce from these cases is, that in specific ulceration of the lower limbs, rest, though it may be prolonged for months, exerts little or no beneficial influence; and even when combined with a good local application, such as the black wash, will not effect a cure, as is seen in the first case I have related. The fourth point that seems to me to be clearly proved is, the paramount value of properly applied mechanical support. I have, in a former publication, endeavoured to show that it is a most valuable substitute for rest; but here it accomplishes far more, and effects that which rest could not—it gives immediate freedom from pain, produces a healthy surface, cicatrizes the wounds, and exerts so powerful and salutary an influence over the limb, that in the second case, where the constitutional taint was still strong and active, the disease was compelled to break new ground, and show itself in another part of the body, being unable to re-establish itself in a part in which the circulation is maintained in a healthy condition by mechanical support. Fifthly, these cases, and many others I have met with, shows the great value of black wash as an application to secondary specific sores, and the great importance of getting it thoroughly in contact with the entire surface. And, lastly, they would confirm, if such were needed, the salutary constitutional influence of the iodide of potassium in the treatment of syphilitic periostitis. Whether or not the inferences I have ventured to draw be thought by the profession to be legitimate, I trust, at least, that the cases themselves, as exhibiting one of the numerous phases of surgical disease, will possess some interest.

FINSBURY SQUARE, LONDON,
March 1849.

Part Second.

REVIEWS.

On the Treatment of Ulcers of the Leg without Confinement, with an Inquiry into the Best Mode of effecting the Permanent Cure of Varicose Veins. By HENRY T. CHAPMAN, F.R.C.S., &c. &c. London: Churchill. 1848. Pp. 156.

A Treatise on the Cure of Ulcers by Fumigation. By GEORGE ALFRED WALKER, Surgeon, &c. &c. London: Longman and Co., and Highley. 1847. Pp. 112.

THERE is perhaps no subject of more general interest to surgeons, than the treatment of ulcers; for the simple reason, that the disease

is so common, and also very frequently intractable and tedious in its progress towards a cure; and notwithstanding our advance in the general principles of surgical pathology, it is only too true that the treatment of ulcers is still to a great extent empirical; most surgeons have some favourite remedy or set of remedies which they apply, without very deeply considering their *modus operandi*.

The different treatises which have been published in this country, may be divided into two classes; those which embrace a general view of the pathology of the different kinds of ulcers, and point out the various methods of treatment best adapted to each; and those which have had for their object to bring into notice some particular remedy or plan of treatment, which the authors, if they do not vaunt it as alone truly specific, at least hold it forth as being of far greater importance than other points of treatment which they consider merely subsidiary. Although the former plan, when properly carried out by the combination of practical experience and scientific investigation, must always be of a higher order, and of more general service; yet the latter has also been eminently useful, by drawing the special attention of the profession to particular remedies, and to a close investigation of their therapeutic action, so that although the originator of each plan may have placed too high a value on his own particular system, or advocated its use too exclusively, yet these remedies, when subsequently and more cautiously investigated, and applied with due regard to general principles and to suitable cases, have ultimately come to form valuable parts of our present resources.

We have been led to these remarks, from a perusal of the two works which form the subject of the present notice, and which afford examples of both the classes alluded to.

The work of Mr Chapman may be taken as a systematic treatise on ulcers of the lower extremities; and gives a concise statement of the principal opinions on the pathology of ulcers, and a review of the principal plans proposed (he would be a bold man who would attempt to give *all*), for treating the various kinds of ulcers; thus giving a very succinct, but comprehensive view of the present state of our knowledge on the subject. Mr Chapman states, that his treatise was originally intended to form part of the Cyclopædia of Surgery, and was laid aside when that work was discontinued; but his subsequent experience in the treatment of ulcers having confirmed his previous views, he has now been induced to publish the treatise in its present form.

The peculiar characters of ulcers of the leg, and the sources of their intractibility are first discussed; the various methods of treatment, constitutional and local, are next passed in review, and this part of the work contains a great amount of practical information conveyed in a plain and concise form. Mr Chapman then proceeds to consider more fully the rationale of the local treatment which in his own practice he has found most useful, namely, the water-dressing, combined with properly regulated support by means of bandag-

ing. He points out the necessity of regulating the temperature of the water according to the condition of the ulcer, and divides it into two kinds, cold and tepid dressings; the former ranging from 40° to 70° Fahr., the latter from 70° to 97° Fahr., and gives at length his views as to the therapeutic action of these applications.

What we consider as perhaps not the least valuable part of his work, is the minuteness with which he enters into the mode of applying his dressings, judging, as we think rightly, that nothing is too trivial in such descriptions, for it is often upon care in apparently trifling points in the dressing, that success in treating ulcers depends. His method of applying the water dressing differs from Dr Macartney's plan, or of that generally adopted.

"Instead of lint, therefore, in such circumstances, I make use of soft sponge, torn up into very small shreds, and soaked in water; these are dropped lightly into the ulcer, and covered with a single layer of lint, over which the bandage is carried, as in shallow sores; gentle support being thus conveyed to the entire surface, and tone communicated to the minute vessels, granulations spring up uniformly and vigorously, and fill the hollow of the ulcer, often with surprising rapidity. The sponge acts, as far as its compressing power is concerned, on the same principle as the wax dressing poured into deep ulcers, in the manner suggested by Mr Stafford; according to my experience, however, it is not only a more convenient mode of effecting the same object, but accomplishes it more speedily and completely. The shreds of sponge should be well soaked, and lightly distributed, in order to avoid any ill consequence from too much pressure by their subsequent expansion. Under this simple plan of treatment, I am satisfied that the granulation and cicatrization, in a large majority of cases of indolent ulcers of the leg of long standing, even when attended with a high degree of irritability, will proceed more favourably and expeditiously, and occasion less inconvenience to the patient, than under any other method whatever. Having expressed myself thus strongly in its favour, it must not be inferred that I regard other measures as entirely superfluous. I have already entered my protest against all such exclusive views."

Our limits prevent us from noticing our author's further remarks on the treatment of Irritable Sloughing, and Varicose ulcers, but we trust we have already said enough to induce the profession to peruse the work for themselves.

Mr Walker's treatise has for its object the recommendation of a plan of applying the fumes of sulphur and iodine in combination to ulcers; it is true, he also commences with a general view of the subject of ulceration and its treatment, but the evident and avowed aim of the work is to bring under the notice of the profession this particular remedy, as almost, if not entirely, specific.

Mr Walker seems to claim originality, principally as to the special kind of fumigation here referred to; for, while he admits that fumigations with sulphur had been previously in use, that iodine may also have been recommended, and that the advantages of the increased temperature involved in this plan of treatment have been long admitted in the cure of certain ulcers, he claims the credit of being the first to propose and carry into execution the methodical application of these agents conjointly to the treatment and cure of habitual

ulcer. We quote his own account of the effects of his plan of fumigation.

"In conclusion, and briefly to sum up the principal effects of fumigation on habitual ulcers of the lower extremity, I may state, that by this method the tendency to ulcerative absorption is rapidly changed by a powerful impression made on the vitality of the affected part—in other words, the "bad habit" of the disease is broken. The sensibility of the sore and of the limb is advantageously modified, as the diminution and quick subsidence of all pain demonstrates. The vessels of the part are as rapidly acted on, and, instead of secreting pus, are stimulated to throw out a plastic secretion, which at once serves as a defence to the sore, and a nidus for the formation of healthy reparative tissue. The inflammatory action, more or less of which constantly attends every species of ulcerative absorption, is soon subdued; and instead of the red, livid, and angry circle which surrounds the sore, we speedily obtain the dull whitish margin, indicative of incipient cicatrization. The callous deposit on the edges of the ulcer—the irregular and morbid effusions which constitute its base—the yellow infiltration which fills or disorganizes the cellular tissue in the vicinity of the ulcer, and frequently that of the whole limb—all these are removed by the absorption which fumigation excites.

"This removal of morbid deposits not only enables nature to proceed more quickly to the completion of the reparative process, but renders the cure more permanent; for the tendency to relapse under the old methods of treatment depends on the cicatrization having taken place over 'unsound flesh.'

"The cure, therefore, is not only quick but lasting. In ordinary cases the treatment is conducted without the necessity of confining the patient to the house, or even to the horizontal position; and it is of a nature which involves neither suffering nor discomfort—neither loss of time to the patient, nor risk of reputation to the surgeon."

Having had no experience in this particular plan of treatment, we do not feel at liberty to criticise it further than to remark, that, judging from the author's statements, and from what we know of the action of similar fumigations in causing absorption of morbid products in certain cases—as, for example, in the simple chronic enlargement of the testicle, and in some skin diseases—it appears that this plan must act as a diffusible and easily regulated stimulant application to the surface of the sore and the neighbouring parts, and, therefore, likely to be of service in chronic or indolent ulcers with callous edges, and induration of the surrounding skin and subcutaneous cellular tissue; but we can scarcely think it so applicable, however modified, to all forms of ulcer, as some expressions of our author would seem to intimate: indeed, from Mr Walker's own admissions, it would not be difficult to show that in the irritable and some other forms of ulcer, the means he uses as preparatory to the fumigations, must act by bringing these ulcers into the condition of an indolent sore before the fumigations can be used with propriety. Still we doubt not that the plan will be found valuable when used in accordance with general principles, and limited to proper cases.

Some expressions in the work before us are rather unfortunate, and unless a calm and full perusal were made of the whole work, might leave the disagreeable impression that Mr Walker wished to

make the vapour cure of ulcers a special practice. One of these expressions occurs at p. 64, where he says:—"The apparatus in itself is extremely simple. It is composed of a series of mahogany boxes, having in the cover of each a round aperture, for the reception of the limb. Each box communicates below with the *engine-room of my establishment.*"—(The *italics* are ours.) This statement, when we first read it, called up to our imagination an ulcer-fumigation establishment, on the same principles as a hydropathic establishment, and a number of patients incarcerated in these surgical stocks, undergoing the *vapour cure.* Now, there was no need for Mr Walker to specify his own establishment in speaking of the mode of applying the general remedy he is proposing, and in so doing he may lead to misapprehension; for though those who, like ourselves, make a point of giving the work a full and fair perusal will find that he subsequently points out a very simple method of applying the vapour at p. 73, without the necessity of having *an engine-room or special establishment,* others may think they have read quite enough of the work; and we shall be much mistaken if some of our contemporaries, misinterpreting such expressions as that alluded to, do not treat the work more cavalierly than it really deserves; for we do think it well worthy of attention, and we accept Mr Walker's statement, "that in earnestly recommending it to notice, he feels that he merely fulfils a duty which every medical man owes to the profession that he cultivates."

We sympathize with the author in his regrets as to the plates, which, owing to some fault of the paper they are printed on, do not admit of being coloured; for in their present state they must have added much to the expense of getting up the work, which they rather injure than illustrate. J. S * * * * *.

Elements of Anatomy. By JONES QUAIN, M.D. Fifth Edition. Edited by RICHARD QUAIN, F.R.S., and WILLIAM SHARPEY, M.D., F.R.S., Professor of Anatomy and Physiology in University College, London. Part III. 8vo. London: 1848.

Demonstrations of Anatomy; being a Guide to the Knowledge of the Human Body by Dissection. By GEORGE VINER ELLIS, Junior Professor of Anatomy in University College, London. Second Edition, Re-written. Part I. Pp. 464. London: 1849.

As we purpose reviewing the first of these works at some length in an early Number, we mention it now merely to congratulate our readers, and especially students of anatomy, on its completion. It is indeed a work calculated to make an era in anatomical study, by placing before the student every department of his science, with a view to the relative importance of each; and so skilfully have the

different parts been interwoven, that no one who makes this work the basis of his studies will hereafter have any excuse for neglecting or undervaluing any important particulars connected with the structure of the human frame; and whether the bias of his mind lead him in a more especial manner to surgery, physic, or physiology, he will find here a work at once so comprehensive and practical as to defend him from exclusiveness on the one hand, and pedantry on the other.

Mr Ellis' work has been well known to anatomists for the last eight years, during which it has continued without a rival as a text book for the dissecting-room. He is the only one who has lately undertaken the laborious task of writing a complete system of regional or relative anatomy, or a *dissector*, properly so called; for although there are several works denominated "dissectors," the arrangement of them is quite that of works on systematic anatomy. With such books the student is at a loss. Taking them as his guide in his practical labours, he will first examine the muscles of a limb or region, and then turn to another part of the book, or to another volume, in search of an account of the vessels, and to another for the nerves; and, as neither description has reference to the other, he finishes his "part" without his attention having been directed to the relations which the different systems of structures bear to each other. What was wanted was a book which would serve as a guide to the student in the dissecting-room, so that he would find in the pages before him, an account of all the parts which could be seen in the region with which he is engaged; and this deficiency the work of Mr Ellis amply supplied.

The plan necessarily involved occasional repetition and breaking up of systematic description; but such a work is to be regarded, not as the only one which the anatomical student is to employ, but only as a companion to one of the text books of systematic anatomy, with which he begins, and follows his teacher. As such we have strongly recommended Mr Ellis' book to those who had the desire to become good practical anatomists; and we have frequently noticed of those who used this book, a great superiority in accurate knowledge of relation; and have felt how much more instruction is required in the dissecting-room by those who use any other work there.

Mr Ellis' work contains an accurate and plain account of the anatomy of the different structures as they lie together in each region, and no relation of importance is allowed to escape the notice of the dissector who uses it. His description of the surgical regions, although practical references are by no means too frequently made, we esteem more highly than those in some books which treat only of these regions; as, for example, the groin in connexion with hernia; and a number of excellent remarks are introduced at the beginning of each part, under the head of "surface markings."

In this new edition great improvements have been made. Unimportant details have been omitted, and the description of important

parts shortened without diminishing their value. Advantage has been taken of recent advances in physiological anatomy, to improve the description of some parts; for example, the brain and its nerves, the anatomical account of which is too often not guided by what is known of their relative physiological importance. Also, alterations have been made in the arrangement, and different types are employed for the parts of greater or lesser importance, and for the directions for dissection, by which the work is rendered much more readable and more easy of reference.

We regret, however, to observe, that the account of the microscopical anatomy of the organs is not omitted in the new edition, so far as it has gone. Not that we have any fault to find with the execution of this department, but surely in a dissecting-room guide, microscopical anatomy is out of place, and nowhere can it be understood by the student unless illustrated by sketches. Descriptions of minute anatomy now abound. The student finds them here in his dissecting-room guide, again in his text book of systematic anatomy, and yet again in his text book of physiology; and in the first of these we think them as much out of place as a description of the parts of hernia would be in the last.

In all other respects the work is most satisfactory, and is a vast improvement on previous works of similar extent. With the assistance of such a book, now that we do not lack bodies for dissection, every one has it within his power to become a good practical anatomist. By it Mr Ellis has established for himself a reputation as a most careful dissector, and most accurate anatomist; and we sincerely trust, that this now improved work will be seen still more frequently in the hands of the student.

The Closing Years of Dean Swift's Life; with an Appendix containing several of his Poems, hitherto unpublished, and some remarks on Stella. By W. R. WILDE, M.R.I.A., F.R.C.S., &c. Dublin: 1848. 8vo.

THE greater part of this little volume lies beyond the scope of our critical province; but as it contains the substance of two articles in the Dublin Quarterly Journal of Medical Science, on the disease which carried off one of the most remarkable men of our country, and as it is the work of so justly distinguished a member of our profession as Mr Wilde, we think that some notice of it may be agreeable to our readers.

We understand that this volume owes its origin to a medical question, raised by Mr Mackenzie of Glasgow, in a letter to Mr Wilde, in regard to the nature of a disease of the eye, under which Swift laboured in the last years of his life. Although on this particular question comparatively little information has been gained, it has led to a most interesting inquiry into other obscure matters.

The first portion of the work is accordingly occupied by a detailed account of the latter years of Dean Swift's life, especially of the facts which bear upon the question of the symptoms of disease under which he laboured. It is well-known, that this great wit is said to have ended his days in insanity; and it has been a current rumour, that he was the first occupant of the institution which he himself endowed, for the cure of the insane. It appears quite certain, however, from Mr Wilde's account, that the Dean did not at any period display symptoms of unequivocal insanity; at the same time there is considerable reason to suppose, that he suffered for more than thirty years of his life under a chronic head affection, which has not only left its traces in various parts of his journals and correspondence, but appears to have produced material changes in the external appearances of his brain. This affection shewed itself at first in the form of giddiness, coming on in paroxysms, and appears to have early impressed him with the idea, that it was likely to be ultimately fatal to him.

Mr Wilde seems to think there is also evidence of one or more attacks of hemiplegia; and it is certain that Swift complains, in his journal to Stella, of weakness of the *left* hand. Whatever was the character of the disorder, however, it appears to have haunted him more or less during all the latter part of his life; and in his old age to have rendered him irritable and morose, and thus embittered, to a considerable extent, his declining years. There appears, latterly, to have been total loss of memory, and extreme difficulty of articulating and finding words; but there is no evidence of any hallucination or delusion, such as is found in the insane.

In the course of the year 1835, it became necessary to make some repairs in St Patrick's Cathedral, which involved the temporary removal of the remains of Swift and the celebrated Stella. The crania were accordingly brought to Dublin, and made the subject of much investigation by the train of phrenologists, who accompanied the British Association of Science to Dublin for that year. The reader will be able to fancy the extent of mortification and surprise of the cultivators of that convenient art of reading character, at finding that the organs of *wit*, causality, and comparison, were scarcely developed at all, that the forehead was extremely low, and that the head only rose from benevolence backwards, being excessively developed at the seat of the animal propensities, especially amativeness and philoprogenitiveness.

It was scarcely possible for any "development" to have been more unfortunate for the phrenologists than this. We know that the genuine devotee of that "*ci-devant science*," as Mr Wilde calls it, can never be imprisoned in an absurdity without some loop-hole of escape being ready for him; but we suspect it will be as difficult to convince the generality of mankind that Swift (whose extreme coldness of temperament is admitted by his most partial biographers) was strongly endowed with amativeness or philoprogeni-

tiveness, as that he was *not* a witty or intellectual man.—To us it appears, notwithstanding Mr Wilde's charitable and patriotic construction of his character in the work before us, that a piercing clear intellect, backed by consummate wit, and selfishness unsubdued by any strong passions or feelings, religious, moral or animal, formed the characteristics of Swift; and although he has been as much and as enthusiastically and perhaps as wrongly vilified on the one hand as he has been vindicated on the other, we think the verdict of mankind will go with us.

We cannot enter at length into the phrenological questions connected with the consideration of Swift's head. To those who wish to follow out the investigation, we would recommend a perusal of the article by Dr Skae in the British Quarterly Review, volume 4, and the controversy which arose out of it in the Lancet for 1846. The inconsistencies discovered by Dr Houston are endless; suffice it as a specimen, that the anterior lobes of the brain were phrenologically small, but really large; while the cerebellum, which was apparently very large, was really, from the low position of the tentorium, relatively small. Neither can we venture to trust ourselves with any remarks on the literary questions in the work, seeing that our space is so short; we can say, however, that the latter half of the volume is entirely occupied with literary and antiquarian researches, and that we have perused the whole with equal pleasure and profit. We congratulate Mr Wilde on the mode in which he has executed his task; and we hope that he will continue to render the moments which he steals from his more strictly professional avocations for objects like the present, as valuable to the public as in this instance.

Pathology of the Human Eye. By JOHN DALRYMPLE, F.R.C.S. London: 1849. Fasciculus I. Imp. 4to., 4 plates.

THIS is the first part of a work on the pathology of the eye, by Mr Dalrymple, somewhat on the plan of the great work on the same subject, by Von Ammon of Dresden.

Mr Dalrymple, in his introduction, says, that "it is not within his meaning to publish *a treatise* on diseases of the eye, but rather to exhibit them as they occur in nature, in a series of drawings, with such explanations as shall identify them with symptoms, and with the general treatment of the case." He intends to give, first, the diseases of the appendages of the eye, then the diseases of the eye proper, and conclude with the malignant diseases. The work is to consist of nine parts, the first of which is now before us; it contains four plates, illustrating some of the affections of the eyelids; each plate containing six drawings, accompanied by a short description of the disease and its appropriate treatment.

As we intend to review this work at some length when it is farther advanced, we shall confine ourselves at present to calling the attention of our readers to it. It promises to be truly a most magnificent work, but only such as we should have expected from its accomplished author;—few having enjoyed the advantages which Mr Dalrymple has of studying ophthalmic diseases, and still fewer having been able to turn them to such a good account. If the work be carried on (as Mr Churchill pledges himself that it will) in the same style in which it is begun, it will be by far the finest series of plates of eye diseases which exists in any language, with, perhaps, the exception of those contained in the *first* edition of Mr Wardrop's work on the Morbid Anatomy of the Eye, certainly much finer than those in the second edition. The drawings are executed in Mr Bagg's usual admirable style, possessing not only a high artistic finish, but being graphic illustrations of the diseases. They are beautifully coloured by Mr Sherwin. The paper and the printing do great credit to Mr Churchill's taste. In short, we do not recollect any medical book which has issued from the English press which possesses greater beauties both for the mind and for the eye, than this one does; it is one which ought to be, not only in every public library, but also on the shelves of all who admire and value a good and beautiful book, and who wish to be able to recognise, and so treat with satisfaction to themselves and benefit to their patients, the manifold diseases of this very important organ.

The Elements of Botany. By M. Adrien de Jussieu. Translated by James Hewetson Wilson, F.L.S., &c. 8vo. London: 1849. Pp. 750. Numerous Woodcuts.

The best introduction to botany in any language, full of good sense, clear description, and careful setting forth of the essential points of the science. The English translation is excellently done, and is highly creditable to Mr Wilson, who has also executed with success his duties as an editor, and in that capacity, made a few useful additions. The volume is compact, well printed, and adapted for use in the class-room. The original cuts of the French edition are used.

Icones Carpologicæ; or Figures and Descriptions of Fruits and Seeds. By Thomas Shearman Ralph, A.L.S. 4to. London: 1849. Pp. 54. Forty Plates.

This volume will be welcomed by botanists as a valuable contribution to the iconography of their science. No similar separate publication has appeared since the great work of Gaertner, although the

want of illustrations of fruits and seeds commensurate with the rapid progress of botanical research has been greatly felt. In the present volume of the *Icones Carpologicæ*—for we hope this is but the first of a long series—200 genera of Leguminosæ are illustrated by original or well selected figures in lithography. A little more firmness and delicacy of treatment are to be desired in the drawings, but as the author advances, no doubt his handling of the lithographic chalk will improve. The work should have a place in the library of every botanist and horticulturist.

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVIII.

MEETING VI.—*February*, 21, 1849.—JAMES SYME, Esq., President, in the Chair.

(Continued from P. 626.)

CONGENITAL MALFORMATION OF BOTH WRISTS.

Dr A. Wood exhibited a child of fourteen days old, presenting an unusual malformation of both wrists and hands. The thumbs were wanting, the other bones of the hands (both metacarpal and digital), were longer than natural; and the flexor tendons of the wrist were contracted, so as to cause great deformity.

PERFORATING ULCER OF THE STOMACH.

Mr Miller gave a short account of a case of perforation of the stomach, which he had seen in consultation with Mr Hill of Portobello, and exhibited a preparation of the morbid parts. The patient having been long dyspeptic, was in the habit of using soda and magnesia as antacids, in large quantities; and the perforation seemed to have taken place immediately after a dose of bicarb. potass had been swallowed. The preparation showed a single chronic ulcer on the anterior surface of the stomach, two inches below the cardia, some thickening of the mucous or submucous tissues in the vicinity of the ulcer, and a sharply defined round perforation of the peritoneal tunic which would barely admit a goose quill. The patient died within 24 hours after the first symptoms of perforation,—and for a large portion of that space of time had been nearly free from pain. Mr Miller was of opinion that the ulcer had gradually thinned the peritoneal coat to fine attenuation; and that, in consequence of the rapid extrication of gaseous matter which the antacid had produced, the stomach, suddenly distended, had given way. He considered the peculiarities of the case to be these: induction of the fatal crisis, by sudden distension—pointing to obvious prophylactic means; and the tendency to obscuration of the case, caused by marked and almost complete cessation of pain, long before death.

INTRA-UTERINE SMALL-POX.

Dr Simpson shewed drawings of two cases of variola in the fœtus, which he had met with during the last month. Both children were born dead. The

first case occurred in the practice of Dr Purdie. The child was near the full time. The variolous eruption was scattered freely over the whole cutaneous surface in the form of pustules. Several of the pustules were umbilicated. The mother at the time of the birth of the child was labouring under an attack of modified small-pox.

In the second case (occurring in the practice of Dr Gordon), the fœtus was expelled about the fifth month. The pustules were principally confined to the trunk of the fœtus, and consisted chiefly of flattened collections of matter. The mother was suffering under modified small-pox.

Dr S. alluded to some other analogous cases with the history of which he was acquainted; and stated, that intra-uterine small pox had now been observed by Mead, Jenner, Hossack, and others, under the following circumstances, viz :—

I. In some cases the fœtus was affected with small-pox, while the mother was suffering under the modified form of the disease, as in the above cases. II. Both mother and fœtus sometimes suffered cotemporaneously under the natural small-pox. And III. In some instances in which the mother was exposed to the contagion of small-pox, but herself escaped an attack of the disease, in consequence of previous variolation or inoculation, the fœtus was nevertheless born affected with it. In these last instances the fœtus in utero being attacked by a disease, to the contagion of which the mother was exposed and yet herself escaped; it was evident, 1st, that we had in this a proof that a morbid contagious matter may be inhaled and introduced into the system without the disease following in the system of that individual (the predisposition being wanting); and 2d, that this morbid matter may nevertheless pass into and produce the specific disease in the fœtus in whom the predisposition to it was present; and 3d, that the morbid matter could only pass from the mother to the fœtus through the medium of the circulation, for there was no communication by the nervous system between the economy of the mother and that of the fœtus; and 4th, that this afforded sure presumptive proof that this as well as perhaps other contagious febrile matters affected the body by first entering the vascular system.

Dr S. alluded to some experiments in which he had vaccinated the mother during pregnancy, but without apparently affecting the fœtus.

Some conversation followed on the prevalence of small-pox, and the neglect of vaccination as the cause of that prevalence. In reference to it, *Dr Simpson* observed, that calculating from the number of deaths by small-pox recorded in England, probably 8,000 or 10,000 persons died every year of variola in Great Britain, or about one human being every hour. He further observed, that he believed that this number of lives would be betimes saved annually if the legislature would adopt a means of ensuring vaccination *in all*. And he argued it would require no expenditure or new machinery to do so. All that was necessary was to enact that besides the registration of the *birth* of every child being demanded by law (as it was at present in England, and would soon be in Scotland), the registration of the *vaccination* of it should be equally demanded by law within six months, or any other specified time after its birth.

INVOLUNTARY GRINDING OF THE TEETH.

Dr Spittal communicated the case of a man, namely, a fine negro, formerly a mariner, and aged eighty-eight years. He was first seen by Dr S. on 2d February 1848, and laboured under bronchitis to a slight extent, together with other ailments usually accompanying approaching dissolution from old age, under which he seemed chiefly to sink. He died eight days subsequent to the above date.

On visiting him, Dr S. was struck with a remarkably loud intermitting rasping noise, not unlike the croaking of a young raven; and not knowing whence the sound proceeded, Dr S. enquired if any such bird was in the room, when he

was informed that it was caused by the patient grinding his teeth. This, it was said, he could not avoid when awake, unless he forcibly kept his teeth asunder. To avoid the fatigue of this voluntary effort, he was in the habit of interposing different substances between the teeth. Sometimes the edge of the bed clothes, at other times small flat or wedged shaped portions of wood, or a small bone button depressed in its centre, which he placed between the canine teeth. These latter substances were placed on the left side of the mouth. The teeth were generally very much worn down and flattened on their surfaces.

Dr S. was informed that this habit had existed, more or less, for about sixteen years, and was said to have commenced with pains like those of rheumatism in the head. It was also stated, that a few years ago the patient had passed a large round worm, after which the grinding had diminished, but still continued. He has since been occasionally effected with abdominal uneasiness, but has passed no more worms. Bowels said to have generally been constipated. Dr S. stated, that as this was an affection of a peculiar nature, and such as he had never before met with or heard of, he had thought it might add to the interest of these meetings to lay it before the Society.

Dr A. Wood, who had also known Dr Spittal's patient, thought the affection a simulated one ; he believed that the patient had made this apparent infirmity the source of considerable profit.

Dr Spittal did not consider that the circumstances of the case warranted the conclusion of Dr Wood ; and in particular stated, that the man was evidently labouring under considerable suffering, and as before remarked, died in a week after his being first seen.

Dr Seller remarked, that there could be no doubt that a convulsive action of the muscles of the jaw occasionally took place ; and stated that he had seen a girl in whom this followed acupuncture of the chin. A loud uneasy grinding of the teeth was the consequence ; to remedy which she used to insert the steel busk of her stays, and pull the jaw forcibly down.

Mr Syme said, that he had not witnessed any case of the kind recorded by Dr Spittal, but he had seen several cases in which the patients were much annoyed by a cracking sound, proceeding from the articulation of the lower jaw. This affection did not appear to depend upon loss of the teeth, as in one instance it had occurred in a young lady, whose case of teeth was tolerably entire.

APOPLEXY OF THE LUNG AND ITS RESULTS.

Dr W. T. Gairdner exhibited drawings illustrative of the morbid appearances in the lungs of a girl æt. 13, who died in Dr Paterson's ward of the Hospital, from pulmonary disease supervening on contraction of the mitral orifice of the heart. Both lungs were irregularly condensed ; the dense portions occurring in many parts as distinct nodules in the midst of crepitating tissue. These nodules, when cut into, presented two distinct appearances. Some of them were of a deep nearly uniform blood colour, and yielded a bloody fluid on pressure ; in short, had all the characters of the *apoplexy of the lung*, as described by Laennec. In others this colour was intermingled with abrupt patches and points of light yellowish grey or stone grey, and in some this grey colour prevailed, so as to form nearly the entire nodule. The grey coloured portions were very like some forms of the grey hepatization, and the more irregularly condensed parts of the lung were of similar colour and appearance. The grey matter was also softer than the red, being at a few points almost diffluent. Microscopically examined, the fluid from both the grey and red condensation contained epithelium and granular cells, such as are commonly seen in hepatized lungs ; but in the red parts the granular cells and masses were infiltrated with more or less deep blood-colour, and were associated with very numerous blood-corpuscles ; whereas, in the grey parts the blood-corpuscles and infiltrated colouring matter were absent, and free granules with imperfect cells and nuclei were present in a much larger proportion than in the other.

These observations appeared to Dr G. to illustrate the process of resolution of hemorrhagic exudations in the lung. He believed that in this, as in all other exudations, one of the first changes was the precipitation of the oleaginous constituents of the blood in connexion with albumen in the form of granules and molecules, which, as Reinhardt had shown, accumulated within the epithelium cells of the organ, so as to form granular corpuscles. In the hemorrhagic form of exudation, where the blood-corpuscles were extravasated along with the other elements, the colouring matter contained in these gradually diffused itself, so as to tinge the granular cells and masses of every variety of shade, from a pale rust-colour to an intense purple. In this state the exudation presented the ordinary appearance of recent apoplexy of the lung. By a secondary process, however, new fluid matters were poured from the capillaries into the occluded air-vesicles, and these were partly expectorated, carrying with them the soluble *hematin* or colouring matter, and partly remained within the vesicles, giving rise to the grey form of condensation, in which blood-corpuscles were absent, and granular elements present in large proportion. This change appeared to commence in particular lobules in the apoplectic nodules, and thus to give rise to a great variety of colour. Ultimately, no doubt, this secondary exudation might be absorbed or entirely expectorated. That these changes had been going on in the case before the society, was proved by the copious expectoration, during life, of prune-juice coloured sputa, a variety of expectoration known to depend on the admixture of fluid exudation, with a large proportion of the colouring matter of the blood.

But the case was further interesting as showing the ulterior disorganizations resulting from hemorrhagic exudation. At one part the drawings illustrated the breaking up of the grey matter into gangrene, forming a cavity subjacent to the pleura, and filled with dark green softened tissue which had the characteristic fetid odour. At another point there had formed a dense mahogany coloured mass, in which there were found, on microscopic examination, angular bodies of a deep purple colour, verging into black, and having an almost crystalline transparency and lustre by transmitted light. (Drawings of these were shown to the society.) These bodies were similar to what are commonly found in the orange-coloured pigment which lines old apoplectic cysts in the brain, and had been described by Virchow also in other situations where extravasation of blood occurs. They are considered by Virchow to be altered hematin.—(See Archiv für Path. Anat. Vol. 1., Pl. 2.)

The last morbid change illustrated by the drawings laid before the Society was the formation within one of the lungs of a cavity which was perfectly empty, and lined by a thin membrane, having the smooth glistening appearance of mucous or serous membrane. This cavity, from its form, and the appearance of its lining membrane, had the greatest resemblance to the first form of dilated bronchi, described by Laennec. Its communication with the larger bronchi, however, was found with great difficulty, and was of a size which only admitted a small probe. A microscopic examination proved that the membrane lining this cavity, notwithstanding its resemblance to a mucous tissue, was not an epithelial membrane; and its true nature was also revealed by the observation, that the fluid scraped from it contained a number of the peculiar angular purple bodies above described; thus showing the cavity to have been formed in connexion with the hemorrhagic exudation, which had probably become broken up and expectorated. Dr Gairdner said that he had seen abundant evidence, that false membranes, formed around cavities caused by ulceration, might assume all the appearance of mucous membrane; and he believed that the so-called dilated bronchi were often in reality cavities formed by ulceration or gangrene of the pulmonary tissue, and becoming afterwards lined by a membrane undistinguishable from that of the bronchi, into which they opened.

MEETING VII.—*March* 7, 1849.—DR GAIRDNER in the Chair.

After the reading of the minutes, *Dr Spittal* requested permission to make a short statement in reference to the case which he had the honour of laying before the Society at their last meeting, and said that in consequence of the doubt expressed in regard to the reality of the affection—in which he believed many of the members participated—he thought it due to the Society as well as himself to make it. No member seemed to have met with or been acquainted with any similar case. Dr S.'s attention, however, has since been called to several cases in Dr Graves' Clinical Lectures (P. 465, &c., 2d Ed., Vol. I. : Dublin, 1848.) The affection is described as "*an insuperable desire on the part of the patient to grind his teeth*,"—originating in a "disagreeable uneasy sensation in the teeth themselves, and is for the moment alleviated by forcibly grinding them together, but immediately returns when the patient ceases to perform this action, which is therefore continued when the disease is confirmed, during the entire day. When asleep the patients no longer grind their teeth, the grinding in all cases the result of voluntary motion."

1. The first case is that of the Countess of Egmont, in whom the grinding was so confirmed as to impel her to indulge in it continually, for when she desisted the uneasy sensation in the teeth became insupportable, and consequently she had to give up all society for several years before her death. Grinding strong and forcible—teeth worn down to the very socket.

2. The Rev. Mr B. was driven from society by the same affection. Teeth much ground down. Incisors and canine, *whetted* by each other, acquired chisel-shaped edges, which sometimes cut the tongue like a knife.

3. A young clergyman afflicted with *tic douloureux* of several branches of fifth pair, including the left dental nerves. Teeth of the left side only ground down. Disease ceased after continuing two years.

4. Henry W., æt. 60. Three years ago gradually got the habit of grinding his teeth, which he now does constantly when awake, and "*so loudly as to be heard in the next room.*" He is said to be unconscious of it unless when spoken to. Teeth ground down.

5. A gentleman, æt. 45, after a severe wetting, was attacked with paralysis of the right side. Recovered ; but about a year after observed in himself a tendency to grind his teeth, which gradually increased so as to prove a "*nuisance to himself and every one about him.*" Appeared to be relieved for six months by actual cautery applied behind one ear, the use of mercury, and extraction of one tooth. The affection returned as bad as ever, however, when iron and other things were tried in vain. *By pressing the tongue against the upper incisors, or by touching a certain part of one particular tooth*, he could at any time arrest the grinding and suspend it as long as the pressure was continued. Dr Graves is of opinion that the irritable state of the dental nerves, which gives rise to this *irresistible tendency to grind the teeth*, depends chiefly on the existence of gout in the constitution.

Dr Spittal trusted he had shown that such an affection as that under which his patient had laboured, was really founded in nature ; at the same time he did not mean to deny the possibility of the man—who was poor, and probably had found sympathy for, and perhaps profit from, his disease—having simulated the affection on fitting occasions. The circumstantial evidence, however, connected with the history of the last week of the man's existence, appeared altogether at variance with the idea that the affection was feigned on that occasion.

Dr Adam Warden read a paper on the effects of a descent in the diving-bell in improving the condition of a deaf patient in whom the affection depended on occlusion of the Eustachian tube from inflammation. The object of this paper was to show that the increased pressure of the atmosphere acted as a delicate wedge, and by forcing air into the tympanic cavity, opened up the parts, and

removed the obstruction in the tube. (This paper will be published in our next number.)

Mr Spence, in the absence of *Dr Taylor*, read an account of a case of ruptured bladder, which will be published in our next Number.

MEETING VIII.—*March* 21, 1849.—JAMES SYME, Esq., President, in the Chair.

TUMOUR OF THE CHEEK.—VARICOSE VEINS WITH PHLEBOLITES.

Mr Syme exhibited the preparation of an unusual form of tumour, and also a wax cast of it, made by Mr Cobbold. The tumour was removed from the cheek of a girl aged twenty-three, who about two years and a half before had received a blow in that situation. Soon afterwards she observed a slight enlargement, which, afterwards, from its size, became inconvenient, though not painful. Mr Syme removed the tumour, by making an incision through the mucous membrane of the cheek, and then pulling it out, which required no force. The mass thus removed was somewhat larger than a pigeon's egg, of an ash grey colour, irregular in shape, and enclosed within a delicate web of areolar tissue. It consisted mostly of tortuous veins, imbedded in a little fat, and was considered by Professor Goodsir, as a varicose condition of the venous plexus, which is situated between the muscles of the cheek, and communicates with the internal maxillary and deep facial veins. Mr Vander Byl, who also examined the tumour, observed that the veins were much thickened in their coats, and contained numerous phlebolites, varying in size, irregular in shape, and smaller than peas; and that these bodies consisted of a horny external portion, enclosing calcareous matter, which under the microscope presented beautiful rhomboidal plates of cholesterine, and masses of granular matter. Extensive erysipelas of the face and head had unfortunately proved fatal to this patient, some weeks after the operation.

NECROSIS OF THE FEMUR AFTER AMPUTATION IN THE LOWER THIRD OF THE THIGH, FOR WHICH AMPUTATION AT THE HIP-JOINT WAS PERFORMED.

Mr Syme exhibited a preparation of the upper two-thirds of the femur removed by the hip-joint operation on the 13th September last. The whole shaft of the bone was necrosed, and the exfoliation extended quite close to the joint, thus showing the necessity of the hip-joint operation. There was no tendency to separation, but a large quantity of new bone had formed round the necrosed shaft. The patient, a labourer aet. 24, was admitted on the 7th September 1848, having, a few hours before admission, received the following injuries, occasioned by a fall of earth while working in a railway tunnel. A very severe compound dislocation of the right ankle joint, with fracture of the tibia above the malleolus, and another fracture a little above the middle; the lower end of the fibula was exposed for several inches, and very much comminuted. There was severe laceration of the soft parts—great effusion of blood, accompanied by the admission of a considerable quantity of air beneath the integuments. The tibia and fibula of the left leg were fractured above the ankle—there was great eversion of the foot, with enormous swelling and ecchymosis of the whole limb; so much so, that it was difficult to say which limb was the most severely injured. Under these circumstances an attempt was made to save both. The exposed portion of the fibula was removed, and proper splints applied. For the first ten days everything went on most favourably—the patient continued in good spirits—reaction was moderate—the discharge from the wound was healthy in appearance, and moderate in quantity: and the swelling in the left leg almost entirely disappeared; but, after that, he was seized with bowel complaint—lost his appetite—his urine became scanty and high-coloured; he had a heavy dingy appearance, and his whole body assumed a yellowish colour; in short, he exhibited all the symptoms of phlebitis. These, however, disappeared in about a week, and his health im-

proved, but the discharge from the wound and from incisions made in different parts of the leg, increased to such an extent, that, as a last resource, though at the time it seemed quite a hopeless one, amputation was performed in the lower third of the thigh by the circular incision. Two circumstances worthy of notice occurred during the operation. The first of these was the greatly thickened state of the coats of the femoral vein; the other, the very dense and almost ivory appearance of the shaft of the femur. This was so remarkable, that several gentlemen who witnessed the operation declared that necrosis of the bone would in all probability ensue; and they were correct in their supposition. The wound at the end of three weeks was entirely healed, except at one small part, where the bone was felt to be bare; but the looked-for exfoliation did not become detached. Three months passed over, during which repeated attempts were made to ascertain if the dead bone would separate. The femur got thickened, and the patient suffered much pain, more especially over the trochanter and neck of the bone. Abscesses formed in all directions, till at length there was such a profuse discharge, that the patient's health became seriously affected, and it was evident that, unless something was done for his relief, he would very speedily sink under the disease. At a consultation, held on the 12th of this month, it was agreed that the only method that could be followed with this view was the very severe one of amputation at the hip-joint. This was accordingly performed on the following day by Mr Syme, by anterior and posterior flaps. There was very little blood lost during the operation, although the large number of twenty-one vessels required ligature.

The operation had been completely successful.

DILATATION OF URINARY PASSAGES FROM CALCULUS.

Mr Spence exhibited a preparation of the urinary organs, taken from a boy seven years of age. The bladder was very much enlarged, and its muscular coat greatly increased in thickness; in the prostatic part of the urethra there was slight enlargement of the verumontanum. The right kidney was converted into a large membranous cyst, which contained a calculus; the ureter of that side was tortuous and enormously dilated, being equal in diameter to the small intestine; the left ureter was also dilated, but in a lesser degree; its dilatation was greatest near the bladder, and it became much contracted near the pelvis of the left kidney, which was dilated; the left kidney itself was considerably enlarged, but its glandular substance, though altered in structure, had not suffered as in the right kidney. The vesical orifices of the ureters on both sides, were of their natural size. Both testicles were condensed and altered in structure, and each contained a small quantity of pus.

Mr Spence stated that he was first consulted in the case of this patient two months before his death, on account of pain and swelling in the testicles, which had then descended for the first time. On asking the parents if he complained of pain in making water, Mr S. was told, that about four years previously he had suffered from retention of urine, which once required to be relieved by the catheter, and that afterwards he had twice passed small calculi by the urethra; they further stated, that though thin and delicate, his appetite was good, that he had no complaint except pain in making water, and after having passed it, and occasionally incontinence of urine; they had never observed any blood in his urine, nor had he had swelling of the feet and legs, or pain in the back, except just before the time when he passed the small stones.

On examining a specimen of his morning urine, it was found of a pale straw-colour, with a thin cloud of mucus floating in it, had an acid reaction on litmus paper, and became clearer and of a darker colour on being heated; under the microscope it exhibited crystals of uric acid, some epithelial scales, mucus, and yellow lithate of ammonia. The parents were desired to return to have him sounded when the irritation of the testicles was subdued. Mr Spence heard no more of him till the evening of the 5th February, when his father brought him to have the urine drawn off; his medical attendant stated that the child

had suffered from retention, requiring the use of the catheter, that he had drawn off the water on the two preceding days; but that on the 5th he had found it impossible owing to the struggling of the child, and had therefore sent him to town; Mr Spence introduced the catheter whilst he was under the influence of chloroform, and drew off between three and four pints of water. For about a week before this attack of retention, his appetite and general health had begun to fail, and for the last few days he had taken no nourishment except a little milk; he was listless and sleepy; his pulse was 100 and small, his tongue a little white but not foul, and he had complained much of pain in making water before the retention came on. On the 7th February, after partially drawing off the urine, Mr Spence examined the bladder carefully with a sound, but could not detect any stone; a dose of castor oil was ordered which acted four times, and brought away a large lumbricus; he was not however relieved by any remedy used; the retention continued to require the use of the catheter twice a-day, and after the 7th February he fell into an almost comatose state, gradually sunk, and died on the morning of 9th February.

Mr Spence said, that there were two points to which he wished principally to direct attention in reference to this case. The first was, as to the cause of the disease; the increased capacity of the bladder, taken in connexion with its increased muscular power, the dilatation of the ureters and the cystic condition of the kidneys, were precisely similar to what is seen as the result of obstruction to the excretion of urine from stricture of the urethra. Yet, the history of the case shows, that the boy, until a week before his death, with the exception of one occasion, never had complete retention of urine, and there was no difficulty in introducing a No. 3 catheter, nor even a No. 4 sound.

The other point was, that the comparatively slight symptoms which seem to have accompanied such extensive organic disease, ought to serve as a practical caution to surgeons. Almost all the symptoms complained of, were such as often arise from urinary calculus; and had the calculus found in the kidney passed into the bladder, and been felt with the sound, there would have been nothing in the general symptoms, state of the urine, or history of the case, to have contra-indicated the operation of lithotomy, which, from the diseased state ofthe parts, would have been certainly fatal.

LACERATION OF RECTUM FROM INJURY.

In the absence of Dr Handyside, *Mr Spence* exhibited a rather rare specimen of injury of the upper part of the rectum, taken from the body of a man who died in consequence of a dreadful laceration, inflicted by the pole of an omnibus, penetrating the abdomen. The pole had entered the abdominal cavity about two inches below the umbilicus, passing obliquely upwards from over the right thigh. The small intestines were torn from the mesentery, and the lower part of the mesenteric artery was also lacerated.

The specimen exhibited the upper portion of the rectum, corresponding to the promontory of the sacrum, denuded anteriorly to a small extent of its peritoneal coat; whilst its posterior part, which was in contact with the sacrum, was lacerated to the extent of about two inches in length, for nearly two-thirds of its circumference.

Mr Miller exhibited a cystic oxide calculus, removed by the lateral operation. The patient had been cut by Mr Liston several years ago. [This case will be published in an early (if possible in next) number.]

Dr Andrew read a case of adherent pericardium, with dilatation of the descending aorta, and softening of the brain, [The case will be published in an early number.] The heart and aorta were exhibited to the Society.

CANCER OF LUNG, LIVER, AND SPINAL COLUMN.

Dr Bennett submitted to the society preparations from the lungs, liver, and vertebral column, illustrative of the case of a woman aged sixty, admitted into

the clinical ward of the Royal Infirmary, labouring under paraplegia. There was great emaciation, with a large sloughing sore covering the entire sacrum. On examining the chest, no particular dulness was heard anteriorly, but from the state of the patient, no examination posteriorly was made. The patient had never complained of cough, or any of the symptoms of pulmonary affection, but, on the contrary, was stated to have been very healthy and strong, up to a month before admission, when on waking one morning she found both her inferior extremities to be completely paralyzed. She sunk exhausted shortly after admission.

The lungs presented to the society exhibited several nodular masses, varying in size from a hazel-nut to that of a walnut, some of them breaking down into a soft pulpy matter, of cheesy consistence, yellowish colour, resembling tubercle. Half of the upper lobe of right lung, posteriorly, was occupied by a mass of this substance, the pulmonary tissue anteriorly being emphysematous.

The liver contained a number of white encephalomatous masses both on the surface and throughout its substance, and in the centre of the right lobe was a large cavity containing the same cheesy material as was found in the lungs. Other masses were observed passing from one structure to the other. The bodies of the 8th and 9th dorsal vertebræ contained a similar morbid deposit, and, from their enlargement, had pressed upon the medulla spinalis, and thus caused the paraplegia. The body of the third lumbar vertebra was also infected with cancer.

The microscopic structure of these morbid masses was for the most part very similar to that described by Müller, as the *reticulum* of cancer; *i. e.* broken up cancer cells, intermixed with numerous compound granular corpuscles and granular matter. In the liver, however, a more characteristic cell-structure was discovered, passing into the retrograde cancerous mass.

The bladder was corrugated, and its mucous membrane was cracked and fissured, presenting in several places an incrustation of crystalline phosphates.

From the sloughs on the back, and the exhausted state of the patient when in the ward, the posterior thoracic region was not examined; otherwise, no doubt, the mass in the upper lobe of right lung would have given the physical signs of condensation. Anteriorly the chest was everywhere resonant, and no anormal râles could be heard, neither were any functional pulmonary symptoms present; and it was certainly remarkable that such extensive disease of the lungs should have existed without causing functional derangement in those organs. Death was occasioned by exhaustion from the sloughing of the back. The case is an example of the great extent to which cancerous masses may be deposited in important internal organs, without materially interfering with the functions of the parts affected.

OLD HEMORRHAGIC LESIONS (APOPLEXY) OF THE LUNGS, RESULTING IN SLOUGHING LIMITED ABSCESSES, AND IN COLOURED CICATRICES—PERFORATION OF THE PLEURA.

Dr W. T. Gairdner had met with several remarkable lesions since his communication to the Society at the last pathological meeting (February 21st) illustrative of the sequelæ of hemorrhage into the lung.

The first of these lesions was from a patient who died under the care of Dr Andrew, in the Royal Infirmary, having presented during life distinct signs of empyema and pneumothorax. On dissection, the left pleura was found half full of fluid; the lung being compressed, and the upper part of the pleural cavity containing air, which had evidently come from a circular perforation of the pleura about half an inch in diameter in the lower lobe. This opening communicated with a superficial cavity the size of a cherry, and numerous closed cavities of nearly similar size were found in both lungs filled with pus, and containing small dark coloured sloughs, such as are commonly found in boils on the skin. Such abscesses were commonly ascribed to lobular pneumonia, but in the present case, the lesions were shown to be the result of extravasation by their occurrence in connexion with numerous coloured cicatrices,

not unlike some of the stages of false corpora lutea, which were dispersed both through the compressed lung and the other, which was generally pervious to air. These cicatrices presented, on microscopic examination, many of those masses of pigment, varying from rust colour to black, which were characteristic of altered hematin. Dr Gairdner was indebted to Dr Smith for a drawing of the abscesses and cicatrices in this case, which was exhibited to the Society.

Another case bearing on this subject was that of a man admitted into Mr Syme's ward with fracture of both legs. The fractures had united, and he was considered convalescent, when he was attacked with shivering and pain in the left side of the chest. Shortly after this the physical signs of pneumothorax were developed, and he died soon afterwards. This man expectorated during the last day of life a large quantity of prune-juice coloured sputum, a portion of which was brought to Dr Gairdner by Dr Littlejohn, who had detected in it, on microscopic examination, beautiful rhomboidal and feathery acicular crystals of a deep red colour. These were exhibited to the society; they were obviously exactly the same in character as those found in apoplectic cysts, and although in this case the evidence of post-mortem examination was wanting, Dr Gairdner was inclined to believe it to have been an instance of hemorrhagic condensation, going through the same course as in the preceding case.

Dr Gairdner had also made the examination of a case recently in Dr Christison's ward, which showed the transition of apoplectic nodules in the lungs into indurated globular melanotic masses, sometimes containing cretaceous matter.

MELANOTIC TUMOURS IN THE HORSE.

Dr W. T. Gairdner showed a specimen of melanotic tumour in the horse, which he had received from Mr Barlow.[1] It was of a bistre colour, yielded an inky fluid, and was of the consistence of a fibrous or sarcomatous tumour. This species of tumour occurred, as observed by Carswell, chiefly among grey and white horses, and was not uncommon in the skin and subcutaneous tissues, especially about the anus. The pigmentary matter was in the form of dark-coloured angular granules, almost the size of blood-corpuscles, of a somewhat crystalline appearance, and mostly of a deep brown or black colour, more or less mingled with a deep purplish tinge. These granules were both free, and enclosed in cells of the diameter of $\frac{1}{80}$th to $\frac{1}{100}$th of a line; they were no doubt varieties of the crystalline forms derived from the colouring matter of the blood, which is known from chemical analysis also to be the source of the melanotic colour. The tumour seemed almost entirely composed of this pigmentary matter and of fibrous tissue.

Dr Bennett said, that these tumours in horses presented quite a different structure from those of the human subject, the latter containing well marked cancer-cells, infiltrated with black pigment, whereas the former contained no such structures. He also said that there was a preparation in the University Museum, made by Dr Knox many years ago, which showed the apoplectic tumours in the lung described by Dr Gairdner.

CHRONIC CAVITIES IN LUNGS—BRIGHT'S DISEASE.

In the absence of Dr Halliday Douglas, *Dr Paterson* laid before the Society the lungs and kidneys of a young woman, admitted into the Hospital under Dr Douglas' charge in October 1848.

Both lungs were extensively diseased. The left was much diminished in its

[1] Mr Barlow writes, "We do not consider the disease malignant, as it does not run on to softening, will heal if cut or wounded, and does not cause death, except by mechanically interfering with some important organ, which is not very common." Carswell and Gluge also assert, that in horses, melanotic tumours have little or no tendency to recur; the latter, however, says, that horses who become affected with melanosis, seldom live longer than fifteen years.—*Path. Anat., Lief.* iii.—W. T. G.

general bulk; and was extensively condensed. The condensed tissue had the dark slate colour observed in chronic pneumonia (similar to what Dr Corrigan has described as a cirrhosis). In the upper part of this lung extensive sinuous excavations existed; and many smaller cavities in which no outlet could be discovered. It was difficult to affirm how far these cavities were the result of ulcerous excavation, and how far they arose from dilatation of bronchial tubes. They were all lined with firm fibro-membranous texture. The lung presented in its lower lobe an egg-shaped nodule of highly emphysematous tissue, and at the lower margin of the lung, were one or two semi-calcareous tubercles. The right lung was also very emphysematous anteriorly; it contained numerous irregular tubercular masses, and in the inferior lobe, there were two large excavations. Both kidneys were very much enlarged, they were pale and injected on the surface, which was however quite smooth. The section presented the usual appearance of what has been regarded as the earlier stage of the granular degeneration; the cortical substance being occupied throughout by very minute opaque granulations. There was a slight deposit of tubercle (not ulcerated) on the mucous surface of the intestines.

These conditions were interesting chiefly in reference to the following facts in the progress of the disease. She had been repeatedly a patient in the Hospital, and was known to have laboured under chest symptoms and albuminuria for eight years. The urine always had a low density—1007, and was always, when she was under observation, very pale, and nearly or quite free from sediment. During the last few months dropsy had existed, but only to a slightly greater extent than is observed in the last stage of phthisis. The condensation of the right lung had manifestly advanced; and the signs of excavation of the left summit became more apparent on her last admission. Hemoptysis was at times profuse; she had night sweats, and sank rapidly of late.

Hemoptysis had occurred early in her illness, and for long much difficulty was experienced by all who saw her in deciding how far the symptoms arose from tubercular phthisis.

EDINBURGH OBSTETRIC SOCIETY.

SESSION VIII.

MEETING II.—*January* 10, 1849.—Dr SIMPSON, President, in the Chair.

THE following gentlemen were elected office-bearers for the ensuing session:—Dr SIMPSON, *President;* Dr THATCHER and Dr R. RENTON, *Vice-Presidents;* Dr W. CUMMING, *Treasurer;* Dr W. CUMMING and Dr J. MATTHEWS DUNCAN, *Secretaries.*

CASE OF SUDDEN DEATH DURING LABOUR. BY DR DRUMMOND. COMMUNICATED.

Mrs Brown, æt. twenty-seven, wife of a paymaster-sergeant in the 71st regiment; married seven years—in her first pregnancy. She had lived for three years in the West Indies, and for some time in America, and, although not robust, had enjoyed, according to her own statement, pretty good health.

The catamenia ceased on the 12th December 1847. Of late, she observed her feet and legs considerably swollen, and sometimes her face puffy. I omitted, however, to ascertain, if these symptoms had existed before pregnancy had advanced, and none of her friends could afterwards inform me.

I saw her, for the first time, on the evening of the 28th September 1848. She had engaged the attendance of another medical man; but, in consequence of his being called to the country, he requested me to occupy his place. About nine P.M., when I saw her first, she appeared to be suffering slightly from the usual premonitory symptoms of labour. She had been sick, and vomited, and complained of general soreness, but not acute pain, over the region of the stomach, and for which warm fomentations were ordered.

A tactile examination could not discover the os, although the head was low down, and distinctly felt through the anterior segment of the uterine wall; no uterine action existed. She slept for two or three hours during the night, complaining several times, however, of the pain over the stomach. About six on the following morning (29th September), I was hastily summoned, and found her in a state of complete coma, after a severe convulsion fit, which came on suddenly, and was said to have continued for about fifteen minutes. Her breathing was stertorous, the pupils unusually dilated, the pulse slow and laboured, the left arm and leg in a state of tonic spasm, the right side completely relaxed.

I immediately opened a vein in the right arm, from which the blood flowed at first slowly (drop by drop), and of a very dark colour. After from five to six ounces had been lost, the left arm became relaxed, so that a vein in it was also opened, which bled freely. After from thirty to forty ounces had escaped the pulse became small and quick; the coma, however, continued, notwithstanding the application of cold to the head, and a strong turpentine enema, which was retained, but never acted.

The os was now felt to have become slightly dilated, the membranes were ruptured, and slight efforts at uterine action were once or twice observed. The coma still continued profound, and she died a quarter before eight, two hours and a half from the first seizure.

The heart was observed to beat for fully three minutes after the last respiratory effort. Repeated attempts were made to hear the fœtal heart, both by Dr Osborne and myself, but without success. The "Cæsarean section" was proposed, but, in consequence of the husband's absence, was not agreed to by the friends.

A small quantity of urine was drawn off by the catheter after death, of a colour as dark as porter, and very highly coagulable, almost gelatinous.

The want of a post-mortem examination in this case, renders it very incomplete; but still I think the suddenness of death gives it sufficient interest, and for which reason I have detailed it. Could the poisonous influence of urea act with such great rapidity, and kill in so short a space of time? Or, is it not more likely to suppose (remembering that she never complained of her head, and that the attack came on suddenly and without warning), that during the first and only convulsion a vessel had given way in the brain? Would the state of tonic spasm of the left side, the right being relaxed, throw any light on the diagnosis? Should any attempt have been made, either before or after the mother's death, to turn? My own impression was, that from the state of the os and the external parts, any such attempt would have been unsuccessful. To have given the child (supposing it alive) the least chance, would have been the operation that was proposed.

Her friends stated that she had long had a pale, sallow complexion, but they are unable to say whether she had ever suffered from dropsy before she became pregnant.

Several members spoke in approval of the energy and propriety of the practice adopted in the case reported by Dr Drummond.

BALL-VALVE OBSTRUCTION OF THE RECTUM BY SCYBALOUS MASSES.

BY DR SIMPSON.

A case of this not uncommon complication occurred a few days ago in the practice of Dr Husband. The patient, when Dr S. saw her with Dr H., was suffering from intermitting paroxysms of abdominal muscular effort, like those of labour. Dr H., on making a vaginal examination, had detected the impacted and distended state of the rectum. A large, hard, oblong, scybalous mass filled up the canal of the rectum. It was incrusted with phosphatic deposit on its external surface. The rectum was in consequence irritated by it, and tender under examination. The patient was chloroformed, the mass broken down by an iron instrument, and with some difficulty removed piece-

meal. Subsequently, some smaller and higher placed masses were expelled by the action of aperients, and the patient relieved from her sufferings.

Dr S. alluded to some other cases of the same kind which he had seen. They often caused great distress to the patient, and the disease was occasionally altogether overlooked for a time, and its nature mistaken. This was more particularly liable to happen in consequence of the circumstance, that a small quantity of thin feculent matter often escaped from day to day in these cases, leading to the idea that the uneasy sensations of the patient, and ultimately the paroxysmal and expulsive pains, could not be the result of obstruction in the bowel. Thin feculent matter in this way escaped, in consequence of the scybalous mass not nccessarily occupying and shutting up the whole calibre of the bowel, but allowing more fluid matters to pass between its surface and the sides of the intestinal canal. When, however, the patient used, as was instinctively done, any straining effort to make the expulsion more complete, the scybalous mass was by this means pushed down upon the lower and more contracted portion of the rectum, and acted exactly like a *ball-valve*, so closed the bowel, and prevented any further evacuation from it for the time. Such patients could sometimes partially evacuate the bowel in a lying, when they could not effect it in a sitting, posture, a result easily understood when we consider the mechanical character of the disease. It was an affection liable to recur from time to time in those who had been once subject to it.

Dr Peddie remarked, that two cases of this kind had occurred to him, in which purgatives and enemata had failed to relieve the symptoms, which were of an urgent character, but that he had succeeded in breaking down and scooping out the hardened masses with the handle of a spoon, assisted by the straining of the patients. One of these cases was that of a gentleman travelling in Switzerland on a pedestrian excursion, and in him the dense mass was found to consist of curd of goats' milk. For nearly a month he had partaken liberally of goats' milk and cheese, while he had a great deal of hard exercise.

CURE OF AN OVARIAN TUMOUR BY PREGNANCY. BY DR PEDDIE.

Dr Peddie stated, that about five years ago he was called to attend a lady on account of a tumour in the right iliac region, and which he regarded as ovarian. It was about the size of a child's head, irregular in surface, and moveable. A considerable enlargement at this part had for some time previously been observed, but advice was now for the first time taken, because the part had suddenly, after some fatigue, become the seat of pain. Leeches, fomentations, and blisters were successively applied, and also an ointment containing mercury and iodine, and the general health was attended to; but while the pain was soon entirely removed, the tumour remained without any very appreciable diminution.

The lady had been five years married, but had had no offspring; and in making the necessary examinations, Dr Peddie discovered that the hymen was so entire as not to admit of the passage of the finger, and of considerable strength. By the finger, however, he was enabled with a little force to rupture it, and he found the uterus in a normal condition. About six months afterwards Dr Peddie was called again, on account of what was supposed to be an enlargement of the ovarian tumour; but he found that, while it remained of about the same size as when last examined, the patient was in the fourth month of pregnancy. Serious apprehensions were now entertained on account of this complication; but gravidity went on favourable to its completion; the labour was safely accomplished, though tedious, and an excellent recovery was made. Immediately after confinement, Dr P. was surprised to find that the tumour had completely disappeared, and not long ago, having occasion to see her during a second accouchement, he could detect no trace of it. In this case, Dr P. had no doubt that the tumour was ovarian; and it was an interesting fact, that the pressure of pregnancy seemed to have accomplished its absorption.

Dr Moir knew of a case of cure of an ovarian tumour in a lady, a patient of Dr Hamilton and his father. The tumour was nearly as large as two doubled

fists, and gradually diminished under a course of weak mercurial ointment, which was applied by friction, continued for more than half an hour every morning for some months, some simple ointment being used at intervals to prevent salivation. She had also a warm bath thrice a week, for at least an hour each time, followed by a brisk walk of a mile or two. Since then the lady has had two children, and no tumour can be felt, although, after any unusual exercise, she feels a considerable degree of weakness and weariness in the part.

DOES AIR EVER BECOME INTRODUCED INTO THE VENOUS SYSTEM THROUGH THE UTERINE SINUSES AFTER DELIVERY? BY DR SIMPSON.

A series of cases that Dr S. saw in consultation, and that occurred in rapid succession, six or eight years ago, seemed to him to suggest the idea whether air might not become introduced into the venous circulation of the mother after delivery, and whether one common result of this accident was not the occurrence of a red or scarlatinoid eruption upon the cutaneous surface of the patient.

The first case occurred in the Lying-in-Hospital. The patient had been delivered of twins. Dr Zeigler was called to visit her, in consequence of some difficulty connected with the birth of the second child. Considerable post-partum hemorrhage, with alternate contractions and relaxations of the uterus supervened, and she seemed to rally very imperfectly from the effects of the flooding. In consequence of this, Dr S. saw her an hour or two subsequently. The pulse at that time was very rapid and weak, almost imperceptible. The countenance extremely anxious, and here and there was an evanescent red scarlatinoid rash over the body. The patient died in a few hours. The body was opened in a short time after death, because it was considered desirable not to incur the fallacy of air being present from decomposition; and Drs Simpson and Zeigler were anxious to ascertain if the anomalous symptoms that were present during life, could possibly arise from the entrance of air into the veins. To make the examination the more certain, the abdomen was opened under water. The lower vena cava, but specially the uterine and hypogastric veins were distended with frothy blood, and the air bubbled up through the water when any of these tubes were opened. The larger veins in the extremities were in the same state.

Some search was subsequently made into the literature of cases in which air had been introduced into the veins during surgical operations upon the neck, &c., in order to ascertain if the evanescent patches of red or scarlatinoid rash remarked in the preceding instance, had been observed in any of these surgical cases. Dr Warren of Boston, in an article on the subject in the American Cyclopædia of Practical Medicine, relates two cases which occurred in his own surgical practice. The first patient recovered after being insensible for a considerable time. While still comatose, "the leaden colour in the cheeks," says Dr Warren, "assumed a *reddish* tinge, and the alarming character of the symptoms was evidently diminished." The second case was fatal. During the period elapsing between the entrance of the air, and the death of the patient, Dr Warren, in describing the symptoms remarks, "The livid colour of the cheeks gave place to a suffusion of *vermillion red*, and no glow in the cheek of a youthful beauty ever gave one so much pleasure as that flush. But the flush soon passed off."

If the red or scarlatinoid rash which appeared in the preceding obstetric case upon the surface of the skin, were owing to the entrance of air into the uterine veins, might the symptom be accounted for on the idea, that the introduced air directly mixed with, and oxygenated the blood in the capillary vessels?

After the preceding case occurred, Dr Simpson was called to three or four other instances in which a similar train of symptoms was present;—namely, great depression after delivery, a rapid and almost imperceptible pulse, and patches of an erratic scarlatinoid rash upon the surface. All the patients

died within two or three days after delivery. In one case which he saw with Mr Kerr, and where the death was much more rapid, no red rash appeared, although the other symptoms were analogous. The first was the only case in which an autopsy was procured. Two or three of the cases had been considered as malignant scarlatina, a disease which was acknowledged by all our highest obstetric authorities to be exceedingly fatal to the puerperal female, but which may possibly in many other cases have been mistaken for, and confounded with, the affection to which this notice refers.

Supposing the introduction of air into the venous system after delivery to be the cause of the symptoms or affection in question, it is not perhaps difficult to understand the mere mechanism of its introduction. Surgical pathologists generally hold, that the air cannot be *drawn* into open veins, unless these veins are not very distant from the heart. But air may perhaps be *forced* into open veins when the open orifices are at a distance from the heart, and when circumstances exist capable of causing it to enter into such orifices.

A mechanism calculated to produce the entrance of air in this last way, exists in the uterus after delivery. The interior surface of the organ, especially opposite the late seat of the placenta, has a number of ruptured venous orifices opening upon it. Supposing air once introduced into the uterine cavity, which in some cases may occur in consequence of the alternate relaxations and contractions of the walls of the organ following delivery (as in after-pains, post-partum hemorrhage, &c.) ; and supposing further, that under the returning contractions of the organ, the expulsion of this air from the cavity was prevented by the presence of a clot at the os uteri, or other such obstructing cause, it will then, under the compression to which it is subjected, be liable to be driven into the open venous orifices existing in the lining membrane of the uterus. A bottle of caoutchouc of the shape of the uterus, and with tubes like the venous tubes of the uterus opening upon its interior, would act in this way on the principle of the force-pump, in sending any air sucked or drawn up into its cavity along these open tubes, if, after being full, its parietes were compressed, and its aerial contents prevented from escaping through the mouth of the bottle, as we fancied to be the case in the uterus under the conditions mentioned.

CASE IN WHICH THE LIQUOR AMNII AND MECONIUM WERE BOTH BLOODY.
BY DR MOIR.

In July 1848 a lady, then at her full period of pregnancy, had suddenly and without warning a discharge of fluid per vaginam, so copious as to run in a stream along the floor, after passing through her clothes and the chair on which she was sitting. From its colour it was supposed to be blood ; she was consequently put to bed, and Dr M.'s immediate presence was requested. On his arrival he found that during each pain there was a gush of dark bloody-coloured fluid, but as it did not coagulate, and as the pulse was not affected, he inferred that it was the result of some abnormal state of the liquor amnii, and therefore allowed the case to go on without interference. In about an hour a full-sized healthy female child was born. The placenta which still continued attached at the time of birth, soon separated, and appeared to be perfectly healthy.

Previous to the infant's being finally separated from the mother, it passed some meconium, and Dr M. was surprised on observing that it was of a very dark red colour, and that on being wiped off the sheets it left a reddish stain. On paying his visit next morning, the nurse, in great alarm, showed him several cloths containing meconium of the same appearance, but as the child seemed in every other respect healthy, nothing was administered but a small tea-spoonful of castor-oil, which brought off a good deal more during the day and evening. Next day the stools gradually assumed a healthy appearance, and have continued so. The child is healthy and well.

The first question that suggests itself in this case is,—whence the bloody liquor amnii ? was it from the intestines of the child ? or from the uterus or its membranes ? Most probably it was from the latter, because, had it been from the

child, we should have had some appearance of blood in the stools after the whole of the meconium was discharged; but there was none. But in his opinion the most interesting question in a physiological point of view, is,—whence the bloody meconium? because, if correct in the former supposition, the fœtus in utero must have swallowed the bloody liquor amnii, and this must have undergone the process of digestion. Are we therefore to infer, that the liquor amnii is always or occasionally one of the sources from which the fœtus derives nourishment. The general opinion hitherto has been, that it does not, but the present case seems to favour the conclusion that it does.

MARRIAGE AND REGISTRATION BILLS OF THE LORD ADVOCATE OF SCOTLAND.

These bills, after having been defeated in the last session of Parliament, are again before the Legislature in an amended form. The marriage bill, which encountered the chief opposition last year, is an attempt to remove those anomalies of the Scottish law of marriage which render it difficult, in many cases, to pronounce with certainty, whether that important relation has or has not been contracted by the parties. That great and valuable peculiarity of the Scotch law is preserved, which enables those who have become the parents of illegitimate children to repair the wrong they have done to their own offspring, and to make them legitimate in the eye of the law, *per subsequens matrimonium*. A special clause (the 10th) secures this, while the whole frame-work of the bill, and more especially the 1st and 8th clauses of it, will put an end to concealment of marriage, and make it difficult for unprincipled parties to contract second marriages during the lives of those to whom they are already wedded.

The other bill, for the registration of Births, Deaths, and Marriages in Scotland, to which the marriage bill may be regarded as ancillary, has been long desired by the medical profession, for the benefits which it is calculated to confer upon their own science; and has therefore been supported, in former sessions, by petitions from the Royal Colleges of Physicians and Surgeons of Edinburgh, as well as by other petitions, emanating from the medical profession. We cannot doubt that the profession will still continue to support the principle of the measure, and be ready to give every assistance in their power in the working of it, if it should become a law of the land. It is plain that the knowledge of the proportional mortality of human diseases, both as regards the whole country, and particular sub-divisions of it, is calculated to throw much light on the causes of disease, and on the efficiency of hygienic and remedial measures; and that nothing but a register will ensure accuracy in our estimates of that mortality, or, by consequence, in the inferences, prophylactic and therapeutic, which we deduce from these estimates. It is evident, too, that this measure has important bearings on the departments of the legislator, the jurist, and the statist, as well as on those elaborate calculations of the expectation of life, on the accuracy of which so large a portion of the property of the country depends.

We received a letter the other day from an intelligent medical gentleman, expressing his opinion that too large a portion of unrequited labour would be thrown by this bill on the members of the medical profession. We have therefore given our best attention to this matter, and have become convinced that there is no ground for such apprehensions.

By clauses 20 and 29,—the former relating to births, the latter to deaths, it is provided that the whole of the parties cognisant of the fact of a birth or of a death, whether parents, occupiers of the house, medical men, midwives, or others, shall be bound, under a penalty, to give notice to the registrar of the sub-district, of that fact; but the penalty shall not be exigible if any one of these has given the notice;—which may be done at any time within six weeks of a birth, or within eight days of a death. In this first step we can see not the shadow of hardship.

The registrar, being now in possession of the general fact, is bound to require more specific information for the purposes of the act, from the same parties.

The clauses applicable to this next stage of the proceeding are the 22d for the births, and the 31st for the deaths. Three months are allowed in the case of births, and fourteen days in the case of deaths, for compliance with the requisition of the registrar, addressed to such of the parties above named as he may think fit. The return is, of course, enforced by a penalty, else it would be neglected. The information may be given in the form of a written document, duly attested ; or, if that is preferred, by subscribing the register in presence of the registrar.

But suppose the statutory period of fourteen days to have elapsed, and no return to have been made by the medical or other persons to whom the registrar has directed his requisition, the object of the law is not in that case to be baffled. The registrar (clause 35) fixes a day and hour for the informants to call at his house or his office to give the required information. If they do not observe the appointment, then the registrar fixes a day and hour wnen he will call on *them* at *their* place of business or residence, and, in this last case, they incur a penalty for failure to be present at the time fixed, besides being bound to attend under a similar penalty on a future day.

Now, it is clear from all this, that every thing has been done which can be done to simplify this matter, and that it never can occasion any greater trouble to any medical man than he ought to be willing to take for so important an object. The compulsory clauses and the personal attendance are reserved as a last resource, merely to compel the careless or refractory to give obedience to the provisions of a most important law. Those who choose to pay common attention, will altogether avoid this difficulty.

We hope we need say nothing more to induce the members of the medical profession to lend their aid to the Lord Advocate in this attempt to confer on Scotland a benefit which England has enjoyed for the last ten years.

BOOKS RECEIVED.

Duke on the Cerebral Affections of Children. Dublin. 8vo. Fannin. 1849.

Pym on Bulam, Vomito Negro, or Yellow Fever. London. Churchill. 8vo. 1848.

Reid on Infantile Laryngismus. 8vo. Lond. Churchill. 1849.

Mulder—Chemistry of Animal and Vegetable Physiology. Part IV. 8vo. Edinburgh and London. Blackwood. 1849.

Wilde—the Closing Years of Dean Swift's Life. Dublin. 8vo. 1849.

Madden on Pulmonary Consumption. London. 12mo. Churchill. 1849.

Cunier—Mémoire sur l'Ophthalmie Contagieuse. Bruxelles et Leipzig. 8vo. C. Muquardt. 1849.

Wilson on the Management of the Skin and Hair. London. 12mo. Churchill. 1849.

Coote — The Homologies of the Human Skeleton. London. 8vo. Highley. 1849.

Smee's Elements of Electro-Biology, &c. London. 8vo. Longman. 1849.

Bouillaud—Mémoire Addressé à l'Assemblée Nationale et à M. le Ministre De l'Instruction Publique. (On the Deanship of the Faculty of Medicine). Paris. 8vo. 1849. J. B. Baillière.

Hawkins—The Hunterian Oration for 1849. London. 8vo. Churchill. 1849.

Journal of the Statistical Society of London, from March 1847. London. 8vo. Parker, 1849.

Blacklock on Cholera. 8vo. Madras. 1848.

Gillkrest on Cholera. 8vo. Gibraltar. 1848.

Transactions of the Provincial Medical Association. Vol. XVI. London. 8vo. Churchill, 1849.

Bremner—Hints on Obstetric Practice. 8vo. Edinburgh. Maclachlan and Stewart, 1849.

Dalrymple—Pathology of the Human Eye. Fasic. I. Imp. 4to. London. Churchill, 1849. Four coloured plates.

Maclise—Surgical Anatomy. Fasciculus II. Folio. London. Churchill, 1849. Four coloured Lithographic plates.

TO CORRESPONDENTS.

On account of the great length of the reports of societies, we are not able to insert this month any of the Cholera Reports, a large number of which are in type. The latter part of the Obstetric Society report is also unavoidably deferred.

Communications on Cholera have been received from Dr Fisher, Dr Paxton, Dr Grieve, Dr Thomson (Coalsnaughton), Dr Thomson (Jedburgh), Dr Clarkson, and Dr W. Mackenzie. We intend to devote as large a space as our matter will permit to this subject next month. We have as yet received no communications from the northern counties of Scotland; but we hope that those of our readers who have fitting opportunities will favour us with the results of their experience as soon as possible. We beg to refer them to the "Medical News" of last Number for a statement of our wishes.

Three communications received from Dr W. Smellie, H. H. B. Co.'s Service.

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*A Short Account of the Superstitions and Popular Practices relating to Midwifery, and some of the Diseases of Women and Children, in Ireland.* By W. R. Wilde, F.R.C.S., Surgeon to St Mark's Hospital, Dublin, and Honorary Fellow of the Royal College of Physicians, Stockholm,[1] &c.

It is scarcely necessary to inform the educated reader, that a superstitious creed, and certain mystic rites, derived from the earliest times, attach to almost every nation in a certain state of society; that some of these are of almost universal acceptation, that others belong to particular localities or races, and that their geographical distribution is a source of interesting investigation, both to the historian and the ethnologist. In many countries these rites and practices are still prevalent, and in others, more advanced in civilization, they are merely preserved in the ancient ballad, the bardic legend, or the traditional romance.

To enter into the inquiry as to the pagan, druidical, buddhist, or christian origin of these, and to compare the superstitions of one country with another, would be a subject of most inviting interest, but unsuited to a periodical such as the *Monthly Journal.* The last remnant of superstition in any country, will, for obvious reasons be the medical—the charm, the herb cure, the incantation, and fairy invocation, to avert or relieve disease. In Ireland, from the character of its inhabitants, and its peculiar political, religious, and social condi-

[1] This paper is an abstract of a communication made to the Dublin Obstetrical Society upon the 1st March, 1849.

tion, there has up to the present time existed a wide and fertile field for the collector of folk's-lore and popular superstition. We have, moreover, a large collection of native medical manuscripts in the ancient gaelic language, some of them of very great antiquity, in which many of our popular cures and medical practices are preserved. Of the general tenor of all these works, and of the particular merits of some, I have already given a description.[1] For the present, I may remark, that while they resemble in many particulars the books of Paulus Ægineta, an edition of which has been lately published by the Sydenham Society, they far surpass in interest to the medical antiquary the value of that work, by the quantity of local, historical, and topographical information which they contain. Many of them are, moreover, useful in a literary point of view, being good specimens of the now nearly extinct Irish language. They are, generally speaking, translations from some of the ancient classic medical authors, with commentaries by native physicians. From these works, from the legends of the last century, and from practices in vogue almost up to the present, I have, with the assistance of friends—Irish scholars, and country medical practitioners—made a large collection of general as well as medical popular superstitions, with the intention of arranging and publishing them at length upon some future occasion. The present condition of this country, the total disorganization of society, extending from the poorest to the highest, the ravages lately made by famine and pestilence, and the enormous emigration which has been and is still going forward, added to the general spread of education and other circumstances, a description of which it is here unnecessary to enter upon, are however obliterating our superstitions with such rapidity, that, in the hope of eliciting inquiry upon the subject, I have been induced to bring forward a portion of my stock without further delay.

PHILTRES.—As love or desire in some shape or another is generally the forerunner of the pregnant state, I think a detail of a few instances of love charms, philtres, potions, and incantations, administered or performed to promote desire, should properly commence this communication. These may be divided into the employment of certain herbs and medicinal substances believed to promote desire, and the performance of certain mystic rites and ceremonies of a rather diabolical nature. There are many Irish legends referring to such rites, and the old bardic traditions make frequent allusions to them. The belief in the efficacy of such means—which, I may remark, are generally resorted to by females, to excite or regain the affection of the other sex—is of great antiquity, and of very general acceptation in all half civilized countries.

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[1] See the letter on the tables of deaths in the Irish census for 1841; and an address delivered at the King and Queen's College of Physicians, Dublin, last year, and published in the *London Medical Gazette* for March 1848.

Othello won Desdemona, it is said, "by conjuration and mighty magic," and

> "—— Practised on her with foul charms;
> Abused her delicate youth with drugs and minerals
> That waken motion."

In Ireland, some years ago, there was scarcely an old herb doctress who could not prescribe something of this kind, and even yet, few years pass that we have not several instances of the administration of cantharides with the intention of promoting desire.

But that the following very remarkable, and disgusting superstition (probably Pagan), has been related to me by at least three persons worthy of the highest credit, and residing in different parts of the western country, I should be inclined to refer it to a period called "The dark Age." In this and all the following rites and superstitions, I shall, in order to economise space, relate the simple facts, stripped of their historical and topographical details.

A female, accompanied by a charm-working crone, such a *callagh* as would have been burnt in Scotland with the "witches branks" in her mouth not long ago, proceeds to a church-yard, and at dead of night (twelve o'clock), exhumes a male corpse nine days buried. With a black-handled knife [1] she removes a stripe of integument a couple of inches broad from the entire length of the body—from head to heel—including if possible a portion of those parts which are usually covered with hair, as the scalp, face, breast, pubis, &c. Having restored the corpse to the grave, the strip of skin is next stretched upon a tomb-stone, and over it certain spells are cast, and certain incantations pronounced by the attendant priestess, who sprinkles it with water found in the hollow of a sacred stone,[2] and then folding it up in the form of a cross, places it over the beating heart of the credulous girl, who, under her dictation, mutters certain other incantations.[3] Thus provided, the love-sick lass returns home, and, watching a fitting opportunity, surrounds with this charmed bandage either the breast, leg, or arm of the man whose love she wishes to obtain. To prove efficacious, it must be applied and removed without his knowledge; to effect which a state of anæsthesia by means of whiskey is generally induced.

Abortion.—Next in order we should consider procured abortion, instances of which frequently come under the cognizance of the medical man and the magistrate. Here, as in the former case, the

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[1] A black-handled knife possesses particular virtues, and is used in performing several of our ancient pagan rites.

[2] It would far exceed the limits or intention of this paper, to note or describe these various formulæ in extenso, but I may remark, that water found in the hollow of an old stone or the socket of an ancient cross, is considered highly efficacious in the cure of many diseases.

[3] Except in a very imperfect way, I have not been able to procure the form of words employed. I have, however, known an instance in which this fleshly girdle was bound upon the pudendum in the hope of procuring abortion.

she-quack is generally referred to, and besides the inculcation of certain mystic ceremonies and Pagan rites, of which the foregoing may be taken as the type, she administers the most drastic purgatives and emmenagogues, of which aloes and gamboge are the chief ingredients. Certain herbs and plants, particularly rue and savine pulled in a particular manner, which I shall describe upon another occasion, are believed by the country people to effect a like end. Whether medicinal substances or herbs are employed, a certain ceremonial is likewise enjoined. Can we wonder at the ignorant Irish girl wishing to conceal her shame by the destruction of her offspring, in a country acknowledged to be one of the most moral in Europe, and where *caste* is more certainly lost by the circumstance of pregnancy before or without marriage, when, in other lands boasted to be the most civilized, induced abortion, even among married females, in the upper ranks of life, is spoken of in society without reserve.

IMPOTENCY AND BARRENNESS.—The possibility of procuring either impotency or barrenness in man or woman is firmly believed by the lower orders of Irish, who think that such can be effected by a charm as well as the employment of medicinal substances. The former is most dreaded during the solemnization of matrimony, when it is believed that if a slighted lover or any person jealous of the happy couple be present, they have a power, by casting a certain number of knots, and setting a spell, of producing either impotency or barrenness. To avoid the effects of this charm, it was no uncommon thing sometime ago, for the peasant bridegroom to appear before the officiating clergyman with all the knots and various fastenings about him loosened—as his cravat, the knees of his breeches, his shoe-ties, and suspenders, &c.; and the female, who dreads such a catastrophe as unfruitfulness, retires with her attendants to undress immediately after the marriage ceremony. This is a very popular and widely spread superstition, common to Scotland, and to many other countries as well as Ireland[1]; and the casting of knots was one of the accusations frequently brought against the witches and warlocks north of the Tweed.

There are several Irish terms of very significant meaning employed in our early manuscripts, and still in use among the Irish-speaking people, to express impotency and barrenness. *Amrid*, meaning simply, barren or unfruitful, is a very common term in Munster at the present day, as well as the phrase *Faar-nâ-maireann agi*, meaning a man with whom it liveth not. *Meatacht*, cowardliness or failure, is also used by the country people in the same sense, and is likewise to be found in the ancient writings.[2] In

[1] See Dalziel's Darker Superstitions of Scotland.

[2] The male organ of generation is styled in the manuscripts, both medical and general, *Erball na Feithe*, Cauda Nervorum, the tail of the nerves; but in

a very old Irish law, preserved in the manuscript collection of Trinity College, the term, *Ban beim*, literally, a white blow, that is a blow that does not cut or draw blood, is employed to express procured impotency. It also means any bodily infliction which leaves no mark or scar. The passage runs thus—"What blow is it for which twenty-one cows are paid (as *eric* or *fine*)? The white blow for which the twenty-one cows or the eric, is this;—If incantation or charm has deprived the man of his hollow sinew (or nerve of desire as it is elsewhere rendered) or the woman of her sloe of desire, there are seven comhals equal to twenty-one cows to be paid for it. For every infliction is a white blow to the shedding of blood."[1] Other early Brehan laws likewise appoint certain punishments for procuring impotency or barrenness, either in man or woman.

The "Sloe of desire," alluded to in the foregoing passage, refers to the ovary or *Cnu*, the nut, so called from its resemblance in shape, and somewhat in colour, to the unripe fruit of the black thorn (Prunus Sylvestris; in Irish, *Droighean.*)

To procure barrenness, it is even still a practice to administer some milk in which a grass mouse (Sorex Araneus; in Irish, *Luch Feir*) has been boiled;[2] and in order that a woman may not bear more children, the after-birth is buried under an elder tree and covered with a wooden bowel by three old women. I believe, however, that it is necessary

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the spoken language it is usually termed *Bod* or *Ball Ferda*, the manly member, or *Ferdacht*, manliness. *Congab*, means the parts of generation in either man or woman, but *Congna*, a horn, is applied only to the male. *Magraille*, is the testes, and *Silne* or *Shillnay*, the seed.

The female organs are in vulgar language called *Pit*; but a more refined expression, and one used in the manuscripts, is *Tod*, signifying female purity; or *Feile*, literally "modesty," but this is occasionally applied to the organs of both sexes. The word *Feile*, modesty, is derived from a very ancient historic legend, "Fial, the wife of Luyhaidh, was a lady of strict virtue, and uncommon modesty, for she was so confounded with shame because her husband had seen her naked swimming in the river Feil, that she languished and died of grief. The stream received the name of *Inbher Feile*, from the fair Milesian, and is so called to this time."—See Keating's History. This circumstance of a death from modesty occurred, it must be recollected, a long time ago—immediately after the Milesian landing. *Bandacht*, womanhood or puberty, is another term still in use.

[1] See the Brehan laws preserved in the Library of the University of Dublin, H. 3-18. p. 229. For this extract as well as for much valuable assistance in arranging the Irish terms used in this memoir, I am indebted to my learned friend, Mr Eugene Curry. Indeed, without the aid of this distinguished Irish scholar, few works bearing upon Irish Archeology could now be produced.

Mr Curry has given me the terms and various passages in the Irish character, but independent of the difficulty of printing Irish, except in this country, it might appear pedantry to print them in a character so little known even to philologists. I have therefore written them with the Italian character. Those who desire to see most of our fatal diseases printed in Irish, I may refer to my Nosology given in the medical memoir attached to the last Irish census.

[2] "The heart taken out of a mouse when alive and tied about the arm of a woman, prevents conception; as Sextus saith."—K'Eogh's Zoologia, Medicinalis Hibernica. Dublin, 1739. p. 65. See also Bell's British Quadrupeds.

to perform certain mystic rites and incantations during this ceremony, but these I have not been able to procure, as the old women and midwives are very chary of this description of knowledge.

There are several special treatises on Midwifery in our collection of medical manuscripts. There is one curious midwifery tract in particular, copies of which are preserved in at least three of our most esteemed records. In this work a great number of receipts are given, both to procure barrenness and to induce fruitfulness in man or woman, as well as to prepare the latter for conception; to expedite delivery; and to discover whether females are pregnant, &c. Of these, the following specimens out of many, may suffice to afford the reader some idea of the nature of the work, as well as its date. The vellum copy from which I have had these extracts made, was transcribed in 1496, from Fiongan MacTully's book.[1] "To burn the meal of *Darnel* and frankincense on a red stone, and let the smoke pass through a funnel under her (a woman), and that will prepare her for conception." This receipt is probably copied from one of the ancient classic authors. In the commentary to the works of Paulus Ægineta, by Mr Adams, lately published by the Sydenham Society, we read that Darnel "may be set down as the *Lolium temulentum*. Dioscorides gives the fullest account of its medicinal faculties; * * * and used in a fumigation with myrrh, saffron, and frankincense, he says, it promotes conception.'" Vol. 3, p. 26.[2]

"Let *Neft* be boiled on wine or on water, and let her sit on a stool with a hole in it, and let the smoke pass under her; or let it be burned on a red stone, and its smoke pass under her; or let the herb itself be placed in her *Fele;* and,—Let the blood of a hare and its runnet be dried and powdered, and drank upon some wine, or ale, or milk, after the menstrual flood, and she will conceive." The Neft was probably the Nefrin of the ancients, which modern authors think was the oriental Narcissus. The hare seems to have been not only a charmed animal, but possessed of peculiar medicinal properties among our ancient Irish authors. Its liver is still used as a cure, or rather a preventative in epilepsy, hydrophobia, and other diseases of the nervous system. M'Tully's book contains, among other aphrodisiacal receipts, the following:—"To dry the penis of a hare, and powder it, and that powder to be taken by the man and the woman, and the woman shall conceive a son; but if it be the woman alone who takes it, she conceives a daughter." The testes of the boar and the roebuck, as

[1] Fiongan MacTully was one of the hereditary Irish physicians. He resided at Aughalustha, in MacDonnoch's country, on the borders of Roscommon and Sligo, in the province of Connaught. For an account of the hereditary Irish physicians and their works, see the *London Medical Gazette* for March 1848. See also O'Halloran's surgical works.

[2] Darnel grass, in Irish *Raidleadh*, "stops at the flux of the belly the immoderate flux of the menses and incontinence of urine."—K'Eogh.

well as the goat, together with several of our native plants, are likewise enumerated among the various receipts, either for removing or for procuring barrenness, given in this curious old work, which any of my readers, acquainted with ancient medical writers, will at once perceive was written, not only in the style, but according to the medical knowledge, of the middle ages. Besides the information which it affords as to the state of medical knowledge, it is also exceedingly interesting for the zoological and botanical information upon this country at the time in which it was written.[1]

In another medical manuscript, transcribed about the middle of the fifteenth century—a commentary upon some of the aphorisms of Hippocrates[2]—there are many curious reasons assigned for unfruitfulness in women; among the rest excessive fatness, by which the os uteri was partially closed. The word fatness, however, also means here "fleshiness." Coldness and smoothness of the womb are likewise assigned as a cause of unproductiveness; and many very extraordinary receipts given for the removal of that state, as well as to enable the practitioner to form an accurate diagnosis as to which sex the cause of barrenness exists in. Evidently the works of Avicenna and Rhases have been the original source of these opinions and remedies.

We are all aware how frequently barrenness depends on physical causes, susceptible of removal by art. Before midwifery was brought to the perfection to which it has at present arrived in the city of Dublin, and just before the time when Mosse, by establishing our noble Lying-in Hospital, placed the obstetric art on a sure and scientific basis—and in so doing gave a blow to quackery in that department at least—there resided in Dublin a celebrated doctress,

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[1] Some years ago, Mr Eugene Curry discovered, among the manuscripts in the collection of the Royal Irish Academy, a very curious poem, probably as old as the fifth century, in which the various animals then known in Ireland are enumerated. The circumstances attending its history are these:—Finn Mac Coole is made prisoner by Cormac Mac Art, monarch of Erinn, who, however, consents to liberate him on being paid a ransom of a couple or pair of every wild animal, which were to be brought to him to the Green of Tara. Cailte, one of Finn's chief officers, takes upon himself this apparently hopeless task, and the poem is a relation of his exploit to St Patrick.

Upwards of five years ago Mr Ball made a communication to the Academy on the subject, a short notice of which will be found in the proceedings of that body for the 12th of February 1844. I am not aware of any further investigation into this matter since. Many of the animals are not now known, or at least cannot be recognized by the names given in the manuscript. It would very much have added to the interest of the subject, and perhaps have ere this been attended with beneficial results, had the poem itself, which is very short, been printed in full, as thereby persons in distant country parts, as well as those engaged in the investigation of subjects connected with early Irish history, might have recognized some of the ancient names, and decided to what animals they belonged. There is another copy of this poem in the possession of Mr M'Adam of Belfast, who, with Mr W. Thompson has, we are glad to find, been working upon the subject.

[2] Library of the Royal Irish Academy, E. 3, 30.

famed for her powers of relieving barrenness. This woman, named Mary Walker, whose advertisement in the Dublin Courant, No. 458, for 1747, says that she "arrived here from Philadelphia, Venice, Madeira Islands, London, and Liverpool," entered into engagements with several ladies to render them fruitful; and, in one of our Dublin newspapers for 1749, there is an account of a trial, in which she sued for breach of contract a lady of high rank. She succeeded at law, and recovered the stipulated sum.[1]

There are several sacred shrines, as well as supposed holy rites, still resorted to by the unfruitful in this kingdom; the most remarkable of which is a saint's bed, in the Island of Arran, off Galway Bay, to which females, even of the better classes, were accustomed to resort, and spend a summer's night, when hope was long deferred. At the Patron of Glendalough, upon the 3d of June, the pretty girls of Wicklow may still be seen embracing the large sculptured cross, opposite the western entrance of the cathedral, in order to induce fruitfulness, as well as in the belief that, by so doing, they shall "never die in childbirth."

PREGNANCY.[2] — The peculiarities and popular prejudices common to the pregnant state, next in order, claim our attention. It was a popular belief among some of the lower orders, that a woman dying during pregnancy was saved hereafter in virtue of the sinless offspring which she carried. This peculiar, but not very widely spread, superstition—which I have known exemplified in this city not many years ago, when an unfortunate female was killed by accident, and great solicitude was evinced by her companions during the coroner's inquiry to ascertain the fact of her pregnancy,—appears to have descended to us from times before the acknowledgment of modern polemical dogmas; for, at present, unbaptized infants are not supposed, by some of the Christian Churches, to come within the pale of perfect redemption, but pass into a state of almost non-existence. Such children are therefore never buried in consecrated ground, but in graveyards set apart for the purpose.[3]

Very numerous, indeed, are the superstitions attending the pregnant state. The following may suffice as examples of these:—One

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[1] Several of Mary Walker's Puffs and Advertisements, say from 1747 to to 1749, are before us, as also the affidavits of persons benefited by her art, as sworn before the Lord Mayor. Among her qualifications I find the following: "She likewise will tell, in twenty-four hours, whether it be the husband's or wife's fault that they have no children, and will make him or her be as fruitful as any man or woman in the kingdom, provided the woman be not past childbearing." Medical practitioners of the present day have acquired celebrity, if not by a like means, at least by inducing a similar effect.

Pregnancy—In Irish, *Torrachus*.

[3] For some of the popular legends illustrating this belief, see the article "Irish Popular Superstitions," in the *Dublin University Magazine*.

of the most general is the belief in "Mother's Marks," which are usually supposed to be the result of accident or fright. Thus—

Seeing a hare is supposed to produce a harelip in the offspring; and, as a charm, it is recommended to the pregnant woman thus circumstanced to make a small rent immediately in some part of her dress.[1] Small clustering naevi upon the face or neck are generally attributed to raspberries, bunches of currants, or other fruit having been thrown at the mother while carrying. These marks are firmly believed, even by intelligent persons, to ripen during the season when the fruit comes to maturity: an idea which receives some degree of support from the circumstance of a heated atmosphere, as in summer, always making those marks appear more red and prominent. The flat, extensive, and cutaneous nævus, which is generally of a claret colour, is believed to be caused by wine having been accidentally spilled over the same spot in the mother in which it appears in the child. The raised lipomatous tumour, which is usually of a dark colour, and often covered with hairs, like an ordinary mole, is popularly attributed to the fact of a mouse or rat having been accidentally seen by, or having been thrown at, the pregnant mother. To avoid the effect of frights of this description, the woman so circumstanced is directed to place her hand immediately upon some part of her body not usually exposed, in order that the mark, which it is believed will appear upon the child she carries, may be seated there; as, wherever the hand first touches when the fright takes place, there it is supposed the spot or deformity will be present in the offspring.

The various monstrosities, as well as those congenital marks just alluded to, are believed to be the result of some strong impression made upon the imagination of the mother. If a woman, while "carrying," wipes her foot or shoe in the grass of a churchyard, it is firmly believed by many of the lower orders that her child will have a club foot (*Cam Reilge*).

The numerous imaginary longings, appetites, and desires of pregnant females are too well known to require description here, and are not peculiar to this country alone.

Although the normal term for gestation in the human female is nine months, there are certain circumstances under which it is believed it may be prolonged; for instance, if a woman in "the family way" passes under the head of a mare with foal, it is a popu-

[1] These superstitions attending the pregnant state are perhaps more widely distributed, and form a more general belief, even among educated people, than any other that I am aware of. In the works of the celebrated Malblanche there is a curious relation of a female in Paris, who, having gone to see a man broken on the rack, while in a pregnant state, was so much affected by it that her child, when born, had all its limbs out of joint. Whatever may be the cause, this is, at least, one of the earliest instances of congenital dislocations recorded.

lar belief that she will carry her child as long as the mare does her offspring.

In order to ascertain the fact of pregnancy as well as fecundity generally, there are many superstitious practices resorted to, of which the following example may suffice. At Ballymascaulan, near Dunkalk, there is an ancient cromlegh of great size, the top stone of which is supported on the points of three others. This upper horizontal flag is of great size, smooth and convex upon its upper surface, so that it is with difficulty anything will rest upon it. A woman visiting this stone may ascertain whether she will ever have a family, and how many, by trying to throw a pebble on the top. If she succeeds, it is regarded as certain that she will be fruitful, and the number of stones she can lodge on the top represent the number of times she will conceive. It is no uncommon thing for the midwife or accoucheur to be asked by the friends of a recently delivered woman how many more children she will have. He is expected to be able to tell by the number of knots (or sinuosities in the vessels) which appeared in the funis.

In the ancient manuscripts, already referred to, we find some curious notices with regard to means for procuring a change of sex,—thus "to procure a change of birth in a woman who has brought forth daughters only,—to boil the testes of a wild boar (*Turc Allaidh*) upon new milk, and eat them, and drink the milk at the time of the monthly blood (*Fuil misa*)."

"The woman who brings forth daughters only, this is her cure, viz., to tie the (*Fiacail*) tooth of a stallion in a thong of seal skin, and to put them under seven masses,[1] and to suspend them at her neck." These curious receipts, to which analagous ones will be found in the old English herbels, as well as in the ancient classic authors, were once believed to be efficacious by the medical profession. It is strange but true that what is now regarded as a popular superstition, was acknowledged as science some hundred years ago. Several examples in proof of this might be adduced from the works of Paracelsus, but even many of the opinions of Sir Thomas Browne, who wrote against "vulgar errors," are at the present time believed to be erroneous, if not superstitious.

Under the head of pregnancy may be included accidental abortion, concerning which we find the following notices in the manuscripts. "Knowledge of bringing forth abortion properly, viz., to boil *Buafalan* (Jacobea vulgaris, or rag-weed) upon water, and put it under the woman, and she shall bring forth safely.

"The seeds of water cresses (Nasturtium Aquaticum, *Bilrugh*) to be boiled on whey for a woman, and she will bring forth a miscarriage."

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[1] When the peasantry wish to render any object sacred or medicinally efficacious, it is laid upon the altar under the gospel, during the celebration of a certain number of masses.

"The juice of the *Gall-biolar* (foreign water cress), to be drunk by a woman who has a dead child." Also for the same purposes, it is recommended "to put horse dung upon a red flagstone, the fumes of which taken into the nostrils, are related to be highly efficacious." "For the shedding of blood of a woman, boil *Esbuc-ban* (Bellis Major, or great Oxeydaisy) on new milk and glorium, and let her drink it." For further information upon the virtues of these plants, we must refer the reader to the works of Keogh and Threlkeld.[1]

In order to procure a safe delivery, the following receipt is given. "The tops of *Brisceum* (Potentilla anserina, or silver-weed) to be broken and boiled on beefs tallow, and squeezed through a linen cloth; then mixed with wheaten meal and salt, and boiled again. Of this to eat nine bits fasting, and three at lying down, during nine nights; and it is to procure safety it is done."

MIDWIFERY.

We now come to the investigation of the more immediate object of this paper, the popular and superstitious practices employed in midwifery, under which head may be considered not only delivery but the puerperal state. In the notes to page 714 I have already explained the meaning of certain Irish anatomical terms for the genitals. The uterus is not only in the manuscripts, but in the spoken language, termed *Macla*, or *Maclog*, literally the place of the son; the place or situation of the child, for the *Mac* is in this case equally applied to males and females. Another term for the womb is *Luihgsheol*, the child's bed. Delivery, or the act of child-bearing, is denominated *lees Naidhean*; but in our ancient medical writings the Latin terms are generally employed, although spelled with the Irish character.[2]

The midwifery practice among the lower orders in Ireland and the various superstitions appertaining to delivery, would form materials for an entire essay; and many curious anecdotes and ancient legends are related of the Irish accoucheuse, for some account of which we would refer our readers to Carleton's Introduction to the Irish Midwife, in his "Traits and Stories" of the peasantry. It is curious, however, that there is no Irish term for the word midwife.

Tedious and Difficult Labour.—In tedious labour, innumerable devices, charms, and mystic ceremonies are had recourse to. One of the most common, is that of loosening every place, person, or thing in or about the house, unlocking all the locks, unbarring

[1] Synopsis Stirpium Hibernicarum; alphebetice dispositarum sive commentatio de plantis indiginis, præsertim Dublinensibus instituta, &c. &c.—Auctore Caleb Threlkeld, M.D. Dublin, 1726.

[2] Any of the diseases of the female genital organs, or the anus, is thus popularly expressed, "A disease between one and the ground." Prolapsus uteri is so termed, and in Irish *Tuitim an Muclaigh*, a falling of the womb; or a falling of the place of the son.

doors and windows, untieing all knots, and even setting free the cows in the byre, in the hope that, by some occult sympathy, not well defined, the matrix may be induced to dilate and expel its imprisoned contents. It is also done to prevent any charm, such as the casting of knots, &c., which might have been performed by an evil-minded person, proving efficacious. But that it would lead us from our original intention, we could show that this was one of the most widely-spread of all medical superstitions.

I have known the following anecdote—a hackneyed one among country crones—related by the nurse-tender to a lady, to amuse her while her "time" was passing slowly. Among the cures and charms supposed to facilitate labour, in cases where it exceeds the usual duration, is that of placing a cuckold's hat, if such can be found in the vicinity, upon the head of the patient. In one instance, several hats belonging to the neighbours had been tried, but without success, when the groaning wife cried out in despair, "Arrah—don't be taising me any longer, but bring me Paddy's caubeen." Her husband's hat was accordingly made trial of; and, it is said, soon proved efficacious. The labour was no longer in vain; the cap fitted.

It is boasted that the Irish midwives were acquainted with the efficacy of spurred rye, or smitted corn, long before the ergot was made use of by regular practitioners. But in cases of want of uterine action, several other substances were employed, some of rather a disgusting nature. Among the rest the husband's urine was regarded as highly efficacious. To promote labour the country midwives sometimes placed the parturient woman with her hips directly opposite a roasting fire, as is the common practice in several parts of India. In some localities it was not in former times uncommon, when the labour was very tedious, to get two or three stout men to shake the unhappy patient backwards and forwards in her bed with great violence; and for some reason, which I am unable to account for, a ploughman was more frequently chosen for such purposes than any other person; but, to prove efficacious, he should be taken direct from the plough. Owing, however, to the number of country midwives that have of late years been educated at our different lying-in hospitals, practices such as those related are seldom had recourse to now.

Labour girdles,[1] as well as the cord of St Francis, were much employed in Connaught some twenty or thirty years ago, in order to ensure a safe delivery. They were even had recourse to by

[1] The first of February or Candlemas day is a high festival among the lower orders in the South and West of Ireland. Among the various rites and superstitions then performed, is that of setting a charm for a labour girdle. This latter is either a ribbon, or a band, or scarf, which having been duly incanted, is hung upon a line or hedge in some exposed place during the night. If, upon examination next morning, it is found to have increased in length, it betokens length of days; but if it has got shorter, it indicates an untimely end to the person who performs the charm. At all events, it becomes endowed

persons in the middle ranks of life, particularly, before any manual interference was permitted. Not only was the patient encircled with the sacred girdle, but she was also surrounded with several other charms and amulets. Those round or ovoid stones which we sometimes meet with in grave-yards and ruined churches, by the sides of holy wells, and on the rude stone altars in the extreme west of Ireland, are believed to possess certain obstetric virtues, and are carried a considerable distance and laid in the bed of a woman in labour. In the vicinity of Clonmacnoise, a friend of the patient is despatched, in any case of tedious labour, with directions to embrace the large stone cross, which still exists among the ruins of that sacred spot, so that the points of the fingers may meet around it; and if, from the shortness of the hands or arms of the person so employed, it is impossible to effect the desired object, then a handkerchief or some article of dress is made use of to complete the span. Upon the return of the messenger, who may be either male or female, the woman in labour is embraced by him or her in the same manner, and in this way the "knot of childbirth" is immediately unloosed. Some of the old midwives pretend to effect the like object by knotting and then untieing over the patient a number of ribbons, on which they had previously pronounced certain prayers and incantations. It was also customary in the west, to attempt to procure a safe delivery, by counting nine articles of clothes (men's if possible), over the woman "during a pain." The midwife generally consoles the mother under her sufferings by assuring her, that in proportion to the amount of pain she experiences, so will be the love and affection of her child. A very popular belief which midwives rather encourage than otherwise, and to which many witty allusions are made by the people, is that these practitioners possess the power of transferring the pains of labour from the groaning wife to a man;—to any person they wish. The husband is generally the person selected, but old bachelors are said to be, above all others, objects of the midwives' vengeance; on which account, as well as the quizzing which they generally get upon such occasions, these celebats are seldom seen at the houses of the peasantry when the woman is "in the straw."

If we turn to the manuscripts, we find some curious receipts given to hasten delivery. "The milk of a hound and of a ewe, and wine; equal quantities to be taken as a drink, and she shall be immediately delivered." For a woman to bring forth a child without danger, let the following words be written on vellum, and tied on her abdomen, viz. "Maria peperit; Christum peperit; Anna peperit; Maria, Elizabeth, et Johannem, Celestium Remegium, Satur, Arepo, Tenet, Opera, Rotas." [1]

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with peculiar virtues, and is ever after called the *Brat Brighde*, or St Bridget's Garment, from its having acquired its peculiar powers upon the festival of that Saint.

[1] These words, written in the form of a square, with crosses between them, form a charm of very ancient date, and described in most books of magic.

And in later times, Keogh, from whom we have already quoted, in enumerating the virtues of the hare, says, "the eyes drawn entire out of the head of a hare taken in March and dried with pepper; one of these being so tied to the belly that the sight of the eye may touch it. This will facilitate labour in women, but the child being born it is to be removed, lest the womb also comes out." Need we wonder at popular superstitions long remaining in a country where this was gravely put forward by a Bachelor of Arts, and Protestant Chaplain to the Baron Kingston of that day!

UNNATURAL PRESENTATIONS.—In cases of cross birth women used actually to be turned upside down, and while thus suspended by the feet shaken violently in order to alter the position of the child. This custom, however, is now very rarely resorted to. Notwithstanding, I am informed, a death actually occurred from its employment not many years ago, near Gweedore, in the county of Donegal.

Spontaneous evolution was, it would appear, known to the early Irish midwives. I have heard several old physicians state that they had been informed of the fact by country nurse-tenders long before my friend Dr Douglas's valuable essay appeared. In Gabelhover's book of physic printed in 1559, there is a receipt given:—"To cause a childe beinge ether alive or deade, to revert or directlye turne itself agayne in his mother's bodye lyinge therein indirectly turnede," (p. 235). The same work contains many curious receipts on subjects connected with midwifery similar to those which I have quoted from the Irish manuscripts.

The rude interference of some of the country midwives, particularly in the province of Connaught, caused frightful consequences in former times. I have heard my father, who was in extensive practice in that part of the country, say that he had known midwives constantly attempt forcible extraction of the fœtus, with the hook of an ouncel or steel bar; and I am informed by practitioners residing in the county of Galway still, that some years ago, when any difficulty or delay occurred in the child's head passing through the external parts, the midwives used to cut or lacerate the perinæum with a sort of curved knife called a "*medoag*," formed out of the point of an old reaping-hook.

The belief in the virtues of the caul or membranes as a preservative against fire and water prevails in this country, as well as in England, yet I have never read or heard of one being advertised in our public newspapers, a practice of rather common occurrence in England.[1] Even the caul of an animal is looked upon by our peasantry as efficacious, provided it is expelled unbroken. It is generally preserved and thrown over the collar-beam of the house. May not

[1] See a very entertaining little book, 'Popular Superstitions,' published in the Series of London Anecdotes, p. 122.

the virtue attached to an unbroken membrane have had its origin in the sage advice of some observant practitioner, inculcated for the purpose of preserving them unruptured to the last?

In dividing the funis, the midwives always leave a longer portion attached to the abdomen of a male than to that of a female child.

To predict the sex of a woman's next child, it is only necessary to observe the following rule. If the woman's last child, or even abortion, was born when the moon was on the increase, it will be a boy; but if on the decrease, a girl.

Hemorrhage.—In cases of flooding a number of popular remedies are employed; some medicinal, others partaking of the nature of a charm. A very old one, and now nearly obsolete, is to bury a harrow pin in the ground and leave it there till the woman's safety is insured. It is also a common practice to include the wrists, ankles, fingers, and toes in tightly drawn ligatures in order to arrest the return of the blood. In menorrhagia it is usual to tie worsted thread upon the fingers, particularly on the last phalanx of the thumb. I have seen very injurious consequences arise from the constriction employed in these cases. Not only in uterine hemorrhage after delivery, but in that which occurs in connexion with the menstrual function, a very popular remedy some years ago consisted in the administration of a few drops of the discharged fluid, mixed with a weak solution of the sulphate of copper. Where the hemorrhage occurred in the unimpregnated female, it was customary to bury, without the patient's knowledge, some of the discharges.

In the manuscripts, yarrow is recommended to be mixed with some fluid and drunk as a styptic. The various popular remedies for hemorrhage generally which I shall describe hereafter, are also made use of;—among the rest, pig's dung, which is believed to be highly styptic, and consequently applied as a poultice to the abdomen.

Management of the Placenta.—In cases of retention of the after-birth, it is even still a practice to force a piece of mould candle down the patient's throat. It is possible that, by inducing vomiting, it may prove efficacious in causing the uterus to expel its contents. But in almost all instances among the lower orders in the remote districts, where the placenta has been retained for an unusual length of time, the cord is either tied to the patient's thigh, or held fast by one of the attendants. I remember having been sent for to a case of this description in a distant part of the country; I found the midwife on her knees at the bed side with a firm grip of the funis in her hand, which she exultingly displayed to me, observing, "Doctor I never let it out these five hours." It is a popular belief that if the funis was let go, it might slip back into the uterus and be for ever lost sight of. I have known more than one instance in which eversion of the uterus was produced by the midwife forcibly dragging the funis.

The human placenta is either destroyed by fire or buried; generally the former. When buried it is usual to place it under a hedge, or in some locality where its grave cannot easily be polluted by the foot of man or beast.

After-pains are said to be relieved by eating plentifully of boiled cabbage.

The subject of the puerperal state, as well as the diseases of children, will be considered upon a future occasion.

In the foregoing recital I have, from the nature of the subject, been obliged to employ a more homely phraseology than is usual in professional writings. To many the description of these popular superstitions may appear uninteresting, and even useless; but, independent of all other considerations, I think it is of importance that young country practitioners should be made acquainted with the popular practices in use among those with whom they have to deal;—practices that are generally had recourse to before the aid of modern science is called in—which they ought by all means to counteract, but which in many instances will be resorted to the moment their backs are turned.

ARTICLE II.—*On Typhoid and Typhus Fevers,—An Attempt to Determine the Question of their Identity or Non-Identity, by an Analysis of the Symptoms, and of the Appearances found after Death in Sixty-six Fatal Cases of Continued Fever, observed at the London Fever Hospital from January* 1847 *to February* 1849. By W. JENNER, M.D., London, Licentiate of the Royal College of Physicians.—(*Continued from p.* 680.)

MILIARY VESICLES—HYGROMETRIC AND THERMOMETRIC CONDITIONS OF THE SKIN.

Typhoid Fever.—Miliary vesicles were noted to be present during life in three of the twenty-three cases here analyzed. In one they appeared on the 84th day of the disease,[1] and continued till the death of the patient on the 27th day; in one of the three there were none present on the 9th day, but a great number were observed on the 11th, and continued till the 14th or 15th, the patient dying on the 25th day; in the third case there was an abundant crop on the 26th day. In all the three cases they were preceded and accompanied by a warm skin and profuse sweating.

In four of the remaining twenty cases miliary vesicles were de-

[1] From the calculation made, when treating of the duration of the two diseases, it appeared probable that I had ascertained very nearly the date of the disease on which those said to have been admitted on about a given day of the disease were received into the hospital. I shall, therefore, henceforth, make no distinction between the two groups, but speak of all as admitted or seen on a given day of the disease.

tected after death; two of the four sweated profusely just before death; the condition of the skin in the other two at that time was not recorded. Therefore seven of the twenty-three had miliary vesicles—*i. e.*, in the proportion of 30·4 per cent., or nearly one-third.

The skin during the first week of the disease was hot and dry in two cases, warm and perspiring in one case. None of these cases were seen before the 4th day of the disease.

During the second week it was hot and dry in one case; hot and sweating in three; cool or cold in one.

During the third week it was hot and dry in three cases; hot and sweating in one; warm and dry in two; cool and dry in five cases.

During the fourth week it was hot and dry in three cases; hot and sweating in one; warm and dry in two; and warm and sweating in one case.

The variations in the temperature of the skin in the same patient were considerable; and in several instances where the typhoid fever was prolonged, these changes were observed to have no relation to the duration of the disease. Thus the skin hot on the 8th day was in one case warm on the 9th, hot on the 10th, warm on the 11th, 12th, and 14th, and again hot the six or seven succeeding days. And the same want of relation existed between the sweats and the duration of the disease. Thus in the case last referred to the patient perspired during the 10th, 12th, 13th, and 14th nights—the skin having been dry during the days; on the 17th day the skin was moist; while on the 15th, 16th, 18th, and 22d, there were no sweats either at night or during the day.

Typhus Fever.—Miliary vesicles were observed during life in two of the forty-three cases. In one the hygrometric condition of the skin was not noted at the time of their appearance—viz., the 12th day of the disease; they had disappeared before the patient was seen on the 16th day—the skin having been cool and dry the preceding day. This patient survived till the 148th day from the commencement of illness. In the other case the skin, hot and dry on the 11th day, was sweating profusely on the 12th and 13th—on neither of which days were any miliary vesicles present; but on the 14th day there were a few in the groin and under the clavicles. This patient died on the 15th day of the disease.

There were no miliary vesicles in eleven cases. The skin in three of the eleven perspired profusely; in one it was cool, in five warm and dry; and in two it was hot and dry.

No note of the presence or absence of miliary vesicles was made during life in the remaining thirty cases; but in three of the thirty some were discovered after death. The hygrometric state of the skin, which existed for some time before the fatal termination, was not recorded in these three cases.

During the first week of the disease the skin was hot and dry in

two cases, and warm and dry in four—none of them being seen before the 5th day.

During the second week it was hot in seven cases, two of which sweated profusely, one on the 15th, and the other on the 13th and 14th days; it was warm and dry in fourteen cases, two of which sweated profusely on respectively the 9th and 10th day; it was cool in twelve cases, two of which perspired freely on the 15th day of the disease.

The profuse perspirations in the above six cases preceded the fatal termination one or two days, and continued till death.

During the third week of the disease the skin was warm and dry in one case, which survived till the 148th day of illness, and cool in three cases. After the temperature began to fall, it was exceedingly rare for it to rise again, unless the illness was prolonged by local disease beyond the termination of the typhus fever.

It is remarkable that of the five patients who sweated profusely, and on whom, nevertheless, no miliary vesicles were detected, four were more than forty years old, their mean age being fifty-five years; while the five patients on whose skin miliary vesicles were detected, either during life or after death, were all under forty, their mean age being thirty-one years.

Seventeen of the forty-three patients who laboured under typhus fever, and whose cases we are examining, were under forty years of age; five of the seventeen—*i. e.*, in the proportion of 29·4 per cent. —had, as I have stated, miliary vesicles on their surface, either detected during life or discovered after death.

Twenty-six of the forty-three patients labouring under typhus fever were forty years of age and upwards; but on not one of these was a vesicle detected.

EXPRESSION—MANNER—HUE OF FACE, ETC.

Typhoid Fever.—The expression in two cases was nearly natural throughout the whole course of the disease.

In the majority of the cases here examined it was oppressed, heavy, and somewhat anxious; the expression of heaviness and prostration was, however, far less marked than in the cases of typhus fever; and in two patients, while delirious, the expression was highly vivacious.

The natural hue of the face—*i. e.*, of the skin of the whole face —was unchanged, excepting in three cases, in which it had a very slightly marked dusky appearance.

There was no flush in three, and no note made of its presence in eight cases.

In twelve of the twenty-three cases the face was flushed; in eleven of the twelve the flush was pink, and limited to one or both cheeks; it varied in intensity, disappeared, and returned occasionally, in the same day.

This limitation of the flush to the cheeks was not peculiar to any

one period of the disease; it was well marked in one case, when admitted on the 8th day of the disease, and continued so till the 23d day, the patient dying on the 25th. In another, admitted on the 5th day, there was a circumscribed flush on the cheeks on the 7th day, which continued with little change till the 15th day, the patient dying on the 17th day.

This flush when conjoined, as it sometimes was, with extreme emaciation, sunken eyes, large pupils, quick breathing, sharp and somewhat anxious manner, forcibly recalled to the mind cases, not of typhus fever, but of phthisis.

Typhus Fever.—In none of the forty-three cases was the expression natural throughout the disease.

In a large majority of the cases both the expression and the manner of the patient were so peculiar, that from them alone the diagnosis might have been formed. They were dull, heavy, oppressed, confused, like those of a drunken man just disturbed from sleep. The mind was rarely intelligent enough after the commencement of the second week to be disturbed as to the final issue; and, as the disease in itself is free from serious organic lesion, all automatic as well as mental expression of anxiety was absent.

The hue of the face, after about the 6th day was, like the expression and manner, peculiar—it was thick and *muddy* looking; the change from this condition to the clearness of health was most remarkable—it was well seen in the patient who recovered from the attack of fever and died of small-pox, before referred to.

This muddy hue had no relation to the flush of the face, for it was often present when the face was pale; moreover, though only noted in the face, it affected more or less the whole skin.

The face was flushed in eighteen cases, and in every case the flush covered the whole face, though in some it might have been somewhat more intense on the cheek than elsewhere.

The colour of the face when flushed was dusky red, and never pink as the cheeks were in the cases of typhoid fever.

In some cases the face was recorded to be pale throughout the disease. Its condition in the other cases was not noted.

HEADACHE.

Typhoid Fever.—In two cases, the histories of which were complete, and the duration of the disease lengthened, there was no headache.

In twelve cases, headache was one of the earliest symptoms; in five of these it disappeared respectively on about the 6th, 13th, 13th, 17th, and 17th day of the disease; in six, before they came under observation, severally on the 15th, 16th, 18th, 21st, 21st, and 28th day. In one case there was a little headache on the 14th day; but, from the subsequent mental confusion of the patient, it was uncertain when it ceased.

Of the previous histories of nine cases, with respect to the point

we are investigating, no particulars could be obtained; but in three there was no headache on the 15th, 21st, and 21st day; in one there was only trifling headache on the 15th day; the state of mind in the remaining five was such when they entered the hospital, that I could not ascertain if headache existed at that time.

Thus headache was in a large majority of cases one of the earliest symptoms of these cases of typhoid fever; and in the majority of them it disappeared spontaneously at the latter end of the second, or during the third week of the disease.

The pain was generally seated in the forehead, and never in the occiput. It varied much in degree, some patients complaining of it severely, others scarcely noticing it. It had no definite character —*i. e.*, the patients could rarely give it any name, as darting, bursting, &c.

Typhus Fever.—In two cases there was no headache.

In nineteen cases it was one of the earliest symptoms, and, in the majority, commenced the first or second day of the disease; in four of the nineteen it disappeared respectively on the 8th, 9th, 11th, and 12th day; in seven there was no headache present when the cases were severally taken on the 8th, 9th, 9th, 10th, 10th, 13th, and 14th day; in three others it was still very severe respectively on the 6th, 7th, and 9th day; in the remaining five of the nineteen there was so much mental confusion or actual delirium present when the patients came under observation, that no dependence could be placed on their answers to questions regarding their sensations; the fact of the existence of headache at an early period of the disease was in these cases obtained from the friends of the patients. In three cases, the histories of which were doubtful, the headache was trifling when they were first seen severally on the 8th, 8th, and 9th day; it had disappeared in all the three on the 10th day.

Of five patients in whom the duration of the disease before their admission into the hospital was unknown, and the histories of whose cases were uncertain, four had some headache when first seen; the fifth was at that time free from pain in the head.

No dependence could be placed on the replies of fourteen of the forty-three patients to questions regarding their sensations, in consequence of their mental confusion; but none of the fourteen while in the hospital complained of headache.

So that headache was one of the earliest symptoms in a large majority of the cases, and it usually disappeared spontaneously by the 10th day of the disease, and in every case before the termination of the second week.

The seat of the pain was generally the forehead, very rarely the vertex or temples, and in no case the occiput. It varied much in severity, was often extremely intense, but was sometimes so trifling, that unless inquiries had been made of the patients, they would not have mentioned it.

It was usually an undefinable pain, not to be characterised by such terms as stabbing, throbbing, &c.

DELIRIUM.

Typhoid Fever.—There was no delirium in three cases, but in one of the three there was considerable mental confusion.

Delirium was present in twenty cases; in ten of the twenty it began severally on the 3rd, 6th, 6th, 10th, 14th, 15th, 23rd, 26th, 28th, and 29th day; seven were delirious when first seen between the 13th and 21st days inclusive; in the remaining three of the twenty cases it was uncertain at what date of the disease the delirium set in.

The delirium continued till death in nine cases; in eight of these the fatal termination occurred on the 12th, 17th, 17th, 21st, 25th, 27th, 30th, and 30th day of the disease; in one of the nine the duration of the disease at the time of the patient's death was unknown.

In one case, which proved fatal on the 23rd day, there was no delirium after the 21st day, from which time till her death the patient lay in a state of profound stupor.

The delirium usually first showed itself at night, the patients sleeping during part of the day. It varied much in amount, sometimes being so violent that the patients left their beds, and even ran screaming through the wards; at others showing itself by slight delusions only discovered to exist by accident, or again by almost constant chattering.

Ten of eighteen patients—*i. e.*, more than one-half, or in the proportion of 55·5 per cent. of those who were delirious after they entered the hospital, and of whom notes on the point were made—left their beds to wander about the ward.

Typhus Fever.—The mind of one patient of the forty-three, whose cases I am here examining, was perfect throughout the disease. This was a very mild case in every particular; the patient died of phlebitis after he had once left his bed. In another case there was only slight mental confusion; this patient also survived the fever. Although there was no actual delirium in thirteen other cases, yet there was in them such extreme mental confusion that the patients could give no account of their past state, "felt bothered," had no idea how long they had been in the hospital, nor in some cases where they were.

Delirium began in ten cases respectively on the 5th, 8th, 10th, 10th, 10th, 11th, 11th, 11th, 12th, and 13th day. It was present when five cases first came under observation, severally on the 6th, 8th, 9th, 9th, and 14th day.

In every instance in which the delirium commenced after the patients entered the hospital, excepting one, it was preceded by a varying amount of mental confusion.

It was uncertain how long nine patients had been ill, who were

delirious when I first saw them. Four patients were admitted into the wards in a state of complete stupor. The delirium continued till the death of the patients in thirteen cases, nine of which proved fatal severally on the 9th, 11th, 11th, 14th, 16th, 17th, 17th, 19th, and 20th day. The remainder of the forty-three patients either sunk into a state of absolute coma, or survived the termination of the fever.

The character of the delirium was usually far less active than that of the delirium of typhoid fever, the patients displayed less vivacity, and fewer of them, seven only of the twenty-four, *i.e.*, at the rate of 29·2 per cent. of those who were delirious after they were admitted into the hospital, attempted to leave their beds to roam in the wards.

SOMNOLENCE.—COMA VIGIL.

Typhoid Fever.—There was no somnolence in twelve cases; one of these proved fatal after the patient had been ill an uncertain time; eleven on the 12th, 17th, 17th, 21st, 25th, 25th, 27th, 30th, 32d, 46th, and 86th day after the illness commenced. These eleven being seen for the first time respectively on the 5th, 6th, 15th, 14th, 7th, 11th, 20th, 28th, 18th, 16th, and 9th day.

Somnolence was observed in eleven cases; of three of these I have no notes to show when it ceased, but it was profound when one was admitted on the 6th day; in four cases which proved fatal after the termination of the fever, somnolence began on the 15th, 22nd, 25th, and 26th day, and ceased on the 19th, 28th, 31st and 27th day, the patients dying respectively on the 44th, 71st, 34th, and 46th day; in three cases somnolence commenced on the 14th, 23rd, and 27th day, and continued till the death of the patients severally on the 23rd, 28th, and 30th day; one patient was somnolent when the case was taken on the 15th day; continued so till the 23rd, and died on the 33rd of pleuro-pneumonia.

The intensity of the somnolence varied from mere drowsiness to profound stupor. I have not included among the cases affected with somnolence, those of the patients who slept during a portion of the day, but who had been awake a great part of the night. *Coma Vigil*, as described under the head typhus, was not present in a single case.

Typhus Fever.—There was no somnolence in sixteen cases; six of these survived the fever. The duration of the disease prior to the admission of four of the sixteen cases, was unable to be ascertained. The remaining twelve were admitted into the hospital, one on the 6th, four on the 7th, four on the 8th, one on the 9th, one on the 12th, and one on the 14th day of the disease, and (those that lived after the termination of the fever being omitted) died, one each on the 10th, 11th, 12th, and 14th days, and three on the 16th day.

Somnolence occurred in twenty-seven cases; in eleven of them on the 7th, 10th, 9th, 11th, 11th, 11th, 12th, 12th, 12th, 13th, and 13th day; in seven before the 10th, 10th, 13th, 14th, 14th, 14th, and 15th day.

It continued in twenty cases till death, which in eleven of the twenty happened during the latter part of the second week, *i.e.*, from the 9th to the 14th day inclusive; in four during the third week, *i.e.*, between the 15th and 20th day; in five of the twenty cases the duration of the disease was unknown. In three cases the somnolence terminated in coma vigil.

Coma Vigil.—By this term I intend to express that peculiar condition in which the patient lies with his eyes open, evidently awake, but indifferent or insensible to all going on around him, and not what some writers on fever have meant by the expression, viz., that state in which a patient lies asleep for hours, and yet declares on awakening that he has never closed his eyes.

Nine patients, or more than one fifth of the forty-three patients experienced coma vigil (as I have above defined the expression) from one to four days immediately preceding their death. These nine cases terminated fatally on from the 10th to the 19th day, and in two cases at an uncertain period of the disease.

RELATION BETWEEN HEADACHE, DELIRIUM, SOMNOLENCE, AND COMA VIGIL.

Typhoid Fever.—The two patients who from the outset of the disease were free from headache, had violent and long-continued delirium, but were up to the time of their death exempt from somnolence.

In two cases only was there any headache after delirium commenced.

One patient, who died after the termination of the fever, was violently delirious on his admission on the 15th day of the disease, and profoundly comatose on the 16th; another, delirious and sleeping less than natural on his admission on the 15th day, continued in the same state till the 26th day, when he became very drowsy and remained so for twenty-four hours, after recovering from this slight somnolence he slept tolerably well till his death, on the forty-sixth day from the commencement of his illness.

The delirium continued after the somnolence began in all the cases, and did not disappear till the death of the patient, unless the somnolence passed into coma.

In one case the delirium was considerable and long-continued, the somnolence trifling.

There was headache in one case, in the course of which there was neither somnolence nor delirium.

Typhus Fever.—Of the two patients whose histories rendered it probable that they had no headache, even from the outset of the disease, and certainly they could have had but little, one suffered from violent delirium, and the other from somnolence terminating in coma vigil.

The headache disappeared before the delirium began in every case

in which the latter commenced after the patient's entrance into the hospital.

Of ten cases accurate information was obtained respecting the date of the commencement of somnolence and delirium; in nine of the ten the delirium preceded the somnolence, in one only did the somnolence precede the delirium.

Delirium was unaccompanied by somnolence in five cases, but two of these had coma vigil. Somnolence without actual delirium occurred in six cases; coma vigil without delirium in three; somnolence and delirium conjoined in nineteen cases.

Delirium preceded coma vigil in five cases, the delirium in one being accompanied by somnolence; this peculiar condition was preceded by somnolence without delirium in one case, mental confusion not amounting to actual delirium preceded it in two cases, and it was present when one case entered the hospital.

SPASMODIC MOVEMENTS.

Typhoid Fever.—Spasmodic twitchings of the muscles of the face or arms occurred in six of twenty-one cases; picking of the bedclothes in two. In no case were there general convulsions.

Typhus Fever.—Involuntary catchings of the muscles of the face, arms, or hands, were observed in twelve cases. In two of these the spasmodic action of the inferior recti muscles of the eyes, and of the levatores palpebrarum gave to the countenance a most remarkable aspect; the spasmodic movements were in both cases excited at any moment by suddenly raising either arm—the patients at the time appeared totally insensible to all going on around.

Two patient's suffered from general convulsions; in both cases they were repeated twice, at intervals of one and three days.

RETENTION OF URINE, AND INVOLUNTARY DISCHARGE OF URINE AND STOOLS.

Typhoid Fever.—The urine was retained in one case only, and the catheter had to be introduced for its removal on the 23d day of the disease.

The urine was passed involuntarily in ten cases; for the first time on from the 9th to the 27th day of the disease. Thus about one-half of the cases experienced involuntary discharge or retention of urine. Ten patients passed stools into the bed; this symptom was noted to be present for the first time on from the 13th to the 35th day of the disease. With one exception the urine was passed involuntarily into the bed at the same time as the stools.

Typhus Fever.—The urine was retained, and required the use of the catheter for its removal in eleven of the forty-three cases. The retention first occurred between the 10th and 21st day inclusive, and with two exceptions between the 10th and 16th days inclusive. It was passed involuntarily in twelve cases—this first took place on

rately ascertained; and as the state of the patient's general health forbade waiting for nature to throw off the dead portions, it appeared to me that the operation necessary for the removal of such diseased portions would be equally severe, and the effect less certain, than removing the internal half of the bone *in toto* at the articulation. Moreover, such operative interference might have been followed by disease of the new substituted bone, whilst removal of the internal half of the clavicle would remove the whole disease; and as the continuity between it and the acromial end was already broken, union being maintained only by ligamentous tissue, the inner half was really of no use to the patient.

The difficulties which in this case rendered the operation more protracted than I expected from numerous trials on the dead body, were owing principally to the previous disease having caused great alteration in the parts around the bone; these were condensed, and their appearance, relations, and attachments altered close to the position of the subclavian vessels. The firmness of the new attachments prevented the clavicle being drawn forwards from these vessels, and rendered a cautious dissection necessary to free that portion of the bone, the altered and irregular form of which, from the deposition of new osseous matter, was also a cause of difficulty, as it prevented me keeping the edge of the bistoury close to its posterior surface. I conceive that stage of the operation the most dangerous; but when the posterior surface of the bone is smooth, or when, after separating the muscular attachments and dividing the clavicle with the saw, that bone can readily be drawn forwards, and the edge of the knife kept close to the part to be removed, I should think the operation would be both more readily accomplished, and with much less risk.

As to the disarticulation at the sternum, that is effected without much difficulty, for when the incision of the integuments over the joint is free, and the rhomboid ligament attaching the clavicle to the rib wholly divided, the articulation is easily opened from above and anteriorly, and then, by using the bone as a lever, it can be twisted so as to divide the posterior part of the capsule, with scarcely any risk to the pleura or great veins; besides, the strong layer of the deep fascia, the origin of the sterno-hyoid and thyroid muscles, and generally a layer of fat, lie between the knife and these important parts; so that, if ordinary care be taken, they can scarcely be injured; and, if further security were required, the use of a probe-pointed bistoury, to divide the posterior part of the capsule, would, I conceive, preclude any chance of accident in this part of the operation.

CASE III.—*Amputation at the Ankle Joint.*

Jane P——, aged ten, of strumous constitution, had suffered from abscesses over the heel and inner ankle, which commenced about two years ago. I saw her for the first time in July 1847, when visiting another case at Coldstream. As her health then had not suffered much, I advised the use of a stimulating

Nine cases were at the time of their admission into the hospital in a state of extreme prostration; the duration of the disease, prior to the time they came under observation, being unknown.

(To be continued.)

ARTICLE III.—*Cases in Surgery, with Observations.* By JAMES SPENCE, Surgeon.

CASE I.—*Severe Injury of the Right Upper Extremity by Machinery; Amputation of the Shoulder Joint; Recovery.*

ON the morning of the 9th Dec. 1847, James Watt, millwright at a large paper mill in the vicinity of Edinburgh, when inspecting the machinery, perceived that a part of it was loose. While engaged in fastening the loose part, the sleeve of his jacket was caught, and the right arm dragged between two wheels, and rapidly crushed. Notwithstanding the fearful nature of the accident, he possessed sufficient presence of mind, on feeling himself drawn forwards, to take off his cap with his left hand, and place it in a position to check in some measure the revolution of the wheels, and at the same time he directed those about him to cut the sleeve of his coat, telling them that the arm was already torn through. He was extricated just in time to prevent the body being drawn in, but not before the soft parts on the right side of the chest had suffered severely. The accident happened about half-past four A.M., and I saw him at half-past seven A.M.

I found Mr Falconer and Dr Thomson in attendance. The patient was lying in bed, pale and exhausted, having lost a considerable quantity of blood; but though suffering great agony, he was quite sensible; the fragments of his clothes which had been left by the machinery had been removed, some stimuli had been administered, and external warmth applied; his pulse was about 120, and the skin was covered with cold sweat. The injured arm presented a frightful appearance; the limb was completely detached from about three inches above the elbow, and the humerus was again broken through obliquely immediately below the insertion of the deltoid, leaving the attachment of that muscle entire; but on the inner side, the fracture had splintered the bone up to within an inch of the joint; the middle part of the humerus, together with the soft parts, were hanging in shreds. On the right side of the chest, the integuments had been entirely removed to within two inches of the sternum; the clavicular and sternal portions of the pectoralis major had been completely torn off, so as to expose the pectoralis minor, the intercostal spaces, and externally the fascia covering the axillary vessels and nerves. The fragment of the humerus was drawn upwards and outwards by the deltoid, and the lower fibres of the great pectoral which were entire, were tightly stretched across the lower border of the large wound, and gave it the appearance of a deep dissection. Although from the nature of the injury, and the state of the patient, I had almost no hopes of his recovery, I thought it right to give him the only chance, viz. by amputation at the shoulder joint.

A small quantity of spirits and water having been given, chloroform was then administered. About a drachm was poured upon some lint and applied to his nostrils by Dr Thomson. He inhaled it readily, and scarcely a minute had elapsed ere it took effect: the fibres of the exposed muscles were first seen in violent action, and the face became suffused, the breathing became stertorous, the fibres of the muscles assumed a slight tremulous motion, and then became perfectly still; his pulse fell in frequency, but was firm; the full effect of the chloroform was obtained in three minutes.

Mr Falconer compressing the axillary artery between his fingers, above the lesser pectoral, I proceeded to amputate by forming my flap from the outer

and back part of the shoulder, where the integument and muscles appeared sound, saving as much skin as possible. From the short lever remaining, I had anticipated considerable difficulty in disarticulating the head of the humerus; but in this I was agreeably disappointed, for, owing to the complete relaxation of the muscles induced by the chloroform, I was enabled to disarticulate it rapidly and with the most perfect ease. The insertion of the lower fibres of the great pectoral, and the other soft parts on the inner side of the humerus were divided, and the fragment of the limb removed. The posterior circumflex and two other vessels were tied, and next the axillary at the point where my incision had divided it: perceiving, however, that this point was close to the origin of some large branches, and fearing, moreover, lest it might have suffered from the injury, I laid bare the vessel higher up, beneath the lower margin of the pectoralis minor, and tied it there also; after removing carefully all doubtful-looking portions of muscle and skin, the flap was brought forward and found to cover the wound much better than I expected; it was secured by several points of suture, supported by compresses placed posteriorly at the lower part to prevent dragging, some warm water dressing was placed over the surface anteriorly, and the whole secured by a split cloth bandage.

During the whole of the proceedings, which occupied about a quarter of an hour, the patient remained perfectly tranquil, and only became conscious after the dressing had been completed; a little wine and water were then given. He stated that he had felt no pain, and indeed knew nothing of the operation, but now felt much easier and revived; his pulse was at this time 68, and pretty firm: I left him in charge of Mr Falconer, desiring that he should have an opiate draught. According to Mr Falconer's report, the pulse gradually rose, and at three P.M. was 84 in the minute; there was considerable thirst, and he complained of darting pain in the wound, also of slight nausea, which, however, soon left him. In the evening he was less thirsty; the pulse continued the same, and he passed about six ounces of urine. Had a draught of thirty drops of solution of morphia at midnight.

At four A.M. on the 10th, his pulse was slightly accelerated and harder; he had slumbered occasionally for about half an hour at a time, but woke with pain in the wound; the skin was warm and moist, and after he had passed about six ounces of high coloured urine, he fell asleep, and rested for some time; at noon he had a laxative draught.

On the morning of the 11th he continued pretty easy, having passed a good night, but as his medicine had not acted, he got a dose of castor-oil. His tongue was white and furred. Before twelve o'clock his bowels had acted twice freely, and his skin was moist, and of a natural heat; he was allowed a little weak beef-tea. In the afternoon his pulse had risen to 100 in the minute, and was pretty full, though soft. The beef-teawas discontinued, and a little thin arrow-root allowed instead. In the evening he had passed water freely, and felt easy, but his pulse was still about 100, and the skin hot; had no headache. To have a draught of morphia and ipecacuan wine at bed-time.

12th.—7 A.M. Has passed a good night, pulse diminished in frequency and soft, and skin of a moderate heat.—3 P.M. Has suffered from sickness since morning. The wound was now dressed for the first time, and at his own request chloroform was administered; but after it began to affect him, vomiting took place, and Mr Falconer desisted from its use. The wound was found of a favourable appearance; suppuration had commenced at the lower part; a little wine was administered previous to the dressing; he felt much easier after its completion, when an opiate draught was given.

At 10 P.M. his pulse was 92, and soft; he still felt sick, and had not slept since the dressing. To have a large opiate draught.

13th, Monday.—Passed a bad night; suffered greatly from sickness and vomiting; pulse 96, tongue covered with white fur, but red at tip and edges; half a grain of solid opium was given him. At noon the bilious vomiting and sickness still continued; pulse 100, countenance anxious. To have one grain

of solid opium. At six P.M. he was much easier, the vomiting had ceased; he had taken a little thin arrow-root and drank some tea. As he had no evacuation from the bowels since Saturday, an enema was ordered, and after it had acted a grain of opium was given at 10 P.M.

On the 14th I saw him along with Mr Falconer, and dressed the wound, which I found uniting at the upper part, and granulating at the other parts of the wound, the suppuration was of a healthy character; he bore the dressing without much fatigue; his pulse previous to the dressing was 86 and soft, the expression of his face and general appearance good. He had had no return of the nausea or vomiting, but his tongue was still somewhat furred, and he complained of thirst. For the purpose of allaying thirst and abating the irritability of the stomach, I sent him some citrated alkali for effervescing draughts.

On the 17th, one of the ligatures came away, and on the 18th another, but without the slightest tendency to bleeding: from this time he continued to go on favourably; the wound healed rapidly, and no circumstance worthy of note took place, except a slight attack of bronchitis, which was promptly and actively treated, and he was able to leave his bed on the 1st of January 1848 for a short time, at which period a great part of the wound had cicatrized. On the 8th of January, the ligature from the upper part of the axillary artery separated (this ligature had been marked at the time of the operation by a double knot on its free extremity; the ligature from the lower part of the vessel, where it had been divided in the amputation, came away on the 17th of December).

In the beginning of February the wound was completely healed, with the exception of one part about the circumference of a sixpence, where a ligature projected, this I took away some time afterwards, and then this remaining part also healed up rapidly.

Remarks.—This case affords an example of the great amount of injury from which the system may recover; and the results of this and similar cases warrant us in entertaining hope and having recourse to operative measures even in the most aggravated cases. Few examples of injury could have presented a more frightful appearance, or less likelihood of a favourable result, not so much from the shattered state of the arm and the quantity of blood he had lost, as on account of the extent of the laceration of the soft parts covering the chest, one side of the chest being almost denuded of its cutaneous and muscular coverings, and the patient lying cold, pallid, and collapsed, from the effects of the severe suffering he had undergone.

From the state in which I found him, I have little doubt that, had he been removed even a short distance to obtain surgical aid, the result would probably have been very different. Indeed, from what I have seen of primary amputations, I believe that the very different results of such amputations in civil hospitals and military practice, and the comparatively greater success which will be found in private practice over hospital cases, is owing, in a great measure to the fact, that in military and private civil practice, the operation is performed either on the spot, or at a short distance only from where the accident occurred, so that the patient undergoes but one shock; whereas a majority of the cases presented to the surgeon in hospital practice, are often brought from a considerable distance, not in the easiest or most eligible conveyances,—and thus the patient just recovering

from the first effects of severe injury, is subjected to a second shock, or rather series of shocks, from the jolting of the shattered limb, before undergoing amputation; a condition which, in itself, apart from other considerations as regards the effects of hospital air, &c., is evidently less favourable than that of the patient to whom surgical assistance has been afforded without his having undergone a rough and exhausting journey.

There is another point to which I would direct attention, as I consider it of some importance to the prevention of secondary hemorrhage, in cases where there is extensive injury of the soft parts around the vessels. In severe accidents from direct violence by machinery, the parts even beyond the apparent injury often suffer indirectly, their vitality is impaired above the points of amputation, and extensive suppuration or separation of small sloughs takes place around the vessels, or the vitality of the arterial tissue itself may have been so impaired by the traction and twisting, that separation of the ligature will probably take place at a period before the canal of the artery has been obliterated, and secondary hemorrhage occur. It was to avoid such a risk in this case, that besides tying the axillary at the point, divided by the knife in amputating, I laid it bare and tied it higher up; and the result, I think, shows the propriety of the practice, for the lower ligature came away in eight days, so that, except for the ligature higher up, secondary bleeding would likely have occurred at that time.

In a case somewhat similar (reported in the *Lancet*, June 1848), but from the description, apparently attended with less injury of the soft parts of the chest, Mr Fergusson tied the axillary artery carefully at the point, divided in amputating. The patient went on favourably till the 9th day, when secondary hemorrhage took place from the axillary, Mr F. then tied the artery above the spot where it had given way; at five o'clock the next morning another bleeding occurred, and the house surgeon again tied the vessel half an inch higher up; at one P.M. hemorrhage again suddenly took place. Mr Simon cut down, and secured the subclavian quickly and skilfully; but the patient never rallied, and died a few minutes after the subclavian was secured. In Mr Fergusson's case there was not, apparently, any cause on account of the condition of the soft parts, for securing the artery otherwise than in the ordinary manner; for no one would advocate cutting down and tying the subclavian or axillary in every case of primary amputation at the shoulder for injury of the arm; but when secondary hemorrhage did occur, would the patient not have had a better chance, if, besides tying the axillary immediately above the point where it had given way, the subclavian had been also secured at that time, so as to diminish the impulse of the blood upon the weaker point of the vessel? In the Number of this Journal for February 1848, I have given the history of a successful case in which I tied the lower portion of the axillary for bleeding from the brachial artery at the bend of the

arm, following a severe burn, besides tying the vessel above and below the bleeding point; and I have there stated the principle on which I acted; the results of that case, and a late careful and more extended repetition of my former experiments on the general effects of the ligature of arteries, have also served to confirm me in the correctness of the particular principle alluded to, viz., that when we require to tie an artery, at a point where the vitality of its coats, or of the surrounding textures is much impaired, it is better at the same time to apply a ligature higher up where the vessel is healthy, so as to diminish the impulse upon the less healthy part of the vessel where the lower ligature is applied to obviate the risk of secondary hemorrhage, should that ligature separate too early, as is likely to happen.

CASE II.—*Disarticulation of the Sternal Half of the Clavicle.*

G. L., labourer, forty-eight years of age, and of strumous habit, had suffered for a length of time from epileptic fits, consequent upon an injury of the head, received eleven years ago, by a fall from a height. He stated that the clavicle had not been broken or injured at that time. In the beginning of December 1847, he was seen by Mr Falconer, Loanhead, on account of an abscess which had formed towards the outer part of the clavicle. This abscess was opened, and although the matter discharged was thin and unhealthy, no diseased bone could be detected by the probe. The wound healed in about a fortnight or three weeks. Shortly after it had cicatrized, the patient fell down in one of his epileptic fits, and on recovering he found a thick but sharp-pointed portion of the clavicle projecting through the thin recent cicatrix. Mr Falconer states, that the projecting portion of bone was of diseased appearance when it first protruded; and when I saw it, about a week afterwards, in the beginning of January 1848, the exposed portion was rough and apparently necrosed. After this, although the parts were kept in perfect rest, abscesses formed at various points near the clavicle, about the root of the neck, and in the supra-clavicular space, in the vicinity of the subclavian vessels; also above the sternum. Near the sternum there was a small opening, through which the bone could be felt rough and bare; and the other abscesses, although the probe could not detect any direct communication with the diseased bone, evidently depended upon the irritation excited by its presence. I saw the patient two or three times after this, but the exposed portion of bone did not become loose; and it resisted every effort to detach and remove it.

After waiting for nearly three months in expectation of the necrosed portions becoming loosened by the natural process, and finding it still impossible to detach and extract it from the rest of the bone, whilst the man's health was rapidly sinking under the constant discharge and hectic, I proposed the removal of the whole internal half of the clavicle from its sternal articulation, so as to include all the diseased bone, to which proposal the patient at once assented.

On the 11th April 1848, I proceeded to excise the diseased portion of the clavicle, assisted by Messrs Goodsir and Falconer, and in the presence of Dr Smith, Messrs Harvey and Edwards. Chloroform having been given, I made an incision about three and a half inches along the inner half of the clavicle, commencing about four lines internal to the sterno-clavicular articulation, and then made a perpendicular incision about an inch and a half in length across the former, at the point corresponding to the sterno-clavicular articulation. The clavicular attachments of the sterno-mastoid and pectoralis muscles were next divided. In doing this there was pretty smart hemorrhage from numerous small vessels. I now divided the clavicle a little external to the exposed diseased portion, and tried to raise the divided extremity so as to pull it forwards

from the great vessels. I found, however, that I could not effect this, owing apparently to some muscular attachments, which had not been completely divided; these I divided fully, when I could see the bone thickened and irregular, owing to the deposition of new bone. I then dissected around this cautiously; and Mr Goodsir attempted to draw the bone forwards, by means of a pair of strong tooth forceps, which I had brought on purpose, in case of difficulty. The dense and firm structures, however, prevented it being raised, so that I had to dissect the extremity of the bone cautiously, as the condensation extended as far inwards as the position of the great vessels. Whenever the mass of condensed tissues was wholly divided, the bone was raised and drawn outwards and forwards from the chest; then, by keeping the edge of the bistoury directed obliquely towards the clavicle, I divided the rhomboid or costo-clavicular ligament, and the other remaining attachments, and by using the free portion of the clavicle as a lever, I was enabled to disarticulate it readily from the sternum. Although there had been smart bleeding during the performance of the operation, only three small vessels required ligature. The consolidation of the parts, and the diseased state of the skin, prevented the greater part of the edges of the wound being united by suture, and in the site of the removed bone there remained a very ugly looking fossa, in which the pulsation of the great arteries could be seen and felt. This cavity was filled slightly with charpie, and supported by a bandage. After the patient was placed in bed, and had recovered from the effects of the chloroform, an opiate draught was exhibited.

I saw him again on the 15th of April, when he was suffering from an attack of bronchitis, to which he had been long subject. He had a constant harassing cough, but was expectorating freely. The impulse of the coughing was communicated strongly to the wound, which was suppurating freely, and healthy granulations were beginning to spring up. With the exception of the cough, he was going on favourably. Warm water dressing was applied to the wound, and he was allowed nourishing diet.

As the accounts I received from Mr Falconer continued favourable, I did not visit him again till the 24th of April; the cough had then almost entirely left him, and he was sitting up with the arm in a sling; the wound was covered with granulations, and contracting rapidly. For a few days previous to my visit, the red lotion had been applied to the wound. From this time I did not see him again till the 7th of May, when the wound had completely cicatrized. Little treatment beyond the occasional application of escharotics to the exuberant granulations had been required; his general health and appearance had much improved, and he now slept well at night, and was free from pain.

I saw him again in September; there was very little falling in of the shoulder of the side from which the clavicle had been removed. His general health continued good, and he had for some months been at his usual employment of a labourer.

Remarks.—The removal of the external half of the clavicle in this case was, I consider, sufficiently warranted by the state of the patient, the constitutional irritation he was suffering, and the repeated formation of ill-conditioned abscesses at the root of the neck, and in dangerous proximity to the large vessels. Under these circumstances there was distinct indication for operative interference, either by removing the diseased portions of the clavicle, or by removing the whole sternal half of the bone at the articulation. I preferred the latter operation for the following reasons:—The dead portion which presented itself was not loose or detached, and the probe introduced through the sinuses detected other dead portions nearer the sternum; so that the full extent of the disease could not be accu-

from the 8th to the 20th day; in two cases the retention of urine was preceded for a day by its involuntary discharge. Either retention or involuntary discharge of urine was a symptom in twenty-one or in nearly one-half of the patients; and as no notes on the point were taken in seven cases in which the prostration was extreme, it is probable that considerably more than one-half were thus affected.

The stools were passed involuntarily in seventeen cases; the day of the disease on which this symptom first occurred was from the 8th to the 21st; it was in every case accompanied by an involuntary discharge of urine.

LOSS OF MUSCULAR POWER AFTER THE PATIENT'S ADMISSION INTO THE HOSPITAL.

Typhoid Fever.—Two patients were able to leave their beds unassisted and with facility throughout the whole course of the disease. One of these patients died on the 25th day of the disease, and the other five weeks after her admission.

Two were able to leave their beds with tolerable facility respectively up to the 15th and 24th day of the disease, but the former on the 16th and the latter on the 26th day, were unable, without assistance, to reach the close stool placed immediately adjoining the bed.

Five patients could, though with great difficulty, get out of bed unassisted, from the 13th to the 30th days, while ten were quite unable from the 5th to the 26th days; there was extreme prostration in eight cases from the 14th to the 30th days.

Of four patients my notes were incomplete.

Two others were admitted in a state of extreme prostration after the disease had lasted an uncertain period.

Typhus Fever.—One patient only could leave his bed with tolerable facility through the whole course of the disease; this was a very mild case in every respect, and the patient died of phlebitis some time after convalescence from the fever; two with difficulty from their admission on the 8th and 7th day of the disease, till the death of one on the 14th, and the recovery of the other, who eventually died of smallpox.

Twelve patients were unable to leave their beds unassisted from the 6th to the 14th day.

In thirty-four cases the prostration was extreme; in a large majority of these cases the prostration became extreme between the 9th and the 12th day. In one case the patient, though able to get out of bed unassisted, with difficulty, on the 5th day of the disease, was quite unable to leave her bed unassisted on the 6th, and on the 9th day was in a state of extreme prostration. In one case only the prostration was not extreme till as late as the 17th day of the disease.

astringent lotion to the ulcer, and the employment of cod-liver oil internally. Under this treatment she seemed to go on favourably for a time, but towards the end of June 1848, the foot became worse; fresh collections of matter formed; and there was constant pain, increased at night, preventing sleep. When I saw her again in August last, her general health had begun to suffer; there was then a considerable elastic swelling over the heel and inner ankle, and an ulcer about the size of a shilling was situated between the inner malleolus and heel. The integuments for some distance around the ulcer were of a dark purple colour, and the subcutaneous tissue was condensed, and swollen with sinuses passing towards the joint. The edges of the ulcer were ragged and irritable; the centre was occupied by a yellowish slough. Amputation was evidently indicated, but the appearance of the soft parts, and the position of the ulcer corresponding to the posterior tibial vessels, rendered it questionable how far it was right to amputate at the ankle joint. Desirous to give her the advantage if possible of amputation at the ankle, I had her brought to town, that I might carefully watch the case.

She came to Edinburgh on the 16th August, and having examined the foot several times, I determined to amputate at the ankle, which I did on the 18th. I employed a modification of Mr Syme's plan of operation, suggested to me by my friend Mr Goodsir. This modification consists in first disarticulating, and then carrying the knife downwards behind the os calcis, so as to form the plantar flap from the heel from above downwards, instead of raising and dissecting that flap upwards. Chloroform having been given, I commenced in the usual manner, with an incision across the anterior aspect of the joint, from the external to the internal malleolus. The knife was then carried deeply across the plantar aspect of the heel, round from the internal to the external malleolus, where the incisions commenced. The form of the flap being thus marked out, I disarticulated by dividing first the external lateral ligament, and then carrying my knife close to the bone I divided the internal lateral ligament, and separated the soft parts on the inner side of the foot, which was now turned forwards. The tendo Achillis was then divided, and by carrying the knife close round the back part and sides of the os calcis, that bone was readily turned out from the plantar flap. The malleoli were removed by the saw, and the operation completed in the usual manner. Two arteries only required to be secured, one towards the outer side of the plantar flap, and the anterior tibial artery. After closing the wound by interrupted suture, I dressed the stump with two long and broad strips of lint, arranged in a crucial manner, and secured by a circular slip, so as to support and press the flap against the surface of the bones, and the patient was put to bed. [This method I have used in several cases of amputation, and found it quite as effectual in supporting the flaps as compresses and bandages. It is more simple, and by keeping the lint slightly moist with tepid water, it can be easily removed in case of hemorrhage occurring. The subsequent treatment is thus facilitated, and much pain is saved to the patient.]

On examining the foot, the soft parts around the ankle joint were found thickened and infiltrated, and the sinuses from the small ulcers opened into the joint. The articular cartilages covering the lower end of the tibia, and corresponding surface of the astragalus, were affected with the brown degeneration, and extensively ulcerated. The bones were denuded, and carious at several points. A section of the os calcis, shewed its external fibrous plate reduced to a very thin shell. The interior was occupied by the soft greasy degeneration common in scrofulous bones, and contained a small portion of dead cancellated texture lying loose in the centre.

It is unnecessary to give the daily history of the case, as every thing went on favourably; the incisions healed rapidly; the treatment required was principally for the old ulceration of the soft parts, which continued very intractable;

it healed well for a day or two at first, and then again became sluggish ; under the use of nitrate of silver, however, it began to improve more steadily.

On the 4th of September, the incisions having been healed for some time, a small abscess formed at the outer part of the heel, which was opened ; and some boils began to appear on different parts of the body. Under these circumstances, as nothing remained but the ulceration of the skin, and as she was now able to sit up and had been out, I thought it advisable to send her home to the country. By a letter which I received from Dr M. Turnbull, dated the 22d January 1849, it appears that for a long time there was little appearance of the ulcer healing until about the end of December, when Dr T. had applied a lotion of sulphate of copper, under which it improved rapidly, and at the date of Dr T.'s letter the ulcer was nearly well, and her general health was good. Since that time I have heard that she is well and going about.

Remarks.—The plan of operation adopted in this case, seems to have the advantage of being performed more rapidly and with less twisting and manipulation of the plantar flap, than when we dissect that flap from below upwards, as in the ordinary method. The form, position, and connexions of the flap, are the same in both methods. In other words, it seems a simpler and more easy method of producing the same result.

The great advantages of the method advocated by Mr Syme, for amputation at the ankle, over all others hitherto proposed, consist, I think, 1st, in forming the flap from the elastic, fatty, and cutaneous tissues of the heel, parts adapted by nature to sustain pressure ; and, 2d, from the lateral connexions of the flap preserving the vessels on both the internal and external sides of the ankle—thus insuring a sufficient vascular supply. Any rapidity or ease in operating, which sacrificed either of these advantages would be too dearly purchased, and most of the plans hitherto proposed, do so to some extent. Thus, for example, the plan of a large flap cut from the internal malleolar and plantar region, as proposed by M. Sédillot and others, besides sacrificing a portion of the plantar tissue, has, moreover, the great disadvantage of dividing the external lateral vascular connexions, and so, to a certain extent, endangering the vitality of the flap. For it is an anatomical fact, familiar to every one accustomed to dissect minute vascular preparations of the foot, and one which ought not to be lost sight of, that the plantar tissues of the heel which form the flap, receive at least quite as much of their arterial supply from the terminal ramifications of the external malleolar, and interosseal branches of the peroneal artery at the ankle, as from the posterior tibial above its division into the plantar arteries, and it is of importance to the vitality of the flap to preserve these sources of supply as well as that from the posterior tibial.

The plan of operation, or rather the modification of Mr Syme's plan used in the foregoing case, preserves all the advantages of his method, whilst I certainly found it more easy and rapid of execution than when I have operated in the usual manner by dissecting the flap from below upwards, before disarticulating.

ARTICLE IV.—*Loose Cartilages in the Elbow-Joint removed successfully.* By SAMUEL SOLLY, F.R.S., Senior Assistant-Surgeon at St Thomas's Hospital. To which is added a Microscopical Examination of these bodies. By GEORGE RAINEY, Demonstrator of Anatomy.

JOHN MOORE, æt. forty-nine, a river pilot of healthy appearance and regular habits, came under my care as an out-patient at St Thomas's hospital in the month of August 1848. He complained of stiffness and pain in the left elbow joint, which was much swollen, and presented the general character of disease of articular cartilages. As I found that after about a month's attendance as an out-patient, very little improvement had taken place in the joint, I recommended his coming into the hospital, explaining to him that entire rest to the joint and careful treatment could alone save the limb.

He was admitted under my care on the 12th of September 1848, and his condition was thus described on admission. "The left elbow is considerably swollen and very stiff; he is quite unable to bend it, and the slightest motion gives him great pain."

Treatment.—A seton was run through the skin on the inner side of the arm, just above the elbow. Iodine with iodide of potassium was administered, and he had three grains of acetous extract of colchicum, with Dovers' powder at bedtime. On the 25th a blister was applied to the joint. The above treatment was persisted in till the 10th of November, without benefit; mercurials were then had resort to, and the bowels being irritated by calomel, I prescribed five grains of grey powder every night. This produced slight salivation in about three weeks, at the end of which time the appearance of the joint was completely altered. Instead of a nearly uniform swelling over its whole surface, I found on one of my visits, to my great surprise, that there was a small circumscribed swelling above, and continuing a little behind the inner condyle. This appeared to be a sac, and I could feel that it contained several loose hard bodies, very moveable, and conveying a very peculiar crackling sensation to the touch. The general tenderness is gone, and the examination of this part gives him no pain; but moving the joint causes him considerable pain, and its mobility is very much interfered with. He says that the little bodies slip about under the skin, and click against each other.

He now states that he first noticed a small lump in this situation last January; it was not painful, and did not interfere with the use of his elbow, but after a little while it became larger and very painful. He says that he was attacked with slight rheumatism about three years ago, but that it went away without medical advice. In June last he was again attacked with severe rheumatism, when he was admitted at this hospital under Dr Goolden, and his disease is described in the ward book as follows:—"Inflammation of the left

elbow joint connected with mercurial periostitis. There is considerable swelling of the left arm from the elbow to the wrist, throbbing especially at night, and the whole forearm is œdematous. It commenced with a small swelling about the size of a nut at the olecranon." The treatment consisted in the withdrawal of blood by means of leeches, in the administration of iodide of potassium in the compound decoction of sarsaparilla, and in the application of iodine ointment to the joint.

On the 7th of July he was removed into one of the surgical wards, and placed under Mr Le Gros Clark, whose treatment also consisted in the administration of iodide of potassium in the compound decoction of sarsaparilla. After a month he left the house, and he then came under my care as an out-patient.

After the discovery of these bodies, I began to consider how far it would be safe to remove them. I could not ascertain distinctly whether the sac in which they were contained communicated with the joint; the manipulatory examination did not give me that impression, though from the history of the case it was most probable that it did. I had a consultation with Mr Green on the propriety of an operation, and he agreed with me that it was desirable. This point appeared to be quite certain, that these bodies had been, and in all probability would be again, the exciting cause of all the inflammation of the elbow joint and forearm under which he had been labouring.

I ordered him a dose of rhubarb and calomel, and on the 11th of November 1848 I operated.

Operation.—I made an incision about one inch in length over the swelling above the inner condyle, dividing the skin, fascia, and synovial membrane, till I exposed the loose cartilages, for such they proved to be. They were easily pressed out of the opening. I introduced my little finger into the sac, and felt the articulating surfaces of the humerus and radius, and I thought I felt an irregular surface on the ridge between the radius and ulna, like an attached portion of false cartilage, but there were no more loose bodies in the joint. Those removed were eight in number. I brought the edges of the wound into close apposition, put one suture in through the skin, and the isinglass plaister over it. I placed a pasteboard splint on the back of the joint, fixing it to the upper and fore arm, so as to prevent the slightest motion in the joint. The wound was quite healed in forty-eight hours, but I kept the joint at rest for a week; after this he was able to move it without the slightest pain—indeed, he did not suffer in any way after the operation, except for the confinement occasioned by the splint. I have seen him repeatedly since he left the hospital, and he has remained quite well and resumed his occupation on the river.

I submitted these bodies to the examination of Mr Rainey, and to him I am indebted for the following excellent description of their structure and mode of development :—

"These bodies," says Mr Rainey, "have a distinct investing membrane, which on its external surface is smooth, but by its internal one is so intimately connected to the body itself, as to admit of being detached only in small shreds. This membrane is composed of fibro-cellular tissue, mixed with granular matter.

"Their internal structure, as exhibited by a section through the middle, is seen by the naked eye to consist of two distinct substances, one being semi-transparent, like fibro-cartilage, the other being perfectly opaque and white, like bone. The former, under the microscope, presents the appearance usually seen in fibro-cartilage. The latter resembles remarkably in its ultimate structure those bones which consist of only one bony plate placed between two folds of membrane, as the thin plates of the ethnoid bone in the nose. In these bones the lacunæ, as in the opaque parts of the body before mentioned, are the same as in other bones, but there are no distinct or well-formed canaliculæ branching out from them. There is in both a stellate arrangement of the earthy matter around the lacunæ, but nothing like canaliculæ; and this appearance is more striking in the bones alluded to, than in the earthy part of these bodies.

"I believe no satisfactory explanation has yet been given of the manner in which these loose bodies are formed in joints, although I think their origin, and the circumstance of their becoming loose in a joint will appear obvious, by a reference to the remarkable character of the epithelium in joints, in the thecæ of tendons, and in mucous bursæ. This epithelium was described by me in a paper communicated to the Royal Society by Mr Simon about three years since, but which has not been published. A few general observations upon this structure will suffice. It is situated in those parts of a joint, theca, or bursa, where it is least exposed to pressure. It consists of loops and convolutions of capillaries of various degrees of complexity proceeding from the surrounding vessels, and projecting into the synovial cavity. These capillaries are enclosed in sacculi of basement membrane, studded with minute oval cellules; from the sacculi enclosing the capillaries, numerous other sacculi, into which no capillaries enter, proceed: these are of various forms and sizes, but generally they are attached to the primitive sacculus by an extremely long and slender filament of fibrous tissue, resembling the petiole of a leaf, the secondary sacculi being its expansion. Sometimes there are several series of these sacculi attached, one series to another, exhibiting an arborescent appearance, but in every instance the secondary sacculi are extra-vascular.

"I have found this kind of secretory apparatus in all the joints, as for instance the finger joints, the knee, shoulder, &c. Now this being the apparatus by which synovia is elaborated in all parts in which this fluid is found, and the bodies just described being found in these situations, they may be inferred to be the product of disease in these structures; the cellules of these fringes, in the place of elaborating synovia from the blood, producing, under the influence of

morbid action, other products, such as cartilage, which becomes converted into imperfectly formed bone. The fact of the secondary sacculi being connected to the primary by extremely slender pedunculi, will suffice to explain the reason why these bodies may become formed in the first instance, the pedicle serving both to keep them attached, and to convey the material from the blood necessary for their development, until they acquire a certain size; but afterwards, from its tenuity, becoming no longer capable of holding them, it breaks, and the bodies become loose, and most probably cease to enlarge."

ARTICLE V.—*Case of Fracture of the Pelvis, with Penetrating Wound of the Bladder.* By JOHN TAYLOR, M.D.

(*Read before the Medico-Chirurgical Society on March* 7, 1849.)

J. P., labourer, aged fifty-eight, was engaged on the afternoon of Monday 22d January ult., in taking down a stone wall. For the purpose of hastening the operation, he had undermined a portion of it, and in doing so, had formed a small trench at its base. Having ceased digging for a time, he raised himself for the purpose of examining the state of the wall, and whilst he was in the erect position, the wall gave way, burying him in its ruins. He fell backwards on the small mound of earth formed from the excavation; when first found, he was covered with the stones up to nearly the top of the chest. His position, as described by those who saw him then, was as follows:—the upper part of his body was lying with the back flat against the mound of earth, the lower part of his body was twisted, so that the pelvis rested on its side, the mark of one hip and trochanter being impressed on the soft earth. Whilst lying under the rubbish, he complained principally of the great weight, and of pain in the left shoulder; on being freed, he was carefully raised and put upon his feet, stood without any support, and raised his legs successively in order to satisfy himself that he had sustained no injury of these limbs. I first saw him some hours after the accident; he was lying on his back in bed, and complaining greatly of pain in his left shoulder, but nowhere else; he was cold and somewhat collapsed, his pulse sixty-four and small. On examining the shoulder, I discovered a fracture of the left clavicle, which was adjusted in the ordinary manner; hot bottles and warm drinks were ordered, and a dose of castor-oil directed to be given in the evening.

Tuesday morning. Had passed a tolerable night, but said he was now feeling his other bruises; of these he referred principally to the right thigh in the neighbourhood of the trochanter. The limb was somewhat swollen and discoloured, no fracture of the neck of the bone could be discovered, but he complained of pain on pressure over the trochanter; to relieve the pain simple fomentations were ordered; the bowels had not been moved, tongue clean and moist, skin natural heat, pulse still sixty-four and small; laxative medicine directed to be repeated.

Wednesday. Had passed a restless night, and complained much of pain in the abdomen, which had commenced about midnight without any rigor. On examining the abdomen, it was found swollen and painful to pressure over the hypogastric region, conveying somewhat the impression of the bladder being distended, but being somewhat more diffused, and the abdominal parietes feeling hard and doughy. On enquiring when he had passed water, he said that he had passed none since the accident, and had no urgent desire to do so, but felt a general uneasiness and restlessness. Having no catheter with me, the urine was not drawn off just then, but in the course of the afternoon I asked

my friend Mr Spence to see him. We found him much in the same state; on introducing the catheter, which was done without the slightest difficulty, about seven ounces of perfectly clear urine were drawn off. Mr S. examined the perinæum, and also the bladder by the rectum, but there was no apparent injury of these parts, and the bladder was felt empty. No great relief was afforded by drawing off the urine; he stated in answer to questions, that he had felt no desire to empty the bladder immediately before the accident occurred.

Thursday. Passed an uneasy night, bowels still constipated, breathing hurried and oppressed, pulse seventy and small, tongue moist, and furred towards the back part, no urine passed naturally. I drew off four ounces of clear urine by the catheter, but still without any relief to the pain or swelling of the abdomen, for which leeches were ordered, calomel and opium exhibited internally, and the abdomen to be well fomented. I again saw him at 4 P.M., and drew off about three ounces of clear urine; no change since last report, except that the breathing was hurried and oppressed; no examination could be made of the posterior part of the chest, owing to the difficulty of moving the patient; loud sonorous rales were heard over the whole anterior portion masking the sounds of the heart. 10 P.M., complained of pain at lower part of left side of chest, in other respects much the same; had passed a little urine naturally with an effort; the catheter was again introduced, and about three ounces of clear urine drawn off, after which he complained of the feeling as if the bladder contracted on the instrument.

Friday. Mr Spence again saw the patient along with me, he was still very restless and depressed, the features anxious and contracted; his bowels had been freely moved by an injection the previous evening, and he had also passed a little water once since last report, but had failed in doing it again, although he had made several attempts. On proceeding to introduce the catheter, the scrotum was found of a dark colour and distended, and the penis retracted as from infiltration of urine, but there was no swelling or infiltration in the perinæum, nor at the root of the scrotum; free incisions were made into the scrotum, and the catheter was again introduced, and from eight to ten ounces of perfectly clear urine were drawn off, being the largest quantity yet obtained; respiration still hurried and laborious, and still complains of pain in the left side. I again saw him at 4 P.M., and drew off a small quantity of urine. *Evening.* Much in the same state and general condition as at last report, but pulse had risen to eighty-four, though still small and weak; the incisions had given vent to a quantity of fluid of a urinous odour, and the scrotum was now flaccid.

To shorten the case I may mention, that from this time he gradually sunk without any material change of symptoms; the urine was regularly drawn off three times a-day; it never was in large quantities, and always clear, with the exception of the last time it was drawn off, a very short period before his death, which occurred on Sunday night about 11 o'clock, when a few grains of chocolate coloured flocculi were seen mixed with the urine.

Post-Mortem Examination.

Thorax.—The right lung was of a mottled greyish colour, and crepitated under the pressure of the fingers; there was slight emphysema at the lower part, but with the exception of that and one or two old adhesions of the pleura it appeared healthy. On the left side there were extensive and close adhesions between the pleura costalis and pulmonalis, but evidently not recent. The left lung was of a dark colour throughout; at its lower and posterior part it felt firm and doughy, and when pressed exuded freely dark coloured frothy serous fluid; the heart and blood-vessels presented no unnatural appearances.

Abdomen.—External appearances.—There was considerable discoloration about midway between the pubis and umbilicus in the mesial line; over the right

side of the pubis, there was a small superficial ecchymosed spot, about an inch long by five or six lines broad.

Parietes of Abdomen.—The section of the parietes exhibited traces of considerable ecchymosis into the substance of the recti and pyramidales muscles, and also effusion of blood between these muscles and the parietal peritoneum, about two and a half inches above the pubis.

Cavity of Abdomen.—The general peritoneal surface presented no increased vascularity, and contained only a very small quantity of clear serous fluid, unmixed with any flakes of lymph. An old adhesion stretched from the root of the transverse meso-colon, in front of the mesentery and small intestines (which it partially hid) to the right iliac fossa. The intestines presented no appearance of injury or disease.

Pelvic Cavity.—The peritoneum, over the last lumbar vertebra, and the promontory of the sacrum, and corresponding to the right sacro-iliac synchondrosis, presented a dark appearance, as if from the effusion of blood between it and the subjacent parts (which was subsequently found to be the case). The bladder, which was contracted, was placed more obliquely than usual, its apex or superior fundus inclining considerably towards the left side ; the upper part of the rectum was also inclined to the left side. The peritoneum investing the posterior and lateral parts of the bladder, and also its recto-vesical reflexion, was of a dark greyish colour. On raising the peritoneum from off the posterior part of the pubis, the pelvis was seen to be fractured, though the bones could scarcely be said to be at all displaced when first examined. The fracture of the pubis on the right side commenced about an inch and a half from the symphysis, running obliquely through the horizontal ramus of the pubis, and separating a long shelving portion or spicula from the posterior part or inner table of the body of the pubis. On the left side there was fracture of the horizontal ramus of the pubis, and at the upper and inner part of the obturator foramen ; this portion was separated by a fissure from the rest of the bone, but not displaced, being retained in situ by the obturator ligament and fibres of the obturator internus, which required to be dissected off before the nature of the fracture could be seen, and the portion removed : this piece of bone has been kept, and by looking at it and the other preparation of the pelvis made to illustrate the nature of the fracture, a better idea may be formed of the injury than from description. There was partial diastasis at the right sacro-iliac synchondrosis. The arch formed by the symphysis pubis, and by the rami of the pubis and ischium, was entire and in situ ; for though on the right side there was a fissure a little behind the junction of the ramus with the tuber ischii, the strong ligamentous attachments retained the parts perfectly in connexion, and the perinæum presented no appearance of any injury. The bladder was next carefully examined, and presented two lacerated wounds at its superior and anterior part. The sub-peritoneal tissue in the pelvis was filled with blood mixed with a thin fluid ; this was carefully removed, when both obturator arteries and veins were found to have been torn ; the obturator nerve on the left side was much stretched and ecchymosed, but not torn.

The bladder and the surrounding parts were removed for more careful examination. The wounds in the bladder were situated at its anterior part, below the reflection of the peritoneum, and were nearly in the same line, the one at two inches, the other one and three quarters above the prostate ; the distance between the two openings was one inch and three-fourths. On opening the bladder by an incision at the upper and anterior part, it was found quite empty ; the mucous membrane presented no unusual appearance except in the immediate vicinity of the wounds, where there was slight ecchymosis beneath the mucous membrane around the orifices. In dissecting the parts around the bladder, they were found infiltrated with a thin dark fluid of a urinous smell, and there was also some sloughy cellular tissue and thin purulent matter situated over the lower and fore part, above the prostate, and by the sides of the bladder.

Remarks deduced from Post-mortem Appearances.—From a consideration of the anatomical relations of the parts, the form and seat of the fractures in connexion with the position of the wounds in the bladder, it appears to me, that though the bones on the right side were in situ at the time the body was examined, yet in the first instance, when the accident occurred, the lower part of the body may have been twisted on its side, and the force of the falling mass applied to the right os innominatum, so that its weight would separate and press inwards the oblique shelving spicula, causing it to perforate and transfix the contracted bladder at the points seen in the preparation. But when the weight was removed, and the patient placed either erect or on his back, there would be a natural tendency of the bones to resume their normal position, owing to the resilence of the parts, and the anatomical connexions of the iliac portion of the os innominatum, the weight of the limb attached to it tending to fall outwards. From the nature of the fracture or rather fissured separation of the portion of bone on the left side, it was impossible that it could have produced injury of the bladder, for it did not project in the slightest, and the soft parts lining it and lying between it and the bladder were not torn through, indeed it required some little force to separate it completely from the rest of the bone.

In most cases of fracture of the pelvis there is generally considerable bruising of the soft parts, and severe pain in the place, which draw immediate attention to the injury. In the present instance, however, there were two circumstances which tended to divert attention from the seat of the principal injury, viz., 1st, the patient complained chiefly of pain in the shoulder and chest; and, 2d, it was stated both by himself and those who found him, that he had stood unsupported when extricated from the rubbish, and had, whilst so standing, moved the lower limbs to satisfy himself that they were uninjured. When attention was subsequently directed to the lower extremities, the pain was referred not to the pelvis, but to the thigh itself; and the marks of bruising, with the exception of the slight superficial ecchymosis mentioned in the post-mortem examination, were confined to the lower and inner part of the thigh. On the Wednesday, when pain in the hypogastrium was first complained of, and when the symptoms directed more particular attention to the urinary organs, suspicion was then entertained that the bladder had suffered injury, and that the pelvis might be fractured; still, judging from the external appearances, and the relative position of the iliac spinous processes, and the integrity of the rami of the pubis and ischium, as felt in the perinæum, there was evidently no displacement; and as no benefit in the way of treatment was likely to be gained by it, we did not subject the patient to minute examination, which could have been accomplished only with great pain and inconvenience. Whilst the general symptoms and the state of the urinary secretion led us to suspect injury of the bladder in the first instance, yet the urine continuing perfectly free from any trace of blood, considered in connexion with the quantities secreted, the subsequent infiltration of the scrotum, and the position of that infiltration, subsequently led us to diagnose that one of the ureters might have been ruptured, and that

whilst the secretion of the uninjured kidney was conveyed into the bladder, the urine secreted by the other had passed into and infiltrated the cellular tissue of the pelvis. As regards the late period at which the infiltration appeared in the scrotum, that may be accounted for in several ways; most probably, however, owing to the size and position of the wounds, and the ecchymosed state of their margins, the urine would not escape readily in any large quantity at first, except under the impulse of efforts such as those made by the patient in his attempts to pass water on the Thursday night and Friday morning, on which latter date, it may be recollected, the infiltration of the scrotum first appeared; or it may simply have happened that the urine which had escaped from the bladder, had, after first passing into the cavity of the pelvis, gradually filtered through the fissured portion of the pubis, and at length made its way into the upper part of the scrotum.

The history of this case, therefore, is both curious and instructive, as affording an example of a penetrating wound of the bladder without bloody urine; for if we except the few chocolate coloured flocculi the last time the water was drawn off, there never was the slightest trace of blood in the urine, nor of any bleeding from the urethra after the accident. And this, it appears to us, decidedly answers in the negative some remarks made by Dr E. Watson of Glasgow, on a case of supposed rupture of the bladder, in the February Number of the Edinburgh Monthly Journal. Dr W. there says, "The first urine obtained by catheterism was eighteen hours after the accident, and that urine was bloody, but never afterwards in the history of this case. Now, would not the urine have been bloody for several days after rupture of the bladder? I humbly think it would." The above case, at least, shows that it might never have been bloody at all.

In conclusion, we may mention, that this patient was a most unfavourable subject for treatment, as he had formerly suffered several very severe injuries, some of them of comparatively recent date. These were, fracture of the femur, fracture of the right scapula, and about nine or ten months previous to the last and fatal injury, fracture of several ribs on the left side, and he had then suffered from severe pleurisy.

[I beg to acknowledge here, what I had not the opportunity of doing at the society, my deep obligations to Mr Spence for his kind and able assistance in the treatment of the case, and for the description of the post-mortem appearances, which is entirely drawn up by him.]

Part Second.

REVIEWS.

On the Influenza, or Epidemic Catarrhal Fever of 1847–48. By Thomas Bevill Peacock, M.D., Licentiate of the Royal College of Physicians, Physician to the Royal Free Hospital, &c.

The object of Dr Peacock in publishing the present volume, is to give to the profession a comprehensive account of the recent epidemic, in so far as it fell under his personal observation. In a brief introductory review, he notices, amongst other subjects, the predominating class of diseases and the atmospheric peculiarities which preceded for some time the appearance of the influenza; the difference in time at which it appeared in different parts of the country, and in other countries than our own; the relative number of the community attacked, and the relative fatality of the disease. The numerous synonyms of the affection are then noticed; our author adhering to the name given to it by Huxham, namely *Epidemic Catarrhal Fever*, although the term *Influenza* has been so thoroughly sanctioned by general adoption, that he does not altogether discard it.

He treats the several varieties of the disease under the following heads:

1. Simple catarrhal fever.
2. Catarrhal fever with pulmonary complication; and,
3. Catarrhal fever with predominant disorder of the abdominal organs.

The first and second comprise those to which the term influenza is commonly applied, while the third is subdivided into three divisions, to which we shall presently revert.

The description given by our author of "simple catarrhal fever," is graphic, and extremely accurate. As, however, most of these cases require little medical aid, we proceed at once to the consideration of "catarrhal fever with pulmonary complication."

The author had twenty-eight cases of this nature under his care at the hospital; the local disease taking the form of—

Acute or subacute capillary bronchitis, in eleven cases;

Bronchitis supervening on tuberculous disease of the lungs, in four cases;

Bronchitis complicated by disease of the heart or aorta, in six cases; and

Pneumonia, in seven cases.

In eight of the eleven cases in which there was acute capillary bronchitis, the disease "commenced suddenly, for the most part with a feeling of cold between the shoulders and down the back, increasing to

slight shivering or severe rigors, and followed by heat of skin, more or less intense, and in some by perspiration. To these symptoms succeeded pain in the head, chest, and loins; a sense of tightness across the chest, difficulty of breathing, and cough, with usually some soreness of the throat." In two, the disease commenced with sickness and vomiting, combined in one of them with pain in the abdomen.

It is worthy of remark, that seven out of the eleven patients had for some time laboured under other pulmonary affections.

For a detailed account of the symptoms, and the *post-mortem* appearances in the cases complicated with capillary bronchitis, we must refer our readers to the work itself; we proceed to our author's remarks on the diagnosis of capillary bronchitis, when associated with catarrhal fever.

"In cases of influenza, when an unusual degree of dyspnœa, lividity of the face, and prostration of strength occur, we may with considerable probability conjecture the affection which is about to ensue, while the physical signs remain of an undecided character. It is only, however, at a later period, when effusion to a greater or less extent has already taken place, that the general symptoms and physical signs become sufficiently characteristic to remove all doubt as to the nature of the disease. The only affections with which capillary bronchitis can then be confounded, are the more ordinary forms of bronchitis on the one hand, and pneumonia on the other; and it should be borne in mind, that the disease is generally combined with some degree of both of these affections, and that its character is variously modified by their presence. The following remarks, therefore, only apply to its more characteristic forms:—

"From pneumonia, capillary bronchitis may be distinguished—

"1st. By the general symptoms. The less sthenic character of the febrile disturbance, which accompanies all stages of the disease; the greater degree of lividity of the face and extremities, the more rapid prostration of strength, and the earlier appearance of symptoms of asphyxia.

"2d. By the physical signs. The absence of dulness on percussion in the earlier stages, and the occurrence of abnormal clearness at the later periods,—the fine crepitation generally first audible in the inferior parts of one or both dorsal regions, and then rapidly spreading over all parts of the chest; and the tendency of this sign to pass into the subcrepitant and mucous rhonchus, rather than to give place to evidences of condensation, such as bronchial respiration and increased resonance of the voice and cough.

"3rd. By the peculiar characters of the dyspnœa, cough, and expectoration. The respiratory movements are rapid, short, and hurried, rather than laborious and irregular; and there is a sense of constriction in the chest without pain. The cough comes on generally in paroxysms, and is fatiguing from its frequency, rather than painful or very severe. The sputum consists of small whitish-coloured and viscid pellets, free from air, which have a tendency to become aggregated into tenacious, solid, irregularly shaped masses, and want the russet colour, glairy adhesive quality, and small air-bubbles, which characterise the expectoration in pneumonia.

"From general or ordinary bronchitis, the points of discrimination are less decided; it will, however, be generally found, that in capillary bronchitis,—

"1st. The degree of dyspnœa, and the lividity of the face are greater, and the prostration of strength not only appears earlier, but is throughout more marked.

"2d. The different forms of crepitant and sibilant rhonchi, are at all times the prevailing physical signs.

"3rd. The cough is less severe, and the sputum in the earlier stages resembles rather aggregations of viscid whitish pellets, than the glairy or muco-purulent masses of ordinary bronchitis.

"Attention to these particulars will sufficiently distinguish capillary bronchitis as a complication of influenza."—Pp. 34-5.

We have given the above extract because we believe it to contain the best diagnostic account of capillary bronchitis yet published, and because it will afford our readers a fair idea of the author's power of describing disease as he sees it.

The prognosis in those cases in which capillary bronchitis is associated with influenza is very unfavourable, four of the eleven cases noticed by Dr Peacock having died.

We shall pass without any remarks over our author's observations on the cases in which the local disease took the form of bronchitis, supervening on pulmonary phthisis or complicated with disease of the heart or aorta, and proceed to the subject of influenza complicated with pneumonia. Dr Peacock observes, and our personal experience fully confirms his statement, that the pneumonia occurring in persons suffering from influenza, differed in many respects from ordinary pneumonia. It was attended by an unusual degree of depression of strength, and was combined with various indications of disorder of the chylopoietic viscera, and more or less general bronchial inflammation. The prostration of strength was usually very obvious from nearly the commencement of the attack, before the local affection had gained much ground. The cough was one of the most troublesome symptoms. In three of the seven cases noticed by our author, there was no expectoration, while in the others the sputa presented the mixed character of pneumonia and bronchitis. Neither pain in the chest, nor dyspnœa were very decided, nor were the movements of respiration much accelerated. The pulse was somewhat quicker than usual, but was either soft and compressible, or decidedly small and feeble. The skin was usually cool and moist. The tongue was usually red at the tip and edges, with a whitish-brown fur at the centre and root; it was moist in the early stage, and became drier with the progress of the disease. Anorexia, nausea, and vomiting were early symptoms, to which diarrhœa with abdominal tenderness, and some tympanitis, succeeded. It is worthy of remark that, notwithstanding the threatening character of these cases of pneumonia, they were readily amenable to treatment, and terminated favourably.

Our limited space prevents us from noticing Dr Peacock's third variety—epidemic catarrhal fever with abdominal complication.

We proceed to the division of his work devoted to the treatment of the disease.

In cases of simple catarrhal fever, "the patient was usually directed to be confined to the room or the bed; to take some form of antimonial or diaphoretic medicine; to use the foot bath, and to restrain himself to a low diet. At the end of a day or two, according

to the urgency of the symptoms, a more nutritious diet was allowed, and some form of tonic or slightly stimulant medicine, with anodynes and expectorants, was prescribed." We very much doubt whether the patient in reality derived any advantage from this treatment. We believe he would have got well just as soon without "antimonial or diaphoretic medicine" or restriction to low diet. Our own experience leads us to the belief, that after having duly regulated the bowels, the most successful treatment consisted in immediate recourse to stimulants and tonics; and that mulled port-wine and beef-tea were of more avail than antimonials or other depressing agents; and the anorexia usually present, rendered any advice regarding spare diet very superfluous. This difference between our author and ourselves is, however, only one of degree, for he concludes by observing, that "tonics and stimulants were employed more freely, and at an earlier period than would, in simple catarrh, have been either necessary or desirable."

The section devoted to the treatment of the cases of influenza with pulmonary complication, is perhaps the most valuable in the whole book, and affords the strongest evidence of the author's skill at the bedside. We strongly recommend it to the most attentive perusal of our readers.

In his remarks on the treatment of influenza with abdominal complication, he specially notices the low rheumatic fever which was frequently present. Under this division he seems to include all those cases in which the pains in the head, back, loins, and extremities were especially severe. Colchicum was his sheet-anchor in these cases; four or five minims of the tincture of the seed, combined with ammonia and opium being given every three, four, or six hours; stimulants or tonics being usually administered at the same time, or later in the attack. Dr Peacock does not make any allusion to a very simple mode of treating the intense evening headache, which has been strongly advocated by Dr Graves, and has proved most efficacious in our own hands, viz., sponging the temples with very hot water. When rheumatic symptoms continued to harass the patient during convalescence, our author's treatment consisted in the internal use of small doses of colchicum, iodide of potassium, and opium, separately or in combination, and in the employment of stimulating and anodyne liniments externally. With regard to the latter, he observes that—

"The most useful external application which I have tried is a combination, in about equal proportions, of the strong tincture of aconite, prepared according to the formula of Dr Fleming,[1] with the compound camphor and soap liniments. The remedy requires to be rubbed in for a sufficient length

[1] As the tinctures of aconite obtained from the London chemists differ extremely in strength, we may observe that Mr Twinberrow of Edwards Street, Portman Square, and Mr Squire of Oxford Street, prepare the tincture in accordance with Dr Fleming's formula.—REV.

of time to produce a strong biting or tingling sensation in the skin, which is succeeded by a general numbness of the surface, that materially alleviates the pain. After having been a few times employed, the proportion of the aconite may be increased to one-half or two-thirds."—P. 96.

We cannot follow Dr Peacock in his inquiry into the nature and causes of the epidemic; but we trust that we have already said enough to ensure for his very admirable and well-timed work a place in the library of all our readers. We took up his volume with very high expectations; we have not been disappointed.

G. E. D.

Fever in its Relations to Sanitary Reform, being the Address in Medicine, delivered at the Sixteenth Anniversary Meeting of the Provincial Medical and Surgical Association, held at Bath, August 16 *and* 17, 1848. By Wm. Davies, M.D., Edin.; Physician to the Bath United Hospital. Pp. 31.

At the graduation of the University of Edinburgh in 1843, the author of this address obtained the distinguished honour of a gold medal for his dissertation on typhus fever. From the high opinion entertained of his essay by the professors, its publication in a separate form was generally anticipated, and in not doing so, we think Dr Davies has done himself considerable injustice. An address scarcely admits of that careful sifting of evidence which the nature of his enquiry demanded; so that while the present paper contains a clear announcement of the author's views of fever, it is not so complete in their illustration as is desirable. Dr Davies is himself conscious of this, and has promised to publish an appendix to the next volume of the transactions of the Association, which shall contain such additional materials as he may deem requisite to a full exposition of his subject. We postpone our *review* of Dr Davies' opinions on fever, until the appearance of this supplement, and confine ourselves at present to a brief outline of the argument in the address.

Dr Davies maintains:—1. That typhus fever is marked by a specific eruption. Without denying that the measly rash may be occasionally absent, we understand Dr Davies' opinion to be, that he would reject all evidence bearing on the origin and dissemination of the disease, derived from cases which did not present this characteristic symptom. 2. That it is propagated by contagion; and, 3. That there is no distinct evidence of its ever spreading otherwise than by contagion. These two propositions embrace the main object of the address, which is to show that defective sanitary arrangements can never in themselves originate genuine typhus, however strongly they may predispose individuals to receive its poison. Dr Davies admits the sufficiency of these causes, more especially when conjoined with poverty and mental distress, to ori-

ginate fevers, but denies that the disease when so produced, ever presents the characters of the exanthematous typhus.

The following extract explains more fully our author's views on the subject of sanitary reform :—

"It must not be imagined," he remarks, "that I have any intention of calling in question the great benefit and advantage, likely to accrue to the health and morals of the lower order of society, by means of the investigations brought about through the instrumentality of a commission of health ; on the contrary, it is because I entertain so high an opinion of the good which might result under the operation of a well-conceived sanitary reform, that I am anxious to bring under the notice of medical men, what appear to me to be grave errors in matters of fact, and a system of loose logic in reasoning on those so-called facts, and drawing conclusions from them, in as far as relates to the subject now under our consideration, namely, the cause of fever. And further, it is obvious, that if the advocates of sanitary reform in their publications hold out hopes to the public, that under the system of regulations which they propose, one of the most fearful and fatal maladies incident to man in this country will be no more seen in our cities ; and if, after trial shall have been given to the plans proposed, the promised result does not follow, the reaction in the public mind will be altogether on the other side, and the good which has been effected will be over-looked in the disappointment at the non-realization of that which was promised. Therefore it is that I am anxious to bring forward evidence which appears to me conclusive, that in the nature of things, after the best sanitary regulations that can be adopted, we shall still be liable to epidemics of typhus in our large cities and towns, especially those wherein extensive manufactories exist ; and that it is utopian, in the present state of our knowledge, to imagine that anything the wit of man can devise, shall have the effect of banishing typhus from our country, until fluctuations in labour, commerce, and the price of provisions shall cease to exist, and every individual of the community shall be so circumstanced as to have within his power a sufficiency of food and clothing to preserve his body in health and vigour ; and then the predisposing causes of typhus, as of nearly all other diseases, will have no existence, and we shall cease to have epidemic diseases, because we shall have no class of our population among whom disease can become epidemic."—P. 20.

The fourth and last proposition is—That the same individual is not for the most part, subject to more than one attack during life, in which character typhus resembles the other febrile contagious diseases.

"It appears to me," Dr Davies observes, "to be well-nigh demonstrable that the poisons of these diseases, namely, small-pox, measles, scarlet-fever, and typhus, are taken into the blood of the individual to be affected before the disease manifests itself, and that in the blood these poisons find the necessary materials for their own increase ; hence they are contagious. It is probable that the particular material of the blood, by means of which these poisons are formed, becomes destroyed during the process, and not being essential to life is not reproduced, or only rarely, and after a long interval of time ; and hence these diseases occur only once during life, just as no yeast would be formed by introducing that matter into any solution which did not contain gluten."—P. 13.

The address of Dr Davies indicates talents of a high order, well-fitted to grapple successfully with the difficult questions involved in the subject of fever ; and we trust that this is only the first of a series

of contributions to medical science from his pen, which it shall be our duty to notice.

We would suggest to Dr D. that a careful inquiry into the nature and origin of the fever which occasioned the large mortality among the Irish emigrants to America in 1847, should form part of the promised appendix to the present paper. This fever was alleged to have been genuine typhus, and was said to have originated in some of the vessels under circumstances where contagion could not be satisfactorily traced.

Lectures on Medical Missions, delivered at the instance of the Edinburgh Medical Missionary Society. Edinburgh. 12mo. 1849.

It is truly said in one of the lectures in this volume, that it is not always possible for the physician to confine himself within the strict bounds of his art. The duties and responsibilities of the sick-room, its humanizing influences both on the patient and the medical man, the situations of confidence into which both are inevitably thrown, the words of consolation or of terror which must often be interchanged, the very thoughts unspoken which arise out of the physician's beneficent ministrations, demand the exercise of powers and faculties which are far removed from his merely scientific functions. It is incumbent on the practitioner to be prepared, at all times, for the fulfilment of these extra-professional duties; to act, not as a *physician*, but as *a man*. He may be called on to stay the career of the morally reckless, by arguments peculiar to himself, or to interpose the words of spiritual consolation or advice, when no one else has the power to do so; and he who is wanting in ministering to the mind, entering into its nobler sympathies, and controlling its baser or morbid affections, will often fail in his mission as respects the body. The career of the medical man, indeed, often places him in situations which have no parallel, as is justly remarked by Dr Wilson, save in the Roman Catholic confessional. In those situations he must be prepared to acquit himself with a tender and delicate appreciation of all the wants of human nature, tempered by that discretion, judgment, and moral rectitude, which should be inseparable from his high vocation. Thus far, the medical man must necessarily undertake the cure of the soul along with that of the body.

In the management of this great trust, however, it will not be surprising if the greatest differences be found among individuals, who are equally impressed with its importance, and equally anxious to perform its duties. Thus many men, of great eminence and earnestness of character, have considered it a solemn duty to use the influence acquired in a medical capacity for the purpose of impressing religious views, and moulding the religious character of their patients; while others, not inferior in piety or disinterestedness, have

uniformly thought it right to decline a responsibility so capable of being turned to good or evil, as inconsistent with their sense of professional duty. Between these two extremes there exist all varieties of opinion and action.

The greater number of the lectures in this volume are evidently dictated by the former of these two views; accordingly, we cannot anticipate more than a partial concurrence of the practical members of the medical profession with the sentiments therein expressed, or the duties therein urged. In recommending this volume to the notice of the thoughtful portion of the medical public, it is no way our intention to open our pages to the discussion of the question of medical missions; but to indicate to our readers that this question has of late excited considerable attention in Edinburgh, and that in this volume will be found the carefully considered opinions of some of the ablest and most respected of our professional brethren.

The volume contains six lectures, and a prefatory essay by Dr Alison (illustrating the general relations of religious truth to science and civilization), the calm reasoning and catholic spirit of which are calculated to command general admiration and attention. The lectures, which constitute the bulk of the work, are devoted to different topics connected with missionary medicine, and are by Professor Miller, the Rev. Wm. Swan, William Brown, F.R.S.E., the Rev. Jonathan Watson, Dr George Wilson, and Dr Coldstream. They are characterised by the evidences of profound conviction and ardent enthusiasm in the cause to which they are devoted, and are generally tolerant and calm in enunciating its claims, and answering objections. As an honest inquiry, therefore, into the responsibilities attaching to our profession, we recommend this work to the attention of medical practitioners.

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1. COSMOS: *A Sketch of a Physical Description of the Universe.* By ALEXANDER VON HUMBOLDT. Translated from the German by E. C. OTTE. (Bohn's Scientific Library). 2 vols. post 8vo. London: 1848-9.
2. *The Poetry of Science, or Studies of the Physical Phenomena of Nature.* By ROBERT HUNT, author of Researches on Light, &c. London: 1848.

IT is very seldom that we bring under the notice of our readers any works except those of a strictly professional character. There are, however, cases in which we deem it expedient to deviate from our ordinary course, and on the present occasion we esteem it no less a pleasure than a duty most strongly to recommend the "Cosmos" and "The Poetry of Science" to the readers of the *Monthly Journal.*

Of the "Cosmos," which has been well characterised by the Chevalier Bunsen—himself one of Europe's brightest ornaments, and

for the profundity and extent of his acquirements standing second, perhaps, only to Alexander Von Humboldt—as *the great work of the age*, we shall say nothing further than that the translation executed by Miss Otté, presents a perfect model for all future translators of scientific works, and that the English reader loses nothing of the spirit of the original German. It is superior to the translations which have preceded it (those of Mrs Sabine and Mr Prichard) in accuracy and elegance of composition; in the circumstance that all the foreign measures are converted into vernacular and comprehensible terms; and in consequence of the addition of a series of notes, having reference to investigations pursued since the original publication of the work, and for which the translator expresses her obligations in the preface to several of her scientific friends. If Mr Bohn's Scientific Library is to be judged by the present sample—if he continue to be equally happy in the selection of his works, and in the choice of his editors and translators—we do not hesitate to assert that it will be a disgrace to this country if he is not duly supported. On the present occasion he has no right to complain, for we understand that nearly ten thousand copies have been already disposed of.

"The Poetry of Science" presents much that is worthy of commendation. Mr Hunt's object in the publication of this volume, is "to link together those scientific facts which bear directly and visibly upon Natural Phenomena, and to shew that they have a value superior to their mere economic applications, in their power of exalting the mind to the contemplation of the universe." His work is divided into sixteen chapters, in which are considered the general conditions of matter, motion, gravitation, molecular forces, crystallogenic forces, heat, light, actinism, electricity, magnetism, chemical forces and phenomena, time in reference to geological phenomena, and the phenomena of vegetable and animal life. In a volume embracing so many and diverse subjects, it is not to be expected that every department should be executed with equal skill. Were we inclined to be hypercritical, we might perhaps complain of the antiquity of some of Mr Hunt's physiological views. But he does not profess to be a physiologist, and on most of the other subjects on which he enters, he may be followed as a safe guide.

Did we not feel that he might shield himself under the title he has selected, we would venture to hint that his style is occasionally more elevated than the claims of science demand. We extract his "idealization of truth" to let our readers judge for themselves:—

"The poet's dream pleases the imaginative mind, and, associating in our ideas all that is graceful and loveable in the female form, with that diviner feeling which causes us to regard her as the nurse of nature, we perceive in the goddess, the enchantress, or the sylph, pure idealizations of the physical powers. The spirit floating over these forms of beauty, and adorning them with all the richness of colour, painting the rose and giving perfume to the violet, is, in the poet's view, one which ascends to nearly the highest point of etherialization, and which becomes indeed to him a spirit of light; they ride upon the zephyrs, and they float in all the luxury of an empyreal enjoy-

ment down to the earth upon a sunbeam. Such is the work of the imagination."—P. 361.

If it be, and we do not doubt the accuracy of the statement, we can only say that the less work the imagination has to do, the better; and that we trust Mr Hunt will allow the "forms of beauty" to remain for the future in undisturbed possession of the zephyrs on which they are mounted.

Having noticed what we deem the faults of the present volume, we ought to say a word or two on its good points. These are numerous, greatly out-weighing the slight blemishes to which we have alluded. Those who are conversant with the recent progress of physical science, are well aware of the value of Mr Hunt's contributions to several of the subjects discussed in the *Poetry of Science*. We can especially recommend the chapters on crystallogenic forces, light, and actinism, as containing much interesting matter not easily found elsewhere. The *idea* of the work is good, and the *execution* very far above mediocrity.

Report of the Secretaries and Treasurer of the Birmingham and Midland Counties' Pathological Society. 1849.

THE Birmingham Pathological Society has now been in existence upwards of eight years; its original establishment, and its subsequent prosperity, being, we believe, in a great measure dependent on the exertions of a distinguished provincial physician, Dr Bell Fletcher. We regard local societies of this nature as of the greatest benefit, not only to their immediate members, but to the profession at large, insomuch as the annual reports serve in reality as catalogues of specimens, of which the working pathologist could otherwise gain no information.

The most interesting case is one recorded by Dr Percy. In a girl aged eighteen years, there were tubercles in the cerebellum, peritoneum, mucous membrane of the uterus, and fallopian tubes, but *the lungs were quite free from tubercles*—thus forming an exceptional case to Louis' celebrated law. (See Researches on Phthisis, p. 153; Syd. Soc. edition.) Dr Fletcher likewise relates the case of a man aged thirty years, in whom there were tubercles in the peritoneum, pleura, and pericardium, but in which none could be detected in the lungs.

We observe that in one or two cases, the age of the patient is omitted. This is a serious fault, being in some cases, as for instance in perforation of the stomach, (see Dr Russell's cases, p. 15), a very important element.

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVIII.

MEETING VII.—*April*, 4, 1849.—JAMES SYME, Esq., President, in the Chair.

REMARKABLE CASE OF SHIPWRECK.

Dr Robert Paterson read an account of the shipwreck of the schooner Jacky of Leith, and of the sufferings and death of her crew, which was composed of the captain, six men, and a boy. The vessel left Galatz on the 24th July 1848. Before sailing, the whole crew had suffered from ague; and when the vessel reached Constantinople, on the 20th August, two men were left behind, and two fresh hands substituted. During the homeward voyage, the weather was bad, and the winds contrary, and on the 27th October, while the schooner was approaching the British Channel, a sea fell on board, and almost sunk her. She now became leaky, and the corn with which she was laden choked the pumps; half the men were sick, and all but the captain manifested singular apathy in contemplating the fate which awaited them. Some efforts were made to keep the leak under, by baling out the water in buckets. On the 28th, two of the crew were rescued by a Dutch vessel, and the mate was crushed to death between the vessels in a boat. Shortly afterwards the schooner sank, and two of her crew were drowned; the survivors, consisting of the captain, the cook, and one seaman, reached the long boat, which floated free. They had no provisions nor water on board the boat, and all three, especially the seaman, were hungry, weak with previous exertion, and labouring under ague. The weather was wet and cold, the sea was running high, and the boat required constant baling. The seaman died exhausted on the 29th. The survivors suffered at first severely from hunger, but the uneasy sensations from this cause soon passed away; their thirst was partly appeased by licking the rain drops off the thwarts of the boat; the cook, contrary to the advice of the captain, also drank sea-water. On the evening of the 3d November (the sixth day of exposure in the boat), the cook died; his death was preceded by delirium. The captain had not strength to heave the body overboard; his legs became benumbed, he began to find his mind wandering—to see sights and hear sounds which he knew had no real existence; he continued to suffer from thirst, and from a pain in the right side of the chest. He had, however, sufficient resolution to abstain from tasting the sea-water, and kept the boat afloat by baling with a tin canister. On the morning of the 5th November, after fully *eight days* exposure in an open boat, without a morsel of food, and only a few rain-drops for drink, cold, wet, and exhausted, almost speechless, unable to walk or even to stand, with hollow eyes and ghastly countenance, he was taken on board the Anne of Poole. He was now treated in the most kind and judicious manner, and when put ashore on the 17th November, at Kingston, in Ireland, was so far recruited as to be able to write to his friends. He returned to Leith on the 22d November, and from this period his case had been watched by Dr Paterson. Sloughs, which had formed on the feet, separated, and the sores healed readily; he gained flesh fast, and by the middle of December was able to go about. On the 22d December, after exposure for some hours to a frosty air in the country, he was seized with a severe rigor, which he believed to be a return of ague. Pain in the right side soon succeeded, together with cough, rusty sputa, and feverishness. There were dulness on percussion in lower part of right side of chest, and dry crepitation was audible over surface of right lung. These symptoms

yielded to appropriate treatment; but on the night of the 29th December he suddenly became sensible of extreme dyspnœa, and of acute pain, stretching from the epigastrium round the right side to the spine; the dulness of right side continued. On the 31st there was marked resonance observed on percussing the right side; the pulse was small and feeble; respiration laborious. He died on the 9th January 1849.

On dissection, the right lung was found reduced to the size of the fist, flattened, and tied down to the spine by recent lymph. There were two pints of flaky serum, and a quantity of air in the right sac of the pleura. At the inferior portion of the lower lobe of the lung, there was a cavity of the size of a walnut, lined with delicate blue membrane, communicating with a large bronchial tube above, directly and freely, and opening below by a small narrow slit into the cavity of the right pleura. The cavern contained a little white tenacious mucus; the tissue surrounding it was somewhat condensed; there was no appearance either of gangrene or of tubercle in any part of the lung. The precise morbid process which led to the formation of the cyst could not be determined. It was probably formed during the patient's exposure in the open boat in October; and perforation of the pleura must have taken place on the 29th December during the apparent convalescence from pneumonia. In the latter part of his communication, Dr Paterson instanced a number of cases in which hunger and thirst had been endured for long periods. The sensation of thirst had been mitigated in the instance of Captain Bligh and his companions, by wearing their clothes after saturating them with sea-water. In the case of the poor captain of the Jacky, courage and hope, which never forsook him, probably contributed to support him under protracted sufferings.

The *President* remarked that authentic cases, such as Dr Paterson had brought forward, contradicted the commonly received doctrine, that three days was the extreme time for which absolute fasting could be endured. There seemed considerable difference in the powers of endurance possessed by persons in health, and by those labouring under certain diseases. Fever patients had been known to live for fourteen days upon toast and water.

Dr Douglas Maclagan instanced the case of a miner, who, in consequence of a fall of earth, was confined in a coal pit for *three and twenty days*, and during the whole of this period subsisted upon a mere morsel of buttered cake. He had access to the water of the mine, and occasionally chewed some tobacco during his captivity.

ANALYSES OF THE BLOOD IN CHOLERA.

Dr William Robertson read an account of his observations on the blood drawn from patients labouring under cholera. He grouped the cases under four heads, described the appearance and general properties of the blood in each, and exhibited the general chemical results obtained from thirty analyses, conducted according to Andral and Gavarret's process. Five of the analyses were made by Dr Douglas Maclagan, one by Dr Christison, and the rest by Dr Robertson.

Average Constitution of 1000 *parts of Cholera Blood.*

	Fibrine.	Org. Serous Solids.	Inor. Do.	Globules.	Total Solids.	Water.	Sp. gr. of Serum.	Sp. gr. of Blood.
Early stage,	2·7	82·2	7·8	103·4	196·1	803·9	1030·0	1053.7
Early collapse,	3.2	94·0	7·0	130·8	235·0	765·0	1033·7	1059·5
Collapse, .	3·2	101·2	6·9	130·0	241·3	758·7	1036·6	1066·4
Reaction stage,	3·5	76·6	6·6	126·7	213·4	786·6	1030·6	1057·9

Distinct indications of the presence of urea had been obtained both by Dr D. Maclagan and by Dr Robertson, upon testing blood taken from the heart and veins after death, and on one occasion Dr Maclagan had obtained similar in-

dications, upon examining blood drawn from the arm during the early stage of the disease. *Urea* seemed most abundant in the blood of patients who died during the stage of reaction ; 1000 grains of blood yielding in one case 1·6, in another ·73 of nitrate of urea. The chief conclusions which these analyses, coupled with certain phenomena of the disease, suggested, were, 1st—That anæmia or an impoverished condition of the blood, acted as a predisposing cause of cholera. 2dly. That although during cholera the relative proportion of the blood constituents was rapidly and singularly altered, there was yet no evidence that the primary operation of the morbific poison was exerted upon the circulating fluid,—the chemical changes recorded were only the *effects* of the morbid process. 3dly. That the changes which the blood underwent up to the period of reaction, consisted in a concentration of the serum from loss of water, and a loss of salts almost proportionate to the amount of water abstracted. 4thly. That the *proportion* of salts was lower during reaction or convalescence than during the progress of the disease. For every 1000 parts of water, there existed during the *early stage* 9·7, during early collapse 9·2, during collapse 9·1, and during *reaction* only 8·4 parts of mineral constituents of the serum. That consequently the phenomena of the stage of collapse could not be due to the loss of salts, and that the saline treatment did not rest upon a good theoretical foundation. 5thly. That the changes observed during reaction were probably due, in part, to absorption of fluid into the circulation, and consequent dilution of the blood. This absorption sufficiently accounted for the small per centage of inorganic serous solids during reaction, and might assist in explaining the marked diminution of the organic serous solids. 6thly. That during reaction the fibrine was often proportionately increased, and that there was reason to believe that blood corpuscules were at the same time rapidly formed. 7thly. That the albumen which was withdrawn from the circulation, was metamorphosed, converted into tissues, excreted by the bowels, and in most cases likewise excreted by the kidneys. In nearly eighty per cent. of the urines examined during convalescence, albumen was detected ; its absence might accordingly be regarded as exceptional. 8thly. That the formation and retention of urea in the blood was always to be dreaded during reaction, and hence that the use of diuretic remedies, and especially of such as were believed to possess the property of expelling urea from the system, was clearly indicated. Tabular statements of the analyses were exhibited.

Dr Christison inquired if an experiment which he had made during the former epidemic, and which proved that the dark blood of cholera was yet quite susceptible of arterialisation, had been confirmed on this visit of the disease. He had found, that the blood when defibrinated by agitation in a phial with pieces of zinc, rapidly assumed the arterial hue when subsequently agitated with atmospheric air or with oxygen gas. Dr Douglas Maclagan and Dr William Robertson had both recently confirmed the accuracy of Dr Christison's observations.

A conversation followed, chiefly regarding the effect of remedies during the recent epidemic. It was stated, that the saline injection of the veins had been practised in the cholera hospital in twenty-five or twenty-six cases, and that although remarkable temporary benefit seemed sometimes to result, no patient had survived the operation more than eight days. In 1832 about 1-6th of those whose veins were injected recovered ; profound collapse was, however, not always waited for, before recourse was had to the operation. The *diuretics* used on the present occasion were bitart.-potass, nitre, digitalis, colchicum, and tinct. lyttæ. Blood-letting practised early was the remedy which Dr Robertson had found most efficacious in averting the most formidable symptoms of the disease.

CASES OF POISONING.

Dr Maclagan stated, that a case of poisoning by sulphuric acid had recently occurred in the neighbourhood. The acid had been carelessly laid aside, and

had been mistaken for ale. Mr Syme had reason to think, that a number of patients who escaped the immediate effects of this poison, subsequently became affected with stricture of the œsophagus, and Dr Christison knew of several such cases on record.

Dr Christison had been recently consulted in the case of a lady and three children, who, about an hour after dinner, all became suddenly and seriously ill, the chief symptoms being vomiting, with sickness and great depression. The soup, of which all had partaken at dinner, was made of hare. The cook declared that the hare, although not in season, was in good condition; but that it was an *old hare*, and had *very yellow bones*. She next day ate the remainder of the soup herself, and suffered like the other members of the family. All recovered. Dr Christison was anxious to hear if other members of the society had met with similar cases.

Dr Douglas Maclagan had been four times consulted on account of somewhat similar symptoms supervening upon the use of rabbit-pie. On two occasions he had unsuccessfully applied the tests for mineral poisons, for jalap-resin, &c., to the remains of the pie. There could be no doubt that the vomiting and purging were connected with the rabbit, and not with the paste, nor with any other article consumed at the same time. In some instances all who ate of rabbit suffered, while those who partook of other articles at table escaped.

It was then moved by *Dr Christison*, seconded by *Dr Douglas Maclagan*, and unanimously carried, "That the secretaries be instructed to communicate the authenticated minutes of the society to the editors of the Edinburgh Monthly Journal, and that the report of its transactions thus furnished should be the only one authorised by the society."

EDINBURGH OBSTETRIC SOCIETY.

SESSION VIII.

MEETING II.—*January* 10, 1849.—Dr SIMPSON, President, in the Chair.
(Continued from P. 709.*)*

CASES OF SUDDEN DEATH AFTER DELIVERY. BY DR M'COWAN.

Dr M'Cowan stated, that on the 16th June 1845, he was requested to visit Anne Barker, æt. twenty-one, stated to be in labour of her first child. On his arrival, he found her suffering from spurious pains, and complaining much of difficulty of breathing, and pain in left sub-mammary region. The face was very œdematous, as also the lower extremities. Under the usual treatment the pains subsided. During the two following days she complained occasionally.

On the 19th she had much pain in her side,—respiration laborious,—pulse, which had hitherto been about 70 to 80, rose to 120, but feeble. Venesection to ten ounces produced faintness, without alleviating the pain. She was then cupped to four ounces, with immediate relief.

About three A.M. of the 20th, labour commenced, and proceeded naturally and speedily till nine A.M., when she was delivered of a still-born male child, and instantly expired.

Post-mortem.—The body presented a generally œdematous appearance.

On opening the thorax, the pericardium was found distended with a dark fluid. The heart was much enlarged, extending about two inches to the right of the sternum. Right ventricle very thin and dilated. The aortic opening could with difficulty admit the point of the little finger; its valves were hard and cartilaginous. The whole heart was filled with coagulated blood. The surfaces of the pleuræ were strongly adherent—the greater part of the left lung was hepatized. The uterus and other organs seemed healthy.

Dr Simpson stated, that ten or twelve years ago, when acting as resident house-surgeon in the Lying-in Hospital, he was called by a midwife to see a patient who had suddenly fainted and died immediately after the expulsion of the infant. The uterus, as felt through the abdominal parietes, appeared firm and contracted. The labour had been natural, without any hemorrhage or other complication. An autopsy was not obtained; but the anterior history of the patient showed the probability of the existence of diseased heart. A short time afterwards a patient, attended by one of the pupils of the hospital, rose up and stood for the first time about a week after delivery. She immediately fainted and expired. There had been nothing particular in the history of the labour or convalescence. Drs Denman, Clarke, Blundell, Meigs, &c., had recorded notices of cases of this kind of sudden death during labour, or after it, without convulsions or hemorrhage. Dr S. had always taught that the existence of heart or chest disease formed a kind of complication, which was not only extremely anxious in itself, but that generally incapacitated the patient from bearing with impunity, for any great length of time, the struggles and efforts of a prolonged second stage. He considered such a complication an indication for earlier instrumental assistance than would be deemed necessary under other circumstances.

CHLOROFORM IN PUERPERAL CONVULSIONS. BY MR NORRIS, SOMERSETSHIRE. COMMUNICATED.

E. R., æt. forty—married—seventh pregnancy—miscarried at the fourth month twelve months since. Had been suffering much from œdema of the lower extremities, and from dyspnœa for a fortnight previous to her confinement on the 11th December. Informed me afterwards that she had noticed a trivial swelling of the left arm and hand, also of her face, in the morning. Urine rather scanty. She was being attended by a midwife.

Shortly after nine A.M. I was sent for in a hurry to see her. I found her insensible, with dilated pupils, pulse slow, *not full*, and rather irregular,—foaming at the mouth, but *without* convulsions. Child presenting naturally, third position; head in the hollow of the sacrum; labour progressing rapidly.

My father arrived about ten minutes after myself, and, whilst I was fetching chloroform, labour was completed, and the patient was comparatively comfortable, but much exhausted. Third stage completed by half-past nine. Just within two hours after this she had another epileptic fit, *without* convulsions, for which I bled her, and took the ordinary measures, with present success. At three o'clock she had a third fit, very severe, during which she was much convulsed. Chloroform was then administered by my father. The fit at once yielded, and she from that time proceeded to convalesce. With the exception of one day's "weed," she has made a favourable recovery.

I may add, that her kidneys yielded at once to the use of bitartrate of potash, though I was unable to obtain any urine to test the presence of albumen. Chloroform was not used in the first and second fits, in consequence of not being at hand till they had ceased. I the less regret it, as its success in the last attack showed its superiority over the ordinary remedies.

MEETING III.—*February* 14.—Dr SIMPSON, President, in the Chair.

ON SOME ALLEGED CONSEQUENCES FROM THE INHALATION OF CHLOROFORM. BY DR. CUMMING.

Dr Cumming was induced to bring the following few remarks before the attention of the Society, from having received from a friend in England a circular letter, addressed by Mr Gream to the practitioners there, the purport of which was to collect from them chiefly the *bad* consequences of the inhalation of chloroform, and especially the exhibition of indecency, both in action and expression, as one of these. Dr C. was anxious, in regard to this last alleged

consequence, to ascertain the experience of the members of the Society, because they had the most ample opportunities of establishing or disproving it as a fact; and, if it indeed were a truth, of ascertaining the cause or causes which might have led to it; and surely it was a matter of no small importance to elicit truth, in reference to so grave an imputation on an agent confessedly of such power, not only in alleviating pain but also in preserving life; for if such a consequence as that referred to were a general, or even a frequent, or indeed more than an *extremely rare*, occurrence, it could not be denied that most of the honourable and delicately-minded members of the profession would hesitate as to its use, even with the advantages they knew to attend its employment.

Was it then a fact, that indecency of thought, expression, or action, was one of the results of its inhalation? In his own experience he certainly could not adduce one single instance of it. He had now employed it in upwards of seventy cases of midwifery and operations for uterine disease; and in none of these had he ever observed the slightest indication of even the most remote approach to such a state. The persons who had been subjected to it, as might be imagined, had varied still more in mental constitution, than even in their class of society and social circumstances; and if such a consequence were at all general, or even frequent, he could not but conclude that *some* instances of it must have occurred to his observation; but this most emphatically had not been the case. He had observed a few individuals give expression to feelings of a purely devotional cast, indicating the habitual tone of their mind; and he had, in peculiar circumstances, heard them utter unconnected words, and he had seen them, as all must, make gestures and contortions of the body; but not having been led by any previous thoughts to imagine that indecency, in any shape, was a consequence of chloroform, he never for a moment conceived that such motions *could* be so construed by any mind.

From the circumstance that much had been written in Scotland about chloroform, that reports had been published of thousands of cases, and that its effects had been detailed with a degree of minuteness to which no other medicinal agent, that he was aware of, had been subjected, and yet that no mention of the melancholy result complained of by some of our English brethren had been made, he should imagine that he was pretty safe in concluding that it had not fallen under their observation, or, if it had, that it had been only so rarely that they had properly not regarded it as a consequence of the use of the agent.

If, then, it had by certain practitioners been observed so frequently as to found on their observations a charge against its employment in every case;—to what was this very striking difference to be ascribed? To Dr C. it appeared irresistible that it was owing to one of two causes, or both combined: either to its having been improperly administered, or to such a state of mind and train of thought having been induced in the mind of the patient, her conversation or otherwise, previous to its administration, as, when continued under its imperfect influence, had given origin to the expressions or actions complained of. If it were a fact, that indecency had followed its use under the care of some practitioners and not under others, and this last a very numerous body, certainly not less qualified to make a proper use of their senses than the others, the result *must* be attributed to the operators; for surely we could not imagine that the females under their care were differently constituted from those under the other class. If the operators were the culpable parties—to what in them was the fault due in the case of female patients? First, to the *imperfect* administration of the chloroform. We all know that, when so administered, a kind of mere intoxication was produced, and, of course, the symptoms and expressions of intoxication would be the result. If a train of thought which leads to impropriety of expression had been previously induced (and this was the second cause), what *could* we expect but that such impropriety would out? That such results as alleged from chloroform were inevit-

able, we know not to be the case; that they were frequent, he would not believe till he ascertained the frequency of the effect from the statements of the administrators themselves, and knew the character of the administrators; and even then a stretch of charity would induce him to assign the former in preference to the latter cause as the true one; and, giving them the benefit of this charitable allowance, he would humbly recommend them to apply themselves, without delay, to the study and practice of the due mode of application, with the comfortable assurance that their task would be an easy one, and the fruits of it pleasant, and all the more so that the previous results must have been so thoroughly offensive.

Dr Simpson observed that the special alleged objection to chloroform to which Dr Cumming's remarks referred, was by no means a new objection. At the time at which various of our now common articles of food and medicine were first introduced into use, these innovations were frequently opposed and decried on exactly the same ground. As a general rule, this was an argument always resorted to by weak and impure minds, when they could find no stronger arguments against any proposed innovation in our customs or habits. For instance; at the present day, no one imagines or argues that the eating of potatoes is liable to cause in those who eat them any indecency of word or action. And yet that was one of the principal arguments maintained by the unscrupulous against the use of potatoes for long after their introduction. In his Traditions, Mr Chambers states that, even as late as the last century, there was a prejudice against the potato for this, among other reasons,—"That it was a provocative to incontinence." A hundred years after this our successors in the profession will probably feel as much surprised at the idea of the use of chloroform exciting improper words and actions, as we are all now at the old idea of the use of potatoes being attended with the same alleged consequences. Dr Simpson had now used upwards of 2000 oz. of chloroform—had never witnessed any such effects, and never expected to witness them. In the circular letter of Mr Gream to the English practitioners to which Dr Cumming alluded, Mr Gream did not ask for any beneficial effects from chloroform that his correspondents might have seen; he asked only for the bad effects; he wanted to see only one side of the picture. And, doubtlessly, he would be gratified by some of the reports given him; for Dr Simpson believed, from what he occasionally heard from some parts of England, that in many districts and towns there, the use of chloroform was still greatly misunderstood; a bad and dangerous article often used under the name of chloroform; the article imperfectly and improperly exhibited; and no proper care and precaution used in its exhibition. But still the profession would take any report from Mr Gream on the subject, *cum grano salis per magno*. In a former amusing pamphlet on the subject, Mr Gream proceeded so far as to alter the Official Report in a case of death from chloroform at Aberdeen, in order to prove to his readers that those who used chloroform may have morbid adhesions of the heart to the pericardium. In the report given in to the Law or Government authorities in this case, it was stated that there existed "old adhesions" between the heart and pericardium which required to be forcibly torn asunder with the finger. Mr Gream pretended to quote this report verbatim, and in inverted commas; but he carefully omitted the word "old," &c., for he wished to prove to the ladies who read his pamphlet that these pericardial adhesions were quite recent, and the *effects* of the chloroform! Our grandchildren will, no doubt, wonder at such pathological ratiocination, even with the omission of the word "old;" but at the present day it is calculated to teach us this circumstance, that one who could tamper with an official and public document, for the purpose mentioned, will have little hesitation in following the same course with any private documents intrusted to him.

In Edinburgh chloroform had now been exhibited, he believed, to 40,000 or 50,000 persons, without a single accident or deleterious result, traceable to its use. Perhaps as many doses of opium, or antimony, or calomel, or epsom salts even

or any other potent medicine, would not have been followed by results equally innocuous. Some two or three hundred die in England and Wales every year of the effects of opium and other medicines given in improper doses, or in particular idiosyncrasies; and when the properuse of chloroform came to be better understood, it would, he believed, stand less high in that list than most other medicines used; while, besides saving pain, it would ultimately be found to save human life to no small extent in surgery and midwifery. One means by which its proper mode of employment would become more widely spread in a few years, was in consequence of our medical students, who daily saw its use, and the rules of using it, practised in the hospitals of Edinburgh and London, leaving their studies and commencing practice with all the knowledge necessary for its employment, and without any of the fears shown naturally by those who were acquainted with its phenomena. It has been often repeated that in Harvey's time no physician who was above forty years of age, would believe in the doctrine of the circulation; and new medical theories and practices were very seldom adopted by men beyond that term. Some beyond this age of conversion have argued and reasoned on the subject in the most extraordinary way. For instance, an old but excellent accoucheur, Dr James Reid, in a late discussion on the subject of anæsthesia, at the Westminster Society, stated that he had used chloroform in three midwifery cases, and that it had *not* relieved the pains of labour. Of course Dr Reid's argument merely and simply showed one thing, that he was ignorant of the mode of giving it, for no one here or elsewhere, who knew anything of the matter, had met with such results. And the most strange results were often ascribed to it. In fact, if *anything* whatever happened to the mother or child for months, it mattered not what, it was by some of our English friends directly ascribed to chloroform. It produced all manner of medical and some surgical diseases If the child had a dislocation or fracture, the cause was the chloroform. The same gentleman, Dr Reid, sometime ago mentioned at the same Society the case of a child dead born, in the practice of Dr Smith, after a very long labour (forty or fifty hours), and where the mother was chloroformed. The child died in consequence of dislocation or fracture of the lower parts of the parietal bone against some projecting point or other in the interior of the maternal pelvis. But the death, Dr Reid argued, was from chloroform; and as the immediate cause of death was the above, hence chloroform could produce dislocation or fracture, and a hundred deleterious results besides. He had heard it accused, on equally logical grounds, of being the cause of a monstrosity on the part of the fœtus, when it happened to be given at the birth of the mal-formed child. Certainly not many years will elapse ere the profession must see the weakness and folly of this mode of argument. But, as it stands, no newly introduced practice had ever, he believed, made so great and steady progress in so short a time as anæsthetic midwifery; and of its ultimate and entire success and adoption everywhere no one here, where all witnessed and practised it, had any kind of doubt.

Dr Duncan stated, in regard to the woman who exhibited signs of sexual excitement at the Clinique, at Paris, when under the influence of sulphuric ether during labour, and whose case had been so often made a foundation for the argument against the use of anæsthesia in midwifery,—that he had been more than once told, in Paris, at the time of her delivery, and on good authority, that she was a female of very doubtful character, and one, consequently, in whom improper expressions, such as were ascribed to her, could excite no surprise. Further, Dr D. was inclined with Dr Cumming to ascribe the excitement altogether to the imperfect and inadequate manner in which the etherisation was conducted, or to the patient having had improper conversation addressed to her.

ON A NEW FORM OF CRANIOTOMY FORCEPS. BY DR ZIEGLER.

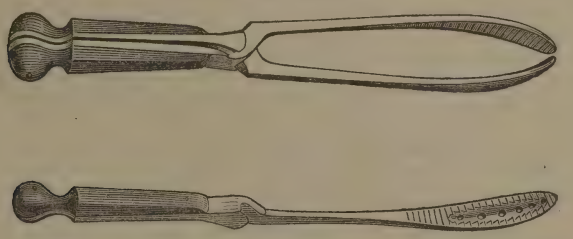
Dr Ziegler stated that he was very averse to adding another instrument of this class to the present obstetric armamentarium. But he had experienced in

practice the want of such an extracting power as the one he was now to exhibit; and it was mainly for the purpose of supplying that want for his own use, that he had caused this instrument to be made. It was intended, as its name implied, for the management of that unfortunate class of cases in which, from confinement or deformity of the pelvis, the birth of a living child could not be effected; in short, it was intended for the extraction of a dead child after the head was perforated and the brain discharged.

It consisted of three parts, which were capable of being put together, by means of a Smellie joint, so as to form two distinct instruments; of which the one was worked on the principle of the common forceps, and the other on that of the craniotomy forceps.

After the cranium had been opened, and the brain evacuated, in a great proportion of cases, if the deformity were but slight, and provided the uterine efforts continued unimpaired, delivery would be accomplished without any further instrumental assistance. But if we had to do with a pelvis whose diameters were still more defective, or if the natural powers were much exhausted, or if dangerous symptoms supervened, then extractive efforts became necessary. Now, under such circumstances, it must have occurred to most practitioners, that if we had a pair of forceps, the blades of which were of sufficient length to reach above the brim, so narrow as that without affecting the security of their hold, they might be safely worked within a defective pelvis, of such strength as not to yield, and whose curvature corresponded to the altered form and bulk of the mutilated cranium, then we should be furnished with the best form of an extractor; one which would be easily applied, not liable to slip, capable of exerting the greatest amount of power, and of accommodating the head to the direction of the passages.

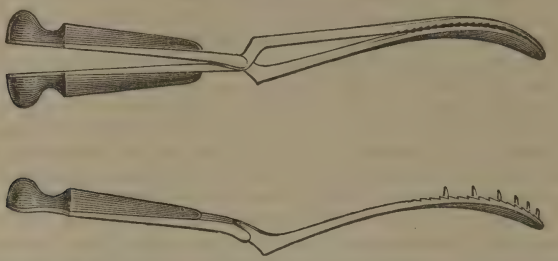
Fig. I., Extracting Forceps.



These objects he had endeavoured to keep in view in the construction of the instrument—under its first form, that of an *extracting forceps* (see Fig. 1.). It measures $13\frac{3}{4}$ inches in length, of which 8 were occupied by the shank and blades, and $5\frac{3}{4}$ by the lock and handles. The blades are of considerable strength, smooth externally, and deeply toothed internally. Their greatest breadth is 1 inch; and the greatest distance between the blades $1\frac{5}{8}$ of an inch, while the points approach each other within $\frac{1}{4}$ of an inch. In determining its curvature, he was guided by repeated experiments on the full-sized fœtal cranium after it had been opened and its contents removed. When treated in this way, the fœtal head assumed somewhat the form of two triangles united together at the base, the chin representing the apex of the one, the collapsed bone around the occiput that of the other. What was wanted then in respect to the curvature, was that the blades should approach near to each other posteriorly, so as to compress the upper part of the head which had been lacerated by the perforator, and was unsupported by the brain, and that anteriorly they should turn sharply round the base, which afforded the only sufficient hold or purchase. On looking cursorily at the instrument under this form, gentlemen might suppose that it would be liable to slip, and Dr Z. therefore regretted that he had not been able to procure a fœtus to show the firmness with which it

maintained its hold. He could assure them, that in many experiments upon dead children which he had made in the old hospital, he had been able to exercise an amount of traction much greater than could ever be justifiable in the operation upon the living female.

Fig. II., Craniotomy Forceps.



Cases, however, did occur, in which it was impossible to employ the instrument under this first form, or in which it was unnecessary to do so; and then we had it in our power, by substituting the crochet blade for one of the claws of the extractor, to use the instrument under its second form, that of the common *craniotomy forceps* (see Fig. 2). It could not be denied, that even with this modification of the instrument, the operator was not capable of exerting the same amount of power as with the former. For, like all other contrivances of a similar kind, its hold was liable to be lost by the tearing away of the portion of bone within its grasp. There was also this further disadvantage, that the head was drawn down more or less laterally, so as to alter in some measure its most favourable direction. Still, when the resistance was not very great, the bones firm, and above all, when the cranium was properly seized, it was perfectly adequate to complete the delivery. This last particular, the mode of seizing the head, was a point of the first importance, affecting much the efficiency of the instrument, and the facility of the operation. Many operators seemed to think that it was of no consequence to what region of the head the instrument was applied, provided only they secured a hold sufficiently firm, but this was a great mistake, and one too into which no less respected authorities than Drs Ramsbotham and Churchill had fallen, as would appear by inspecting the plates which these authors had given, as illustrative of the operation of craniotomy. There the instrument was represented as fixed upon the frontal bone, and it was abundantly obvious, that traction exercised upon that point would have the effect of separating the chin from the breast. Nor was this the only inaccuracy in that plate, for it would be observed that the operator was represented as extracting in the direction of the axis of the outlet, while the head was still above the superior aperture of the pelvis. But, returning from this digression, and comparing the instrument with those of the late Drs Davis and Campbell, it appeared much less clumsy and formidable; and the teeth being fewer in number, smaller in size, and less closely set, they were less likely to break up the bone, and thus leave a series of jagged processes liable to injure the soft parts of the mother. The circumstance too of its counterparts being capable of separation, was not unimportant; for when the head was high, and it was necessary to secure an extensive grasp of the cranial bones, it was neither easy nor safe to do so without such an arrangement.

FIBRO-CARTILAGINOUS TUMOUR IN THE LEFT FALLOPIAN TUBE.
BY DR JOHN YOUNG MYRTLE.

During the examination of the body of a lady who had sunk from cerebral disease, induced by intemperance, Dr M. came upon a large tumour which completely filled the upper strait of the pelvis, extending from a little above the brim to within three quarters of an inch of the orifice of the vagina, and at

first sight looked like an enlarged ovarium. By dint of not a little cutting and tearing, it was with difficulty removed, along with the uterus and appendages, when it appeared to be a very firm fibro-cartilaginous tumour in the left fallopian tube, and in which, at its upper and lateral part, the left ovarium was partially embedded and firmly adherent; the uterus and right ovarium were healthy and normal in size.

The tumour, on being cut into, proved of all but cartilaginous hardness, and presented the most distinct appearance of concentric laminæ, of equal consistence, but varying in thickness—thicker at the circumference, and becoming gradually thinner towards the centre, the nucleus being a small roundish body of exactly similar texture. The tumour filled the pelvis so completely that no examination of a satisfactory nature could have been effected per vaginam, and by its habitual pressure had given rise to enormous distension of the colon. Might this tumour be the result of a morbid nutrition of an extra-uterine conception, commencing at a very early period, as tumours of this description are exceedingly rare, if, indeed, any are recorded in this situation, or was it merely a simple fibro-cartilaginous growth?

ON THE INTRA-UTERINE PESSARY. BY DR CUMMING.

Dr Cumming exhibited one of Dr Simpson's metallic uterine pessaries which he had that morning removed from a patient who had worn it without intermission for six months. The patient had suffered since the birth of her first child, seven years ago, from prolapsus uteri, complicated with hypertrophy and elongation of the cervix uteri, the prolapsus so great as to protrude beyond the external parts; this state of the parts had induced the general weakness and wretchedness of feeling so universal in such cases. Added to all this, there was retroversion of the uterus as ascertained by the finger, but still more satisfactorily by the uterine bougie. The elongated cervix was first excised, but the prolapsus was too great to be much benefitted by this operation. Dr C. therefore introduced the wire pessary, delineated in the Dublin Quarterly Journal—(No. for May 1848). The patient, for the first few days, complained of slight irritation, and during the first-mentioned period thereafter, the discharge was greater than natural, but in a short time she became so accustomed to the instrument, and it so adapted to the parts, that, though before its introduction unable to walk any considerable distance without great fatigue and uneasiness, she had, within the last month, walked more than seven miles, and with ease. It should be stated that the three last menstrual periods have been marked by a less discharge than usual. Finally, the patient submitted to its removal only on the express understanding, that if the prolapsus should return so as to make her at all uncomfortable, the instrument was to be re-introduced. Dr C. stated that he had brought forward the case, not certainly on account of its rarity, nor because he was able to report the final result, but because in the course of his reading he had recently observed statements made in regard to the uterine pessary and bougie, quite opposed to his own experience of them, and because this case, among many he had seen of a similar kind, tended to prove that the evils attendant on the use of these instruments were due, in the great majority of instances, not to them but to their imperfect and faulty introduction and adaptation.

CASE OF INVERSION OF THE UTERUS. BY DR SKAE.

Dr Skae stated that almost all the recorded cases of uterine inversion appeared to have been consequent to parturition at an advanced period of utero-gestation; this accident, in most of them, having been produced by the employment of injudicious force in attempting to remove the placenta. Later writers had pointed out the occurrence of this unfortunate accident under circumstances which entirely excluded the possibility of its resulting from mechanical violence of this nature, inasmuch as it was found, before the separation of the child, by division of the cord; at other times, when the funis had been so putrid as

to break by the slightest traction, and moreover in cases even where the child was expelled after the death of the mother in the absence of all manual interference. Other recent authorities on the subject, under the impression that the accident might happen with the best management on the part of the practitioner, had gone so far as to assert that it was by no means of very unfrequent occurrence. But while it might be fairly inferred, that the great majority of cases which took place were produced by imprudent interference, there appeared sufficient evidence to establish the fact that it was an accident which might occur, without necessarily implying any injudicious treatment on the part of the practitioner. The following case afforded strong corroborative proof of this view of the question, from the remarkable circumstance that inversion happened, in the absence of all foreign interference, at such an early stage of pregnancy as the fourth month, a period at which there was every reason to believe there was much less liability to the accident than after parturition at the full term of utero-gestation, when the ligamentous connexions of the uterus, the highly developed and relaxed state of the vagina, os internum and cervix, and the accommodating condition of the other pelvic viscera, all constituted so many predisposing causes to the occurrence of inversion. It took place under the following circumstances :—

I was requested, in January 1845, to go and see a Mrs F., æt. 36, in the fourth month of her eighth pregnancy, who, I was informed, was very ill, in consequence of having had a miscarriage. I found her in a state of great distress and exhaustion. She complained of severe headache, of intense pain in the back, and both iliac regions, but especially the left, extending down the thighs anteriorly, and even affecting both ancles. The pulse was small and weak, and ranged between 120 and 130, the skin cool, and covered with a clammy moisture. She stated that she experienced constant down-bearing pain, attended with flooding, and a sensation of something having fallen down within her. I was farther informed, that in consequence of considerable bodily fatigue which she had undergone two or three weeks previously in removing her family from Queensferry to this place, she had been seized with flooding on Wednesday the 15th (ten days before) ; that on the following day, abortion had taken place, after being four months pregnant ; that on Saturday the 18th, she had felt so well as to get up and attend to some household matters, but that the flooding had afterwards increased to such an extent, as subsequently to necessitate her confinement to bed ; that in consequence of feeling very weak, she had that morning (Saturday 25th), about ten o'clock, taken some wine and water, which she immediately vomited ; and in the act of vomiting, which was severe and continued, she was sensible of something falling down within her ; and from that time up to the present—half-past ten o'clock, P.M.—that sensation continued, along with down-bearing pain, flooding, much general uneasiness, and extreme prostration of strength.

On introducing the fingers of my right hand into the vagina, I found the passage nearly filled with an elongated and somewhat irregular spherical tumour of pretty firm consistence, and having shreds of membrane attached to it ; and on passing one or two fingers up to the os uteri, I could trace the neck of the tumour entering within it, and having an equally firm attachment, as it were around the whole circumference of its inner margin, the os internum itself being open to the extent of two inches in diameter, and being otherwise in a dilatable condition. Without withdrawing my right hand from the passage, I examined with my left the hypogastric region externally, and by this means satisfied myself of the absence of any intervening body corresponding to the uterine tumour. Having now no doubts as to the mass within the vagina, consisting of the uterus in a state of almost complete inversion, I immediately grasped the organ, and by moderate but steady and continued compression, in the direction of the os uteri, manipulating with my fingers as occasion required, I succeeded, in the course of some fifteen or twenty minutes, in returning the whole mass within it. In endeavouring, however, to push the fundus up to its

normal position, the resistance and accompanying pain was so great as to deter me from persevering, and I desisted from the attempt, contenting myself for the present with having returned the whole organ within the os tincæ. The poor woman appeared to experience immediate and great relief from what I had done, and expressed herself to that effect. Before leaving her, I ascertained that the hemorrhage had ceased, and ordered cold applications in the event of its recurrence, and trusted her to the effects of an opiate and absolute quiet till I should visit her next day. The following morning I found Mrs F. continuing apparently better ; she had slept none during the night, but had felt tolerably comfortable. On examining per vaginam, however, I found the fundus uteri projecting a little beyond the os tincæ, which had contracted considerably since my previous examination ; but I was able with little difficulty to myself, and only trifling pain to my patient, to push it upwards to its proper relative position within the pelvis. Recovery was slow, but uninterrupted ; and, with the exception of slight occasional pain in the left ilium and both ancles, she is now in perfect health, and has menstruated naturally since the occurrence. She has since been again pregnant. When labour came on, and after the os uteri was well dilated, the entire ovum was expelled at one pain, and without rupturing the membranes. The child was alive and healthy.

In this case there could be little doubt as to the accident having happened twelve hours previous to the time of the organ being returned. The immediate cause of its occurrence must have been the forcible contraction of the abdominal walls which play such an important part in the act of vomiting. According to the statement of Mrs F., it was during the violent retching that for the first time she was sensible of something falling down within her. Up to this period she had complained principally of hemorrhage and accompanying debility, but, after the vomiting, her sufferings were greatly aggravated, and continued to increase in severity, till the displaced organ was returned. The continued hemorrhage, which had been going on during ten days previously had produced a very relaxed state of the os uteri, which no doubt was greatly favoured by constitutional predisposition ; and such a condition of the parts was but little calculated to offer resistance to the escape of the uterus, forced down, as it must have been, by the powerful action of the diaphragm and abdominal muscles on the contained viscera.

Dr Skae had seen another case of partial uterine inversion about six years previously. It happened in the hands of a midwife, in consequence of her having used unjustifiable traction of the funis, with the view of extracting the placenta. He found, on examination, the fundus uteri filling the cavity of the pelvis, having completely protruded beyond the os tincæ, and having the placenta still adhering to it with considerable firmness. The patient exhibited symptoms of exhaustion to some extent, and complained of down-bearing pain, extending along the loins, iliac regions and thighs ; there was hemorrhage also, but not in any material quantity. In this case the separation of the placenta and return of the displaced fundus were speedily and easily accomplished, and no bad symptoms retarded recovery, although, since that period, the patient has suffered from prolapsus uteri.

CASE OF INVERSION OF THE UTERUS. BY DR ZIEGLER.

Dr Ziegler detailed the particulars of a case which he had witnessed. On the 5th March 1841, he was requested to visit Mrs M., ætat. thirty-two, who had been naturally delivered of her fourth child about two hours before his arrival. Immediately after the extraction of the secundines the patient had been seized with severe pain across the loins, accompanied with faintness, a sense of dragging within the pelvis, and violent abdominal expulsive efforts. These symptoms the medical gentleman in attendance had endeavoured to relieve by repeated doses of laudanum, exhibited by the mouth, and also in form of enema. The placenta was shown to Dr Z., surrounded by about two pounds

of blood; it was much broken up, and the funis disrupted at its placental connexion. Suspecting from these circumstances the nature of the accident, Dr Z. requested to be conducted immediately to the patient, whom he found in a state of hopeless exhaustion, with features collapsed, and countenance deadly pale; hurried breathing; no pulse at the wrists; and the surface of the body bedewed with a cold perspiration. On removing the binder which had been applied round the abdomen, Dr. Z. was instantly struck with the absence from the hypogastrium of the ordinary firm, globular, uterine tumour, and on pressing his fingers more deeply into the pelvis, he became sensible that the fundus of the uterus had been doubled down, having a "cuplike cavity," with firm and well-defined edges. That inversion had taken place was sufficiently apparent from this external examination; but on slipping off his coat, and carrying his hand into the passages, his suspicions of the nature of the accident were amply verified. The whole vagina was occupied by a large, fleshy, elastic swelling, with a rough and bleeding surface, and having its neck encircled by the os and cervix uteri.

Having got the patient to swallow some brandy, he proceeded immediately to restore the inverted organ to its place. With this view, grasping the tumour with the right hand, and firmly compressing that part of it which had been last inverted so as to diminish its bulk, he pressed it steadily upwards and slightly forwards in the direction of the axis of the brim, while, with the left hand over the pubis, he endeavoured so to moderate the amount of upward pressure as to prevent the danger of tearing the organ from its vaginal connexion. In this way, and after a few efforts, the return of the uterus was so far accomplished as to leave only a portion of its fundus depressed about the size of half an orange. And now, applying his fingers circularly around this remaining part of the tumour, and continuing to exercise compression of its bulk with the steady pressure upwards, it suddenly bounded from his hand with the elastic spring of a piece of caoutchouc. The reduction being now complete, the hand was carried onwards to the fundus, and moved along its external surface, while brisk friction was practised externally, with the view of provoking the uterine fibres to contraction. For some time these means were ineffectual, the uterine parietes remaining soft and flaccid, while blood continued to ooze from the mouths of the vessels, reducing still lower the ebbing powers of the patient. With much difficulty one drachm of ergot, in brandy, was swallowed, and warm flannels were applied assiduously to the epigastrium and extremities, and by steady perseverance in the frictions externally, such degree of uterine contraction was at length established as was sufficient to put a stop to the hemorrhage, and prevent a return of inversion.

But although the means were thus far successful, the patient never rallied, the pulse at the wrist never returned, the power of swallowing was lost, and she fell into a protracted syncope, and after a convulsive movement expired.

Judging from the lacerated appearance of the funis, after birth, Dr Z. had at first too hastily concluded, that the cause of inversion in this unfortunate case had been undue traction on the cord, before the uterus had sufficiently contracted to separate and throw down the placenta. But in this view he was probably mistaken, as the medical attendant assured him that no violence had been employed, and no difficulty experienced in the removal of the placentary mass; and that the cord, though feeble, was of the ordinary length, and not twisted around any part of the infant. The child had been rapidly expelled. From this statement he was perhaps warranted in regarding the case as an example of what had been termed spontaneous inversion, depending, not on mismanagement, or any force applied to the funis, but more correctly referable to an irregular and spasmodic contraction of the fundus and body of the uterus itself, with an atonic condition of the os and cervix. But whether the one or the other was the real cause, it could not but be matter of regret, that the nature of the accident was not sooner detected, so that the appropriate means for replacing the uterus might have been more early adopted.

REPORTS ON CHOLERA IN SCOTLAND, 1848-9.

DUMFRIES.—*(From Dr Grieve, Physician to the Dumfries and Galloway Royal Infirmary, and Consulting Physician to the Crichton Royal Institution).*—The history of cholera as it prevailed in Dumfries in 1848, presents no small analogy to that which prevailed in 1832; indeed in many respects it proved an exact counterpart, its rise, progress, decline, habitats, duration, number of persons attacked, and comparative mortality, being as nearly as possible, the same; nor did the meteorological phenomena differ to any great extent, the malady continuing to rage with the same remarkable severity, apparently independent of, and uninfluenced by every possible variety of weather and temperature. In regard to the much vexed question, is cholera contagious or not? I have carefully, and without having any preconceived opinions to support, examined and dispassionately weighed all the facts connected with the history of both epidemics, and I am bound to say, that I have no rational grounds for believing it to be contagious; on the contrary I believe that it has its origin in, and is propagated by other sources than the communication of the sick with the healthy, and that exhalations from the human body have not the power of reproducing the disease.

If it spreads by contagion at all, it is unusual, and exceptional, and under circumstances which science has as yet failed to recognize. I believe the choleraic virus, or a materies morbi exists in, and is diffused through, the medium of the atmosphere, but under what peculiar circumstances this malaria is generated I am at a loss to determine; certainly it appears to be independent of, and unconnected with effluvia arising from the decomposition of vegetable and animal matters.

I shall now endeavour to prove that cholera as it prevailed in Dumfries in 1832, and 1848, had a spontaneous or sporadic origin, and that no sources of contagion or infection existed, from which in either epidemic the first cases could have contracted the disease; and that its diffusion can only be satisfactorily accounted for, by admitting the influence of epidemic causes. In 1832, the first case occurred on the 15th September, and in 1848, on the 16th November; and it has been proved beyond a doubt, that neither of these individuals had ever been in contact with infected persons, or in any district where cholera prevailed; in fact, on both occasions when the disease broke out in Dumfries, the nearest infected town was Edinburgh, with which neither of the individuals, it has been satisfactorily ascertained, had any communication direct or indirect.

The next well authenticated cases did not occur (as would naturally be supposed) if contagion had any thing to do with it in the same house or tenement, or in the same street or locality, or amongst the attendants or relatives, but in different parts of the town, not one of these persons had seen, or had any communication with those first attacked; nor have I found in my experience of both epidemics in Dumfries, that those more particularly exposed to the influence of contagion by attendance on the sick, viz:—medical men, nurses, and relatives, were more liable to be attacked with the disease, than others not so exposed. Not one of our nurses in Dumfries was seized, though engaged night and day, in all possible conditions of moral and physical depression, in attendance upon the sick. Upwards of eighty cases of cholera were treated in the Infirmary, and when the first case of cholera was admitted, there were about thirty patients in the house, none of whom took the disease. No pains were taken to isolate them from the infected; on the contrary several of them volunteered and were permitted to attend upon the sick, and continued to do so during the prevalence of the epidemic, which extended over a period of two months; and during this time, of all those in attendance upon the sick, nurses and medical men included, only *one nurse* was attacked; and she it has been proved, had a day or two previously been in the town, thus exposing herself

to epidemic influences. The Crichton Royal Institution, situate in the neighbourhood of Dumfries, was placed by the superintendent, Dr Browne, under a restrictive cordon, and no cases of cholera occurred in the Institution. This, however, can have little or no influence on the question of contagion, inasmuch as there were other Institutions, such as the Alms-houses, in which no cases occurred, though no similar quarantine measures were had recourse to. No cases occurred in the House of Refuge, a fact which in my opinion is strongly corroborative of the non-contagious nature of epidemic cholera. Let us suppose that instead of cases of cholera, we had an equal number of persons removed from houses or localities, in which acknowledged contagious diseases, such as measles, small-pox, fever, whether scarlet, puerperal, or typhus had prevailed, would it not, I ask, have been considered extraordinary if some instances of these diseases had not been manifested among the refugees?

We have numerous and well authenticated instances of persons residing in the surrounding districts and neighbouring parishes, who having come into Dumfries for a longer or shorter period, but without having seen or come into contact with infected individuals, have returned home and been taken with cholera, without any extension of the disease to their relatives or attendants. Cases in point have occurred in the parishes of Dumfries, Troqueer, Tinwald, Caerlaverock; and in the town of Annan, sixteen miles from Dumfries, several cases of cholera occurred during the epidemic of 1832, all of them imported cases, the disease not extending to a single inhabitant of Annan. In short, I have had occasion to observe during my experience of both epidemics, innumerable instances of exemption from cholera under the most adverse and depressing circumstances. With the above facts before us, it is impossible not to recognize in the spontaneous origin, the rapid diffusion, and in Dumfries equally rapid decline, of cholera, a prevailing epidemic influence, and which contagion *per se*, would fail to explain or account for.

FALKIRK—(*From Dr Hamilton*).—The first case of cholera which occurred in this locality during the present epidemic, was imported. A Highlander, in search of employment, from Dalkeith, Edinburgh (where cholera then prevailed), and Borrowstouness, in succession, was seized with cholera between Borrowstouness and Falkirk, and came to the office of the parochial board here on November 2, where he lingered for some time and died. He passed through the village of Laurieston, a mile and a half to the east of Falkirk, but I cannot say whether he halted there.

On November 13, a case occurred in Laurieston, which was then reported as only "suspicious," but which, now that I have seen this epidemic, I have no hesitation in pronouncing Asiatic cholera, which proved fatal in thirty-five hours. On December 16, a second child, lying in the same bed as the preceding, was seized, and died in thirty hours. In a few days afterwards a third child, in the same bed also, was seized with similar symptoms, and recovered. And on December 6, the mother of these children, still occupying the same bed, was seized with blueness of surface, cramps, and vomiting, but she also recovered. The husband, the only other member of the family, had also diarrhœa; but no other cases showed themselves in this village for some time afterwards.

On December 6, a man Broom, of dissipated habits, living in a damp public house on the side of the Forth and Clyde canal, two miles from Laurieston, and about half a mile north from Falkirk, was seized with particularly well marked symptoms of the disease, and died. Immediately after this date, there occurred in this locality, on the sides of the canal and of the Carron river, at Grangemouth, two miles to the east, and in other places in this neighbourhood, a rapid succession of cases, amounting probably (for they have been very irregularly reported) to about 400.

The virulence of the epidemic at its first outbreak was extreme. After

Broom's case, 39 others are reported, up to January 30, of whom only 7, or 1 in $5\frac{1}{2}$, recovered.

Immediately succeeding these cases, under nearly the same treatment, at about two miles distance, nearly 2 out of every 3 seized recovered, a statement which shows the futility of deductions as to treatment from mere statistics in this disease.

One of these, William Rae, a strong labourer, without, it is stated, any premonitory symptoms, was seized, and was seen within twenty minutes by Mr Thomas Girdwood, surgeon, and by myself within half an hour. We both found the pulse at the wrist already extinct, and the surface cold and blue. I have not heard of any case here which proved fatal in less than four hours; but one man I am aware died after being ill for that space of time.

The general characters of the epidemic have been very much the same in this quarter as I have seen described in the periodicals, as occurring in other parts of the country. Cramps have often been absent, especially in children, who have suffered in a large ratio, and frequently neither the purging nor the vomiting has corresponded to the general severity of the affection.

In the severer forms of the disease, I have not seen, and I have not heard of, any particular treatment that has here proved successful. A few cases have recovered which appeared almost beyond hope, without any particular treatment apparently having produced this. When of a milder, though still very severe type, opium or laudanum, combined with stimulants, internally and externally, a cloth soaked in turpentine and applied extensively over the abdomen, with a very hot cataplasm above it, have in my own practice seemed to me the means that could be most relied on for controlling the symptoms.

Damp localities, filthy dwellings, externally and internally, and especially confined, ill-ventilated, and overcrowded apartments seem to me to have been here among the chief immediate causes of the development of the disease. In the houses of the collier population, to which I shall immediately refer, as well as in a large proportion of the other houses where the disease broke out, no proper ventilation could be obtained, from the windows and doors all opening towards the same direction. If we add to this the circumstance, that among this class, as remarked to me by one of themselves, a cholera case usually proved nearly as attractive in drawing them together as a marriage festivity, we shall have explained to us, I suspect, the chief cause of the activity of the pestilence in these instances.

We seem to have pretty conclusive evidence of the extensive diffusion of some unknown and unusual agent in this district, from the almost universal prevalence before cholera appeared, and since then, of diarrhœa and of flatus in the bowels, as well as from the capricious mode in which the epidemic has, in some instances, broken out in different and distant localities. I do not see, for example, how we can otherwise explain the occurrence of some of the first cases, especially that of Broom's, and of some others which immediately followed. The agent referred to seems to have been sufficiently powerful to have produced these spontaneously, or if they were connected with previously existing cases, the link in the chain of connexion has escaped our notice. As the epidemic proceeded, however, the case became altered. I think there have been numerous instances in which the disease was propagated in a manner similar to what we see in typhus or in scarlatina; or, in other words, it has shown itself to be contagious. Of this I shall give a few proofs.

1st. On December 31, 1848, a person named Ure came from Shotts, sleeping one night near Coatbridge, where cholera prevailed, to Stenhousemuir, three miles from Falkirk, where no case of cholera had up to that time been reported. He was shortly after seized with cholera, and died in twenty-four hours; and in two days afterwards, in the same house, his mother was also seized, and died in twelve hours. The following may be taken as another example of the same kind: Mrs Walker, living in Bainsford, a mile or so north from Falkirk, was seized on January 13, and died. She was attended by Mrs

Johnston, who was seized on January 15, and died in her own house at some distance from Mrs Walker's ; and on January 16, her husband, Henry Johnston, was also seized and died. Or again, about the same date, and in the same locality, a man, Ure, was seized and died, and his daughter, who attended him, was immediately after seized and died in the house of a Mrs Anderson, who in a day or two afterwards was also seized, but recovered. Here is another striking case of the same kind : Margaret Mellis, living in Bainsford, was seized on January 4th, and died on the 5th. In two days afterwards her mother was seized in the same house, and died. While the mother was ill, another daughter, living in Falkirk, went to see her, stopping a night in the house. She was seized immediately afterwards, and lay in another sister's house in Grahamston, about half a mile distant, who, as well as one of her children, was immediately also seized, the two sisters lying in the same bed. Or, here is a still more striking instance : A man, Brodie, lived at the beginning of February, in a house in Falkirk, where several cases of cholera had occurred. He was in Laurieston (in which place no case of cholera had occurred for five weeks) on February 5, and went into the house of William M'Gregor, where he was seized with cholera, and was removed next night in a cart to his own house in Falkirk, and died. On February 7, Mrs M'Gregor was seized with diarrhœa. On February 8, her child, two years old, previously quite healthy, was seized with cholera, and died ; and on February 10, Mrs M'Gregor was also seized, and died on February 12, on which day William M'Gregor was seized, and died on February 16. On February 15, Marion Crichton, a woman living in a room above W. M'Gregor, and who was frequently in his house, was seized, but recovered ; and on February 16, Jane M'Gregor, sister of W. M'Gregor, and living next door to him, was seized and died. Another woman, Ellen Renny, who acted as nurse to M'Gregor, had diarrhœa on February 9, and was seized with cholera, in Jane M'Gregor's house, on February 16 ; and on the same day her child was also seized. Still more, on February 20, Agnes Smith, another nurse to these parties, was also seized with purging, cramps in the belly, and blueness of surface.

2d. The same thing is strikingly shown by merely looking at the chain furnished by the dates at which the disease attacked, in Bainsford and Falkirk, several families connected by relationship, and who naturally had constant intercourse with the parties affected, but whose houses were situated at considerable distances from each other.

On December 16, Mrs Thomas Miller, living in Bainsford, was seized, and died on the 17th. She was attended by her sister, Mrs Rankine, from Falkirk (where cholera was not then prevailing), who was seized on the 19th, and died in her own house, after lingering for some weeks. On the 30th, Mrs Rankine's daughter was seized, and recovered. On December 24th, Mrs Thomas Logan, living in Bainsford, sister-in-law of Mrs Miller, having previously been affected with diarrhœa, was seized, and died on the 25th. Mrs Charles Logan (mother-in-law to the above, but living separate from her) dressed the dead body of this patient, and was seized on the 26th, and died on the 27th. On December 30th, Charles Logan, son of the preceding, and living in the same house, was seized. On the same day, John Millar, father of Mrs T. Logan, was seized, and died ; and on January 4, Thomas Logan, husband of the same, was seized, and recovered. On January 5, William Logan's wife was seized, and recovered ; and on January 8th, William Logan's son was also seized, and recovered. It is further to be observed, that excepting this string of relations, and the cases previously mentioned as having occurred in Bainsford, only five other cases are reported as occurring there from December 16th to January 8th, although there are in this village about 250 families.

3d. It seems to me that the same conclusion must be arrived at from observing what occurred at two collieries in this neighbourhood, where the disease broke out, the particulars connected with which have been kindly furnished to me by Mr T. Girdwood and Mr Coubrough, surgeons here. It first made

its appearance at Skinflats colliery, near Grangemouth, ten to twenty feet above the level of the sea. The houses inhabited by these colliers, consisting generally of two rooms, are built on a clayey soil, are as usual with this class dirty, internally and externally, and are badly constructed, there being no proper means for ventilation, as has been already mentioned. The number of houses inhabited by the colliers when the first case occurred was 40 ; the number of persons living in these was 273, and the number of families 48. This gives an average of 7 to each house. The disease broke out on January 3, and the last case occurred on February 9. During that period 83 persons have been affected, and 36 have died, giving an average of nearly 1 in 3 affected, and of more than 1 in $7\frac{1}{2}$ dead. Of the forty houses occupied, there are only three into which the disease did not enter.

It is instructive to notice the course pursued by the epidemic in this locality. The colliers' houses form a continuous line, running from south to north, for a few hundred yards, being at their northern extremity connected with the small village of Old Skinflats, which consists of eleven families (thirty-eight individuals) not colliers, besides a few collier families also. Now, it is remarkable, that cholera, which broke out a little to the south of Old Skinflats, had attacked a great part of the "colliers' row," and had on the 8th and 9th attacked two collier families in the village, but had not, up to January 13th, when about sixty colliers had been seized, affected a single individual of the eleven families living in Old Skinflats. On the 14th, however, it broke out in the house of a person of the name of Millar, who kept a public house, and who, though he would not allow the colliers to drink in his house, I am informed, supplied drink to those who came for it, at all hours. This family suffered severely, six were seized, and five died. The only other of the thirty-eight inhabitants of the village affected, was a Captain ———, of irregular habits, who, it is stated, was in Millar's house getting whisky, immediately previous to being seized. These facts seem to me precisely analogous to what I noticed in 1847, among the railway labourers living in Camelon, near Falkirk, when typhus spread extensively among them, but attacked in a small proportion the other classes (mostly nailers, sufficiently dirty in their habits too) among whom they resided, and the explanation of the facts will be found I think the same in both instances. The other inhabitants had little intercourse with either the colliers or the railway labourers, and therefore remained comparatively free from the diseases which attacked both.

I have not been able to find any distinct connection between the first cases at Skinflats, and those at Grangemouth, Bainsford, or any other place where the disease at the time prevailed. In Kinnaird colliery, however, in which it broke out on January 9th, the connection with Skinflats, from which it is distant rather more than a mile, is very distinct. Jean Marshall had been attending her sister, affected with the disease at Skinflats, and was sent home to David Marshall's, her father, on the 9th, while affected with the premonitory symptoms, and became very ill, passing into the coliapse stage, but recovered. Peter Marshall, uncle to J. M. (who resides a quarter of a mile distant from David Marshall, but who had, it is stated to me by the Inspector of the parish, been in David Marshall's house immediately previous to becoming affected), was seized on the 12th, and David Marshall, father of J. M. on the 14th. On the 18th, Robert Duncan, residing next house to Peter Marshall, and who rubbed him, and was often in his house, was seized ; and within six days from this date, two children and his wife were also in succession seized. The whole four died, and other cases immediately followed in the same locality. On January 24th, a sister of Jean Marshall came to visit her from Carronshore, a neighbouring village, and on going home was seized and died. Shortly afterwards this person's husband was also affected, in Carronshore. Many other facts, similar to those I have mentioned, might, I have little doubt, be collected in this neighbourhood; and if we admit, that in these contagion was probably the means by which the disease spread, the presumption becomes very strong that, in the still more numerous class of cases, where two, three, four, five, or even more

were seized in succession in the same room, and often in the same bed, contagion there also was the chief cause of the diffusion of the disease. It is to be remarked also, that mere filthiness will not account, in many cases, for the rapid spreading of the disease in families. Millar's, for example, at Skinflats, was a remarkably clean and well-kept house, and yet six were affected in it.

Continued contiguity to the sick, confined apartments and want of ventilation in these, and the officious crowding of friends or strangers into the house of the affected persons, have certainly appeared to me the chief *immediate* causes of the rapid diffusion of this disease. None of the surgeons, who generally remain only a short time in each house, has been affected in this district, but several nurses, as has been mentioned, have fallen victims to it. Whenever I have been able to enforce due attention to isolation and ventilation, I have seen no instance in which more than one individual became affected in a house; whereas, where this has not been attended to, the list of instances in which more than one person has been seized in a family is truly startling. Reckoning nurses or attendants upon the sick, for the time being, as members of the family, I am aware, including the whole cases, of forty-seven instances in which two in the same house became affected; of twenty-three instances in which three in the same house became affected; of three instances in which four became affected; of six instances in which five became affected; of four instances in which six became affected; of two instances in which seven became affected; and of one instance in which eight in one room were affected. In two confined and filthy tenements, included in this list, twenty-four were seized.

Some interesting facts bearing upon the question of contagion, have been communicated to me. Mr Coubrough, surgeon here, states to me that Mary Easton, residing at Kinnaird, washed the bed-clothes in which Andrew Turnbull died (cholera being the disease), and was shortly afterwards seized with the same, and Mr Ronald, surgeon here, has mentioned to me an exactly similar case which occurred to him in Stenhousemuir. Another fact is communicated to me by Mr Waddel, surgeon here. Two grand-children of Mr A.'s, residing in Grahamston, part of the burgh of Falkirk, were brought to Mr A.'s house from Kilsyth, where both their parents had, a day or two previously, died of cholera. Within three days Mr A. was seized; his wife, and also the nurse who attended him, followed, and all three died. It may be remarked here, that very few cases had occurred previously in this locality, but since that time (February 6th), a number of others have occurred in rapid succession. I have already had occasion to make a similar observation as to other places more than once in this report.

Mr Adams, surgeon, Grangemouth, has communicated to me a singularly interesting case as connected with this question. Miss —— came from Glasgow, to visit her friends at Grangemouth, on December 26th, in both of which places cholera was then prevailing. She was seized the same night, and died next day. The bed-clothes were all subsequently cleaned, and the room fumigated. On February 17th, upwards of seven weeks after this lady's death, Mr and Mrs ——, having for a night or two occupied the bed and bed-room, in which Miss —— died, were both seized with symptoms of cholera, but recovered. At the latter date, cholera, which had for a fortnight disappeared from the town, attacked four or five other individuals.

Immediately subsequent to the disappearance of cholera at Skinflats, a low fever attacked some of the inhabitants, and, about February 22, scarlatina also showed itself. What relations do the poisons producing these three affections hold to each other? Are they different, or are they only modifications of the same subtle agent? Bearing upon this point, I had occasion to see a curious case:—A girl, who had been resident at Skinflats, was removed (February 10) to Stenhousemuir, two or three miles distant. She had been complaining ever since, and I had to see her on February 19, when she appeared to me to be

labouring under the low fever I have mentioned. On February 22, she was attacked with cholera; and in about ten days afterwards, a sister, who lay in the same bed, was also seized with the same.

Larkhall, near Hamilton—(*From Dr Weir*).—The village, in general, is situate in an airy and healthy localty, being for the most part built on an eminence or ridge between the rivers Clyde and Avon. Typhus fever is generally less prevalent and less fatal here than in other villages in the neighbourhood. The population amounts to nearly 3000, mostly composed of weavers, and a few colliers, which classes, from the nature of their trade, are often placed in peculiarly poor circumstances. The houses for the most part are clean, and well ventilated, but in a certain part of the village, of a lower situation than the rest, the houses are very damp and unwholesome, the drainage very bad, and the dunghills, &c., close to the walls; this part, likewise, is inhabited by a worse class of people, accustomed to indulge freely in spirituous liquors; it was here to the greatest extent, I may almost say wholly, that the cholera prevailed.

The disease commenced about the 1st December, and to the 10th February there have been upwards of fifty cases, of which seventeen have proved fatal. More children under fourteen years of age were seized with it than adults; but on the whole they seemed to withstand the virulence of the disease better, and recovered more easily, than the old people; nearly all the fatal cases were of adults. Those of intemperate habits stood the disease worst of all; although living in comfortable houses, with plenty of food and fire, they suffered more, and died in a shorter time, than those who lived in damp localities, exposed to both cold and hunger.

Larkhall is about four miles south from Hamilton; and the nearest place where cholera prevailed at the time it first appeared, was Coatbridge, about twelve miles distant.

With regard to the transmission of cholera no accurate facts are known, and it is doubtful whether it may have been brought from other places, or originated spontaneously. There was always a constant communication kept up between this and Coatbridge by the colliers. Previous, however, to the first case of cholera, diarrhœa was present to a great extent.

Though not so contagious as small-pox or measles, which usually attack once in a lifetime, yet so far as I have experienced, if typhus fever be contagious, the same may be said of cholera, it occurs in the same localities, for the most part attacks the same class of people, and follows exactly the same laws as typhus fever. Nurses, friends, and next door neighbours are more liable to be seized with it than those who do not come in contact with it, though living in the same locality.

Moffat—(*From Dr Matthew Paterson*).—On the 9th of December 1848, cholera made its first appearance here, and its first victim was a young man named Robertson, reputed to be of intemperate habits. He left Dumfries to join his father and family, who had come to Moffat shortly after the outbreak of the epidemic in Dumfries. Robertson was seized before he reached Moffat. He died about three hours afterwards.

On the 25th of the same month, a shoemaker's wife of the name of Ewart was taken ill with cholera in the seventh month of pregnancy, and died after twenty-three hours illness. In this case the exposure was indirect; she was in the habit of frequently visiting her husband's cousin, who washed the clothes belonging to the deceased Robertson. On Saturday, the 30th December, Ann Blyth was seized with cholera, and died after thirteen hours illness. She had washed out the house and clothes of the deceased Mrs Ewart. On the 2d January 1849, a man named Ewart, uncle-in-law of the deceased Mrs Ewart, was seized, and died from the complaint after a short illness.

On the 6th January 1849, another case occurred; this was the only one that did not terminate fatally; and, so far as known to me, was not exposed directly or indirectly to the choleraic contagion.

On the 27th January a young man, John Walsh, from Selkirk, "*en route*" to Thornhill, was seized when about thirteen miles from Moffat, but continued his journey till he came to a shepherd's hut, about eight miles from Moffat. He died after an illness of twenty-one hours.

MAUCHLINE, AYRSHIRE—(*From Mr J. Mitchell, Surgeon*).—The disease has appeared here in the endemic form, and as yet has confined itself to one locality. This locality is bounded on one side by a churchyard wall, and on the other by a stream which runs through the centre of the village, and performs the office of a common sewer. All the cases, fifteen in number, have occurred in the immediate neighbourhood of this stream. One half of the cases were persons of very dissipated habits, while in all there was a very obvious destitution. Besides these fifteen cases, we have had many cases of diarrhœa, which might possibly have lapsed into cholera but for treatment. In October, November, and December of 1844, we had nearly as many cases of typhus fever, and with two exceptions, they occurred in this very locality. The confinement of the disease to this locality is all the more strange, as it is by no means the worst in the village; and in a place called the Cowgate, which would rival the old wynd of Glasgow, not a case of diarrhœa even has occurred to my knowledge save one. With regard to the typhus of '44, the most of the cases could be easily traced to contagion. In the present disease I cannot say that one case can be so traced. We have had cases occurring on the ground floor of a house on one day, and in the upper flat the day following, but no contact of the parties; while, in a case which I shall not soon forget, a man lay with his *mistress* for the greater part of a night, the latter being collapsed (and indeed dying by his side) and yet he escaped. None of the attendants have been seized except in one case, where a young woman, being in attendance upon three patients simultaneously taken ill in the same house, was herself affected on the following morning. On the whole, I should say that if contagious, it is greatly less so than typhus; but little dependence is to be placed upon any conclusion arrived at on a review of such a limited number of cases.

DALRY, AYRSHIRE—(*From Dr W. B. Hamilton*).—Cholera appeared here on 1st January, cases occurring at intervals of two or three days till it reached its height about the 30th, when it was very general and fatal; since that it has gradually declined, though there are still cases occurring here and there.

We could not trace the first case as being imported from any infected locality. It has observed the same course, in regard to locality, that it did in its attack in 1832; so that, were a third attack impending, the inhabitants of certain tenements here would feel particularly uneasy. These localities are generally subject to fevers of a typhoid character.

I have seen nothing to make me consider it a contagious disease. I am more inclined to think it in some way associated with defective drainage. I think this opinion is well supported by its course here.

In an isolated house, in a street where not a case had previously occurred, I saw five persons struck down with it within three hours of each other—these persons were living in different families, and for the most part not aware of each others' illness: these five all died. The houses on either side lost two each, and the house right opposite lost one. There have been no new cases since. It was like a discharge of grape-shot. In this case I think the objection of defective drainage applies. The patients were in comfortable circumstances, and of cleanly, temperate habits.

BORROWSTOUNNESS—(*From Mr Forbes Angus, Surgeon*).—The cases of cholera which occurred here under my care, were seven in number.

They were all in one badly ventilated close, with the exception of two. One of these was the case of a midwife, and the other in that of a tavern-keeper. Both were strong women, and the midwife was in close attendance upon the tavern-keeper. The husband of the latter came from Grangemouth with the disease upon him. Three women lived under one roof. Two recovered and one died; the mother of one came from a distance to the funeral, and died in a few hours' illness.

There were three recoveries and four deaths in all. A child who suckled in a case where the mother died, lingered with every symptom of cholera, and died in two days.

My faith is much shaken in the supposed non-contagion of the disease.

It must be noticed, however, that the state of the drainage in the only locality where the disease appeared, was horrible, and that there was an interval of ten days between the first four and the last three cases.

Another case of doubtful nature was observed in a boy of five years old, who recovered.

Coldstream—(*From Dr Turnbull*).—The first case of cholera reported occurred in a man, who was seen, I believe, only a very short time before his death. He had not, to the best of my knowledge, been exposed to any contagion. The person who dressed his body took the disease, and died; and, I believe, the same fate befell one of the women who dressed her body.

All these cases occurred in one locality. None of those cases having been seen by either my father or myself, I cannot give any accurate information respecting them. The following facts, however, are what came under our own personal observation. About the time the above cases occurred, my father was called to two cases in separate apartments in one house, in a distant locality from the other cases. Both cases occurred at the same time, and both died the same day; neither had been exposed to any contagion whatever; and, although the house was damp, ill ventilated, and crowded, no other case occurred in it.

We were called to many cases where one only of a large family was seized, notwithstanding the constant communication between all the members of the family. One girl we were attending for small-pox was seized, while confined to bed from this disease, and died after a few hours illness. She had certainly not been exposed to contagion; and, although there were many persons living in the house, which was situated in a very bad locality, no one else was seized. We almost never, in fact, saw two cases in the same house. In one house, however, which was in a most filthy state, two persons died shortly one after the other. A third person was seized in the adjoining room, and died; and after her death a family went to reside in the house, shortly after which one of the members took it, and died.

In by far the greatest majority of cases we could trace the disease to no contagion, and it generally seized only one member of a family. Of this fact I could give very numerous instances. Judging from what we saw here, I believe all the medical men arrived at the conclusion that it was not, strictly speaking, a contagious disease; and the cases (of which there were several) which occurred in the country, especially led us to arrive at this conclusion.

Niddry, Mid-Lothian—(*From Dr Fisher, Portobello*).—The village of Niddry is situated about three miles to the south-east of Edinburgh, being nearly equidistant from the city and Musselburgh, and about two miles directly south of the Frith of Forth. It consists of a straggling row of houses, about thirty in number; the locality is low and damp; it is shaded from the south by a grove of trees, and is enclosed in a semi-circle by a small stream. The houses are mostly of a very wretched description, many being thatched, and destitute of flooring and ceiling. The drainage, before the appearance of cholera, was most defective, indeed scarcely any existed; the houses were surrounded by accumu-

lations of filth, and an enormous manure-heap, composed of night-soil and urine from the adjacent city, lay in the immediate neighbourhood. The majority of the inhabitants were in destitute circumstances; some received employment as day labourers, but not a few were in the receipt of parochial aid. The number of inhabitants at the outbreak of the disease was 138, of whom 70 were adults, and 68 children. 17 persons were attacked with cholera in the village, and 2 in its *immediate* vicinity; in all 19 cases, 10 of which proved fatal.

The disease first manifested itself on the morning of *1st November* 1848, in the person of Janet Walker, age 11, who died the same morning. This girl had been previously in good health, and had not been in communication with any place in which the disease prevailed. Mrs Nicholl, age 43, was attacked the same evening, and died next day. This woman was in attendance on the first case, and assisted in dressing the body. She was seized in about an hour afterwards with the disease in the most decided form. A woman named Tait, who had been for some time resident in the village, went into Edinburgh on the same forenoon, that of 1st November, and was there, in the course of the evening, seized with cholera. On the *3d November*, Mrs Callender, age 38, was attacked and died on the 4th. She had not been in communication with the sick. On the 4th, Peter Banks, age 37, was attacked, and died on the 6th; no communication with sick. On the 5th, Peter Nicholl, age 12, in communication with mother, attacked on 1st; recovered. Same evening, David Preston, age 30, had been attending the previous case during the day. He died. On the 6th, Lawrence Nicholl, age 7, recovered; mother and brother previously attacked. On the 7th, Robert Dixon, age 64, died same day; no communication with sick. On the 9th, Mrs Hall, age 33, recovered; no communication with sick. James Callander, age 7, died on 21st; mother was seized on the 3d. On the 13th, Isabella Banks, age 6, recovered; father attacked on 4th. On 15th, Rubina Nicholl, recovered; mother and brothers previously attacked. This was the fourth case in Mrs Nicholl's house. This girl was again severely seized on 23d, and a second time recovered. On 18th, Jessie Banks, age 3, died; father and sister attacked on 6th and 13th. On 21st, Mary Robertson, age 45, died same evening; no communication with sick. Grace Murdoch, age 29; died; no communication with sick. Ann Dixon, age 60, recovered. Husband attacked on 7th. On 26th, Thomas Robertson, age 7, recovered; mother attacked on 21st. W. Moore, age 5, recovered; no communication with sick. On 28th, Isabel Denholm, recovered; attended Murdoch and Robertson.

Thus eleven of the nineteen cases had been in actual contact with the sick; in the remaining cases, no communication, or even proximity, excepting that of residing in the same village, could be traced. With regard to the introduction of the disease in the case of the girl Walker, on careful inquiry there could not be found the slightest ground for supposing that she or any of her family, had been near any sick person. Supposing, for the sake of argument, that cholera is contagious, I would ask what is its period of incubation? Enquiring into the Niddry cases on this supposition, the results are most incongruous. For example, Mrs Nicholl, and David Preston, were attacked within at most one or two hours after exposure; while James Callander, a weakly child, and Mrs Banks' children, were seized after an interval of six or seven days. Five days intervened between exposure and attack of T. Robertson, and fourteen in the case of Anne Dixon.

It is impossible to give the proportion of those attacked, to those in contact with the disease, with anything approaching to exactitude, but I have no hesitation in stating my belief, that the proportion must in Niddry have been most trifling; difficulty being often experienced in preventing the huts of the sick from becoming over-crowded by persons drawn thither by a morbid curiosity, while numerous volunteers were never wanting from more praiseworthy motives, to assist in the necessary duties of tending those smitten with the malady.

On three different occasions, I allowed the healthy to occupy the same bed

with the sick, and in none of these instances did any untoward symptom arise, and I am aware that the same course was on several occasions pursued during the prevalence of the disease in this place (Portobello) with a like result.

I have enumerated only those cases which were distinctly marked, and of whose nature there was no doubt. Many cases of diarrhœa occurred in the month during which the epidemic prevailed in Niddry, but these were generally quite amenable to the usual treatment. In the cases which I have enumerated above, the invasion of the disease was invariably sudden, the worst features of the malady supervening with great rapidity. The remedies which I principally relied on, were calomel in large and repeated doses, opiates in small quantities, and stimuli. I found naphtha useful in checking vomiting, and the stimulating bath was of service in children.

The epidemic prevailed in Niddry for the period of one month, and its subsidence and disappearance were nearly simultaneous with the carrying out of various sanitary measures, such as the improvement of drainage, and the promotion of cleanliness.

Carrington, Mid-Lothian—(*From Mr Robert Edwards, of Lasswade*).—Several cases of cholera had occurred in the house of a farmer at Straiton (a village near Loanhead, where the epidemic was remarkably severe), and on the 7th January one of his servants (Janet Inglis) had premonitory symptoms of the disease. On the 8th she went to reside with her father and stepmother at Carrington, where at that time cholera had not shown itself. Immediately on her arrival she had acute symptoms of cholera, but eventually recovered. On the 14th, her stepmother, who had nursed her, took cholera in the same house at Carrington, and died after sixteen hours illness. Mrs Lyall, who attended the stepmother of Janet Inglis while ill, took cholera on the 14th, and died on the 19th. On the 18th, Thomas Law, who had visited Mrs Lyall's house during her illness, took cholera, and died on the 23d. On the evening of the 18th, Charles M'Innes, a lodger with Thomas Law, on returning to his lodgings, took cholera, and eventually recovered. On the 20th, Thomas Law's wife took cholera, and died at noon on the 21st; and Robert Inglis, father of the first patient, on the 20th took cholera, and recovered. These were all the cases in Carrington up to the 1st February—[The date of the letter.]

Carrington is situated in a healthy locality, and is a remarkably clean village. The dwellings are not crowded, every cottage having two apartments. The inhabitants are temperate. Inglis, Law, and M'Innes, were all labourers, and were neither poor nor ill fed. No circumstance in their habits nor in the locality of Carrington were, in my opinion, likely to predispose them to cholera—excepting Mrs Inglis, who was not by any means a robust person; Mrs Lyall, who had for a considerable time past been subject to attacks of diarrhœa; and Thomas Law and his wife, who were both aged and infirm.

Three houses only were visited by cholera in Carrington. The houses of Inglis and Lyall were separated merely by an entry of a few feet. Thomas Law's house, about two hundred yards distant.

Charles M'Innes, upon seeing Law unwell, upon his returning to his lodgings, grew frightened, and fled the house; but, feeling himself growing ill in his new habitation, he was compelled to return to his lodgings, and lay up. No symptoms of cholera whatever have made their appearance in the house where he took premonitory symptoms up to this date.

Riccarton—(*From Dr Paxton of Kilmarnock*).—Riccarton is a village of a thousand inhabitants, to the south of Kilmarnock, and separated from it by the river Irvine. We may divide the village into three portions. In the first, the houses are built on a patch of alluvial land, little above the ordinary level of the river, liable to be flooded by it, and which was so just before the breaking out of the disease. The houses here are nearly all of two stories, much sub-

divided, and densely inhabited by a rather poor population of labourers, and of (of late) unemployed calico-printers, also a few coal-miners. The second is considerably above the first portion; it is built on a rising ground quite out of the reach of inundations, and surrounds an open space, in the centre of which stands a crowded church-yard, a good deal higher than the floors of the surrounding houses. These are all old and crammed by a very poor population, chiefly Irish, a good many of whom are on the poor's roll.

The third portion of the village is formed by the houses built on each side of a road lately opened to Ayr, nearly parallel to, and within thirty or forty yards of the old village. The houses are comfortable cottages, generally inhabited by one, and in no case by more than two families. The people are of a superior class, being partly the better class of artizans, and partly miners belonging to the coal works of Mr Cuninhame of Caprington Castle, and all picked men, well attended to both morally and physically.

In the first two portions of the village the drainage is very bad, the sewers being merely open drains filled with almost stagnant waters, &c., and there are dung-pits at the back of every house; cleanliness, unless enforced, is very little attended to. In the third portion, the people are cleanly, and the roadway being cut and levelled a foot or two below the natural surface of the ground (over which the houses are built), ensures dryness and drainage, although still not very perfect; the dung-pits, &c., are almost all back from the houses at the extremity of the gardens.

The supply of water over the whole village is scanty, being taken from open wells, or from the river.

The first case occurred on the 20th January, on the alluvial flat in the first division of the village, in a house inhabited by an unemployed printer, the patient being a child of his who died next day. The house had been flooded to the depth of a foot a few days before that time. Only one other case occurred in this man's house, an older child who recovered. The second case was an old woman behind the church-yard, in the second division of the village, who also died; she had no communication with any one out of the village. The third case was an Irish labourer, who was at his work when seized; his lodging was in the neighbourhood of the church-yard, though not immediately adjoining it, and in one of the worst houses in the village, a horse and pigs having been kept *in* the house, and the dung-pits at the back of it were in a shocking state. A number more of this man's family became affected with the disease.

The cholera went on after this spreading rapidly on the alluvial flat, and around the church-yard, infesting the same houses, and selecting the same families and even individuals, as had been affected with typhus fever, in an epidemic which prevailed very extensively in the village in the summer of 1847. In the third locality, the new portion, only a single case occurred, and the man from previous bad health was in poor circumstances, and thus, perhaps from both causes, predisposed to the disease—he recovered.

The evidence here seems rather against contagion, as none of the medical attendants were seized; the assistants also all escaped with the exception of a nurse, but as she was taken from a lodging in one of the worst localities, this ought to go for nothing. I am also aware of three families in the village who had relatives in Hurlford, where the disease prevailed in a very severe form. None of those who merely went and came to see their relations were affected, nor did they communicate the disease to their families, but one woman who went and resided there, to wait on a son, took it and died.

We have found in this village that families who have been better circumstanced as to means, and cleanliness, &c., have enjoyed a remarkable immunity from the disease. The coal-miners here, who as was remarked above, are a very superior class of men in every respect to those of any locality I am acquainted with, have suffered very little, and not at all, except where their houses have been in the bad localities; thus forming a remarkable exception

to what has taken place elsewhere in this district, where the mining population has in general suffered *most severely*, as at Hurlford, which is only a mile and a half distant.

Cholera prevailed in Hurlford some time before it visited Riccarton; also in Galston, New Milns, and Darvel villages, respectively one and a half, five, seven, and nine miles distant, and all higher up on the banks of the Irvine, the same stream which flows past Riccarton; there had also been some scattered cases in Kilmarnock, previously to the 20th January.

Since the first case on the 20th January up to this date, there have been in all 146 cases. Cured 114, died 26, remaining under treatment 6 cases.

(*To be continued in next Number.*)

CHOLERA IN PARIS.

Within the last few days, there has been a slight and gradual diminution in the number of new cases. The following are the returns from the different hospitals on the 21st April:—Total, 2335; deaths, 1317; dismissed cured, 175. The different hospitals have suffered very unequally. In the Salpêtriére alone there have been 790 cases and 563 deaths. As yet no cases have occurred at the Hôpital du Midi (for the reception of venereal cases), and it is curious that, in 1832, this establishment enjoyed a similar immunity from the disease. At the Val-de-Grâce, there has not been a case of cholera among the venereal patients, and at the Lourcine (for the treatment of venereal disease in females), there have been only a few cases. Of the very numerous private patients of MM. Ricord and Vidal (de Cassis), not one has been attacked with cholera. Is the venereal poison an antidote to that of cholera? or is this exemption to be ascribed to the exhibition of mercury, to which, in Paris, almost all these cases are submitted? The French medical journals are almost completely filled with memoirs and discussions on the nature and treatment of cholera. Let us hope that their present experience will afford some real additions to our knowledge of the disease.

DEATH OF PROFESSOR BLANDIN.

We have to record the death of M. Blandin, at the age of fifty, which took place on the 17th inst. The immediate cause of death was an attack of cholerine, but he had been suffering for some months from chronic disease, the nature of which is not mentioned. M. Blandin was born at Aubigny, department of the Cher, in December 1798, where his father held a government appointment. At the age of seventeen he proceeded to Paris to commence the study of medicine. He had no friends, and his means were very small. But in Paris, with its excellent concours, these are not insuperable obstacles to the advancement of talent. From 1819 to 1841, amidst many defeats, M. Blandin was appointed successively Interne Prosector of the Faculty of Medicine, Hospital Surgeon, Professor Agrégé, and lastly, Professor of Operative Surgery, in which chair he was a worthy successor to Richérand and Dupuytren. He was ardently devoted to his profession, and his untiring industry ceased only with his life. The concours has been accused of so exhausting those who appear in its arena, that the moment success crowns their labours, they cease to work; but the life of Professor Blandin, as well as that of most of the professors in the French schools of medicine, is a sufficient refutation of this statement.

As an author, M. Blandin is most favourably known by his memoirs on phlebitis, angeioleucitis, partial amputation of the foot, extirpation of the metacarpal and metatarsal bones, and on autoplasty, of which he was a most zealous advocate. He also contributed numerous articles, in different departments of surgery, to the Dictionnaire de Médecine; and was the author of several works on descriptive and surgical anatomy. In his death, the Parisian school of medicine has sustained a loss, which it will be difficult to supply.

MEDICAL FEES IN SARDINIA.

The fees of physicians and surgeons in this kingdom are fixed by a tariff of the 28th November 1841, at the following:—A simple visit is 9d., increasing according to the time of day or night, distance, and length of visit, to about 8s. In surgery the fees vary according to the degree of the surgeon, as well as the time, distance, and operation, from 6d. to 8s.; and in the Bassa Chirurgia degree (the phlebotomists and dentists) the extent of whose occupations are defined by law, petty distinctions in the fees are actually made between bleeding in the arm, hand, or foot, the prices being 2¼d., 3d., and 4½d. respectively; and it also costs 2¼d. to have a tooth extracted, and 4½d. to have a root or fang of it removed, according to the imperial laws of the King of Sardinia.—*Medical Times.*

CONSUMPTION IN PAISLEY.

Mr Lorimer, the town chamberlain of Paisley, has, for several years, drawn up and published the bills of mortality for that town. From the bill for 1848, it appears that the mortality in Paisley during that year amounted to 1630; 770 being males, 766 females; 43 sex not stated, and 78 still-born. The mortality during years 1845, 46, and 47, was respectively 1268, 1522, and 2167. The deaths during 1848 are, therefore, within a fraction of 33 per cent. below the deaths during 1847. Consumption proved the most fatal of all diseases in Paisley, no fewer than 296 having died of that disease during 1848. In 1847, the numbers were as high as 386. The deaths from consumption, therefore, formed 19 per cent. of the total mortality in 1848, and 18 per cent. in 1847. The peculiar habits, but especially the nature of the occupation of a large proportion of the inhabitants of Paisley—a manufacturing population, confined to the close air of cotton and other mills and manufactories—is no doubt the cause of this unusual proportion of deaths from consumption.

MEDICAL STUDENTS IN THE UNITED STATES.

From a report of the statistics of the universities and colleges of the United States for the session 1847-8, it appears that the number of students at present undergoing their studies in these establishments amount to 4033, the number of graduates to 1096, and these from thirty-seven universities and colleges.

FEES FROM INSURANCE COMPANIES.

The Manchester Medico-Ethical Association has been attempting to procure the payment of fees to the medical referees of applicants for life assurance. They proposed half a guinea for policies under L.500, and a guinea for those at and above L.500. After a protracted correspondence, fifteen offices have consented to pay; forty-one will not pay; and sixty-two did not reply to the circular. The names of the fifteen offices are given—they do not include one Scotch company.

Books Received, next month.

We have encroached on the space of the "Retrospect" for this month, to afford room for the Cholera Reports.

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*Case of Cystic Oxide Vesical Calculus; Removed by Operation.* By James Miller, F.R.S.E., Surgeon in Ordinary to the Queen for Scotland; Professor of Surgery in the University of Edinburgh; Senior Surgeon to the Royal Infirmary, &c.

The cystic oxide calculus, vesical, and removed by operation, is rare; and the following example may, therefore, not improperly occupy a small space of the Journal. The narrative is drawn up by my active and intelligent clerk, Mr Vanderbyl.

"Charles Irving, æt. 52, teacher, admitted February 6, 1849. When fourteen years of age, in riding, his perineum received a blow from the shoulder of a horse. The first urine he passed after this accident was very bloody, but what came subsequently was quite clear. After a few days' rest, he perfectly recovered; suffering no further complaint till 1824, or ten years after this, when for four days he was almost unable to pass water at all. During three or four days he took some acid drops; and on the fourth day after this partial retention had commenced, he passed a calculus, the size of a hazel nut, oval-shaped, and very rough. It was followed by ten or twelve more, about the size of peas. There was no pain, or other symptom of stone, before this retention; and he continued perfectly well afterwards till 1832, when severe symptoms of stone made their appearance. He was especially troubled with pain in the groin, and at the point of the penis, after making water, which at this time was very high coloured. He was treated medically for nearly two years; but as the symptoms did not abate, he placed himself under the late Mr Liston, who, after sounding him, performed the lateral operation for removal of the stone, on the 24th of January 1834. The patient says he remembers perfectly the size of the stone; which, according to his statement, was egg-shaped, more than three inches in length, and weighed five ounces.

"The operation was quite successful; the urine came by the natural passage on the 12th day; and the patient continued well, though occasionally troubled

with pains in the legs. These pains increased to a very troublesome extent; and in 1836 (two years after the operation) he wrote to Mr Liston about them; receiving the following answer:—

"'12, Old Burlington Street, London,
April 20, 1836.

"'Sir,—I don't think there is much chance of your having another stone, if you have been taking any sort of care of your stomach. If there is really occasion to think so, you must try to come and put yourself under my care here. I can take charge of you in the North London Hospital (we don't call it an infirmary here, and your letter found me with difficulty) where you will be as comfortable * * * * as in the Edinburgh one. Write me and let me know whether there is really a stone. Get your friend, Mr ———, to sound you; and let me know the result. Direct to me as above.—I am, yours truly,

"'Robt. Liston.'

"'P.S.—I think the stone I extracted was large and very rough. Any other, if *there* now, must have been formed in the kidney since, and come down.'

"Under medical treatment, without sounding, it would seem, the pains at length disappeared (1840), and he again enjoyed good health till 1843, when a cart, which was upset, fell on him. His body was bent down between his legs, and he lay insensible for some minntes. It was said that he had fractured his spine. He was admitted into the Dumfries Infirmary; where he was bled and leeched; and here he remained for ten weeks. The lower half of his body, though not insensible, was nearly paralysed; the urine was passed with pain and great difficulty; and after leaving the hospital he continued gradually to lose the power of his legs. At the end of two months, two setons were introduced, one on each side of the spine, opposite the injury. A few weeks after this he could walk better. The setons remained for two years and three months.

"About eighteen months after the accident, symptoms of stone re-appeared; always becoming aggravated in the winter months; and last December they became so severe as to induce him to come to Edinburgh for the purpose of having the stone removed.

"His digestive organs are generally good; his diet always plain; his bowels are usually loose. He has seldom observed any deposit in the urine, except shortly after the injury of the spine, when the urine became very thick, and presented an ammoniacal odour. On admission, he complained of all the ordinary symptoms of stone, the existence of which was discovered by the sound.

"February 14th.—He has had an occasional dose of castor oil since admission, and is in good spirits. To-day, at noon, Professor Miller performed the lateral operation for removal of the stone, cutting in the cicatrix of Mr Liston's incision. The operation was accomplished in forty seconds, the patient being under the influence of chloroform.

"February 15th.—The urine passed freely through the tube last night. Removed a large clot from wound. Patient feels quite comfortable. Pulse natural.

"February 16th *vesp.*—Tube removed—patient doing well.

"February 27th.—Patient has continued very well since last report, and urine came by the natural passage to-day. It is somewhat muddy, and deposits a considerable quantity of mucus on standing.

"March 15th.—He has improved gradually—has been out of bed several times, and to-day had a walk on the green—(one month exactly since the performance of the operation).

"*Observations on the Urine.*—Two days before the operation, the urine was of a light colour, but very muddy, and deposited a large quantity of mucus after standing awhile. It was slightly coagulable; and, when placed under the microscope, showed numerous pus corpuscles, with a few epithelial cells and nuclei, and some crystals of triple phosphate.

"On the 12th day after the operation, the urine was for the first time passed by the natural passage. On examination, it was found to contain numerous small crystals of cystic oxide, which sometimes occurred in masses. A few larger ones were also scattered here and there. The urine was thick and muddy, though very pale-coloured. It was slightly coagulable by heat and nitric acid. Odour not unpleasant; specific gravity 1013—distinctly acid reaction.

"Since then I have examined the urine twenty times. It is always very light coloured, though thick and forming a considerable deposit on standing. It is always distinctly acid—average sp. gr. 1012. Heat causes a very slight cloudiness; but this is removed by nitric acid, which developes a beautiful pink colour, which becomes almost purple on adding more acid, but is changed to amber colour when the acid is added in great excess.

"About a month after the operation, I was unable to detect any cystine in the urine; and at present only a few epithelial cells and nuclei, with some pus corpuscles, are observable. There is still a slight deposit on standing. No film was ever observed on the surface of the urine.

"*The calculus* is of a flattened ovoid form,—measures 1 and 2-5ths of an inch in its long diameter, and weighs three drachms and fifteen grains. It is covered by a loose pinkish white coating (of triple phosphate), about one line thick;—which thickness, though pretty uniform, makes the surface of the calculus less rough than that beneath the coating. The calculus itself is peculiarly rough; the elevations and depressions are small, yet vary much in size. It is very hard, and of a greenish yellow colour.

"On being divided, it exhibited a central ovoid nucleus—about one-fourth the size of the calculus, and somewhat more yellow than the surrounding portion. The nucleus was very dense at its centre and circumference, but contained some irregular excavations in its substance.

"The surrounding portion of the calculus was densest around the nucleus, and appeared to consist of an aggregation of small grains; which, on moving the calculus about, reflected the light, like the shining surface of minute crystals.

"A few days after the operation, by the kindness of Dr Maclagan, I was enabled to ascertain chemically, as well as by the microscope, the true nature of the calculus. The cystic oxide crystals were observed in beautiful hexagonal plates, which were sometimes aggregated together; and a small chip of the stone presented all the chemical characters ascribed to cystic oxide."

One or two observations suggest themselves.

1. The operation was repeated on the same side of the perineum. On this point authorities differ. Some advise cutting in a fresh place, on the patient's right perineum. Among these was the late Mr Liston. When called upon to cut a patient who had been cut before, he preferred a new wound, unless when the old remained fistulous. I am inclined to think, however, that a consciousness of his own dexterity, or rather of his *ambidexterity*, had more influence than he himself was aware of in this matter. He liked operating with his left hand, when occasion served. In this case, Mr Syme, in consultation, expressed his preference for repetition of the original wound; and, in consequence, I had no hesitation in following my own inclination. The parts were found stiff and rigid, of course; dilating but imperfectly before the finger, and requiring a freer use of the knife's edge than ordinarily. But there was, at the time, a peculiar satisfaction in following the plain track of so great a master in the art; and, as regarded the future, there arose a comfortable anticipation, that from the main danger of the operation—infiltration of urine—the patient would, in

a great degree, be exempted, by reason of the dense structure of the parts incised.

2. "The operation was accomplished in forty seconds." I have kept this statement in the report, unchanged and unexpunged; in order to protest against the tendency to *time* operations, which I believe to be both injurious and absurd. Absurd; because it requires no argument to show how erroneous must be the computation of any operation's excellency by the mere number of minutes consumed in its performance; it is not a race, nor any match against time; but a deliberate, cautious, contemplative procedure,—each step of it apt to evolve something new, which in its turn demands consideration and resource. Injurious; because the practice of "timing" may throw a surgeon off his propriety, by urging him to a greater haste (in public practice, at least), than what is consistent with prudence and safety. Rapidity and excellence of execution differ greatly from each other in all performances, and in none more widely than in those of surgery. The only legitimate ground of excuse for hurry was the desire to save the patient from unnecessary pain by unnecessary protraction. But, by anæsthesia, that plea is wholly removed. The patient suffers nothing. On the score of pain, the surgeon is free to adopt the utmost deliberation and care in the procedure; and he is much to blame, if he fail to do so. All things going smoothly, the operation may be safely and slowly over within a couple of minutes. But if any doubt or difficulty arise, the question of time should surely never be allowed to enter as an element into the case at all.

3. When did the stone grow? Was the bladder free from foreign incumbency during the interval between the first operation, and the invasion of "symptoms" eighteen months after the injury of the spine? Or was the cystic oxide formation begun at the time the patient consulted Mr Liston, by letter, in 1836; grown to a considerably sized stone at the time of the spinal fracture; and, after that, incrusted by deposit of phosphates—such tendency being the well-known ordinary result of such injuries? I incline to the latter supposition; but of course it is impossible to arrive at any accuracy in the matter.

4. It would be interesting to know the chemical nature of the former calculus. It, I presume, is in the collection bequeathed by Mr Liston to University College, London; and, perhaps, if the eye of any gentleman connected with that institution light on this notice, an elucidation of this point may be kindly undertaken.

5. What are the best means of preventing return? Authorities differ as to the mode of treating the diathesis. Some advise antiphlogistics and alkalies; others, acids and roborants. In this patient, cystine was found in the urine for about a month after the operation; afterwards it disappeared. The treatment followed was tonic regimen, with small doses of iron.

6. The alternation of the diathesis. Dr Willis states, "the specimens of cystic oxide examined have all been remarkably pure; I

think only two instances are on record in which the cystic diathesis appears to have been followed by any other, and in both of these instances this was the lithic."[1] Here it was the phosphatic; but the determination of this was, no doubt, as already stated, the intercurrent injury of the spine.

7. This is the only patient I have yet found entertaining a dislike to chloroform, after having made trial of it in a serious surgical operation. He had been cut both ways; without and with chloroform; painfully conscious of all the steps of the operation, and happily unconscious of everything. Were he to be cut a third time, he said he should do as at first; have his wits about him. But I am inclined to think this was very much "*une façon de parler;*" and that were the third emergency to arise, mature deliberation would give its voice in favour of anæsthesia. He is possessed of fine courage, certainly; yet I think he would be sadly wanting in discretion, were he to encounter the pains of lithotomy unassuaged, rather than incur the risk of a slight squeamishness or headache, by the use of that which would annul suffering quite—and, as experience shows, safely too.

ARTICLE II.—*Cases of Vascular Tumour of the Urethra, with Remarks.* By H. BURFORD NORMAN, Fellow of the Royal College of Surgeons, Surgeon to the St Marylebone General Dispensary, and the Western Ophthalmic Institution.

THE object of the following communication is to give in a connected form a full account of a class of cases by no means unimportant or uninteresting, and of which individual examples have probably come under the observation of most experienced practitioners. To furnish this I venture not to depend on my own observations alone, but avail myself of the recorded experience and observations of many others. The inquiry into this subject which I instituted for my own information I have felt to be both interesting and profitable, and it is my desire that the result as here evolved may be so to others also.

That morbid growths, answering to the title of "Vascular Tumours of the Urethra," have long been known, without referring to an earlier date, we may learn from the two following quotations from eminent London surgeons of the last century.

Thus Mr Sharp wrote[2]:—"That small excrescences may occasion violent disorders in so tender an organ as the urethra, I have seen a notable instance in the urethra of a virgin, where they grew in small quantity upon the orifice of the meatus urinarius, and for many

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[1] Willis on Urinary Diseases, p. 270.

[2] Critical Inquiry into the Present State of Surgery. By Samuel Sharp, F.R.S. London, 1750. p. 168.

months had produced most excruciating torment, which continued till I had totally extirpated them."

And Mr Broomfield[1] recorded his acquaintance with the disease in the following words :—"I make no doubt that many professors of surgery have seen as well as myself fungi of a considerable length from the orifice of the urethra both *in men* and women. I have known several instances in the female sex whose cases have been supposed to be nephritic, which on examination of the parts affected have been found to be owing to those fungous excrescences of the urinary canal, and have yielded to the application of the bougie, though they had returned frequently after they had been taken off with scissors."

Though these authors have omitted to give any particular description of the growths that they had met with, there is enough to be gathered from their short statements of the general features and symptoms of the malady to enable us to draw a fair inference that they were the same productions as were at a much later period described by Sir Charles Clarke as "The Vascular Tumour of the Orifice of the Meatus Urinarius."[2] To Sir Charles Clarke belongs, however, so far as I know, the merit of giving to these growths a "name," and to the profession a good description of their ordinary characters and symptoms.

"The tumour," he says, "is *seldom firm;* of a florid scarlet colour; if violence be offered to it, blood of the same colour is effused; it is somewhat *granulated,* exquisitely tender; appears on accurate examination to shoot from the inside of the urethra; seldom acquires a large size; its attachment is slight, and it is so moveable as to appear almost like a detached body lying upon the part. It gives rise to great suffering during micturition, and to a vaginal discharge. It is common to the married and the unmarried; most frequent in the young." The tumour was never seen by Sir Charles Clarke "in a person above the middle age." He notices that a similar kind of disease existed occasionally "in the interior of the canal, giving rise to the symptoms of tenderness on pressing the urethra, and a general swelling of the part;" and refers to such a case successfully treated by Sir James Earle by means of large bougies.

In most systematic works on diseases of the genito-urinary organs, and especially in those which treat of the female organs, these tumours are described; and in those published since Sir Charles Clarke's work, the name given by that author is adopted.

Thus Burns says:[3]—"Excrescences may, notwithstanding the opinion of Morgagni, form in the course or about the orifice of the

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[1] Broomfield's Chirurgical Observations, &c. Vol II., p. 296.

[2] Diseases of Women, &c. By Sir Charles Clarke. Third edition, p. 303.

[3] Principles of Midwifery, including Diseases of Women and Children. By John Burns. Sect. 23, p. 70. Fourth edition.

urethra, and generally produce great pain, especially in making water, on which account the disease has been sometimes mistaken for a calculous affection. The agony is at times so great as to excite convulsions; and it is not uncommon for the patient to have an increase of her sufferings about the menstrual period. The tumour is vascular, florid, moveable, and exceedingly tender. When excrescences grow about the orifice of the urethra, they are readily discovered; but, when they grow high up, it is much more difficult to ascertain their existence." Dr Baillie[1] says they "cannot be known but by the sensation of a catheter passing over a soft body." Burns refers to many interesting cases, here-in-after to be cited. The late Dr David Davis makes mention of these tumours, and gives a most interesting and complicated case in illustration (see Case 9). Sir B. Brodie[2] limits their size to that "of a pea, or horse-bean." He has met with many instances of them.

Dr Ashwell[3] thus treats the subject:—"This is by no means an unfrequent affection of the mucous membrane of the urethra. It usually attacks the young, whether married or single; is very rarely seen after the cessation of the menses; consists of a small *granular* tumour, generally pediculated and moveable, attached to one part of the margin of the meatus, or just within the urethra, *whose two prominent characteristics are great vascularity and exquisite sensibility.* Sometimes these growths are not moveable, but appear like several raised coalesced granulations, or occasionally two or more isolated independent ones may be seen near together. Every now and then they extend partially along the urethra, and may even be located at the neck of the bladder. They are covered by a delicate *pale*[4] mucous membrane, which is broken on being touched during examination, and blood exudes from the soft, feeble, and injected capillaries. . . . . If they interrupt or come in contact with the stream of urine, the act of passing water causes intense pain, and this is usually the first and most constant symptom of the complaint. After a time the urethra and bladder become irritable, and the frequent desire to micturate is suppressed by the dread of the pain which it excites. Sexual intercourse is intensely painful, and is therefore obliged to be suspended. Walking exercise is attended with suffering, and in fact any movement of the vulva, by slightly displacing or pressing upon the growth, produces instant pain." Leucorrhœa, and various sympathetic pains of the pelvis, loins, &c., are mentioned as accompanying the tumour.

Mr Safford Lee[5] has met with these tumours "in the young and

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[1] Morbid Anatomy. P. 321.

[2] Lectures on Diseases of the Urinary Organs. Third edition, p. 92.

[3] Ashwell on the Diseases of Women. Second edition, p. 710.

[4] Dr Ashwell, in this description of the *covering* of the tumour, differs from all others, who speak of it as "florid" in appearance.

[5] Tumours of the Uterus and its Appendages.

old." The largest example he had seen (as large as a date) was in a woman above forty; and he had one under treatment much older.

He notices the occasional participation of the nymphæ in similar disease, its occasional extension along the urethra to the bladder, and also that the tumour is frequently divided at its extremity. Amongst the symptoms to which the tumour gives rise, he says, "One patient I saw had pains down the inner side of the thighs to the knees; and Professor Simpson told me of one case where pains in the soles of the feet were complained of."

Boivin and Dugés[1] describe them as "fungoid tumours," occasioning great inconvenience in micturition—formed on the meatus, the borders of which orifice concur irregularly to the formation, rounded, though often granulous, varying in size from that of a pea to a cherry. They refer also to the experience of Madame Lachapelle, and of Cullerier and Dubois in similar cases. In the Atlas to their work (Pl. XL., figs. 3 and 4), are representations of these morbid structures.

It is evident, from the general, and, still more so, from the particular descriptions of these growths given by the authors already cited, that the "excrescence is in general granular or tuberculated on its surface, and possesses considerable firmness." This is not, however, uniformly its character; it is sometimes *perfectly smooth*, and of great delicacy in structure, as the following cases, which I have met with, will show. Occasionally, I am informed, they scarcely exceed the solidity of jelly; but I have never seen them so fragile as this. The following are instances of the smooth variety:—

Case I.—Mrs L., aged 52, married, the mother of a numerous family, short, slight, and of fair complexion, an infant-school mistress, and in a state of good health, came under my care in September 1845. For 15 months previously, she had had "something growing near the water passage." She first became aware of its existence from the pain she experienced in intercourse with her husband, and which has increased so much as to compel entire abstinence therefrom for sometime past. Since she became subject to these pains, she has also had a vaginal discharge. She has been under the care of two medical men. By the advice of the first, who had not inspected the parts, nitrate of silver was regularly applied to the growth, occasioning such excessive pain that she was obliged to leave it off, without experiencing any benefit. The second, an eminent obstetric physician, made an ocular examination, and prescribed a lotion which was equally inefficient. When I saw her, she could neither walk, stand, or sit in comfort from the pain, which was much augmented every time she made water. I examined the vulva, and found, growing from the lining of the urethra, the orifice of which it occupied, tilting at the same time under its edge into the vagina, an excrescence about half an inch long, *perfectly smooth*, of a ripe red cherry colour, and *very delicate* texture, pediculated and kidney-shaped, the pedicle being attached a very little within the orifice. It was so sensitive that the slightest touch gave acute and excruciating pain. There was also a somewhat abundant muco-purulent vaginal discharge. After a week's delay in consequence of the fears of the patient, I applied a ligature around the excres-

[1] Diseases of the Uterus and its Appendages. By Boivin and Dugés. Translated by Hemming. p. 542.

cence, in the following manner: the patient being placed in a favourable position, and the labia and nymphæ separated by an assistant, the excrescence was drawn forwards by means of a pair of artery forceps, so as clearly to see the attachment of the pedicle; through the base of the latter and a portion of the sound membrane around it, a needle, armed with silk, was carried; the needle was then cut off and the four ends of the silk were tied, two and two, around the base of the pedicle, involving in the noose a portion of nearly the whole thickness of the urethra. The ligature was applied on the 23d of September; on the 25th it was thrown off with the shrivelled excrescence. The part appeared likely to heal well. The patient was free from pain, and had been comfortable and able to pass her water easily from the time of the operation.

December 4.—Ten weeks after this she again applied to me suffering as before the operation, but less severely. A little granulation-like excrescence had grown up in the old spot, and occasioned the inconvenience. I then applied a drop of nitric acid. This gave severe pain, but appeared completely to have destroyed the growth. The patient soon became easy, and the part healed.

In December 1846, a year after the application of the acid, and fifteen months after the ligature, I heard from the patient that she continued free from her malady.[1]

Case II.—S. B., a poor married woman, æt. 56, enjoying excellent health, especially the last few years, since "the change of life," consulted me, December 10th, 1846. For four months previously she had been suffering excessive pain in micturition, and almost incessant desire to make water, the dread of which was at times so severe as to cause a retention of the urine. She has sometimes fallen on her knees in the street in very agony when the desire has come on. These symptoms have been accompanied for the same period of time by severe pain in the loins, thighs, and lower part of the belly. When water is passed, she describes it "as cutting her through," Walking and sitting are both painful; she is only easy in the supine position with the thighs separated.

On examination, I perceived in the orifice of the urethra, a small purplish red body, apparently very vascular, of very delicate texture, and perfectly smooth surface—it filled the orifice of the urethra, and projected up towards the pubis. To the touch it was excessively tender. Its figure was that of a young kidney bean—its external border, convex, was about three-fourths of an inch long; its internal one, concave, was attached by a narrow pedicle to the lining membrane of the urethra a little within the orifice. There was some increase of the vaginal secretion.

Next day I applied a ligature to this tumour in the same way as in the preceding case. In three days the excrescence and ligature came away. The orifice of the urethra then appeared generally in an over-vascular condition. I feared its becoming involved in similar disease, and applied the stick of nitrate of silver freely within the canal. On two subsequent occasions I applied caustics. On the first potassa fusa, and on the second the pernitrate of mercury, not feeling sure that the morbid state was quite got rid of. Subsequently to this, as soon as the parts would bear it, bougies were passed every other day for a month. From the time of the application of the ligature she never suffered any pain of consequence, and made water easily. A short time after applying the caustics there was a little scalding felt in micturition, but nothing more.

Nine months after, the patient called on me to say that she had been entirely cured.

[1] In reading a paper on this subject recently before the Harveian Society, I learned that this patient had suffered again from reproduction of the excrescence, and had had it twice removed by other practitioners. I am indebted to the gentlemen who took part in the discussion for much information on these tumours, which is embodied in this essay.

CASE III.—Occurred in the person of a young woman, a prostitute, who was under my care for primary syphilis and vaginal discharge. The excrescence was very like the last, but *far less sensitive*. I did not think it right to interfere with it, whilst the syphilitic sores existed, for fear of inoculating a fresh place, and before they were quite cured. I lost sight of the patient, and consequently the opportunity of doing anything to the tumour.

CASE IV.—I was called (November 7th, 1848) to see Mrs C., living near the Middlesex Hospital. She was said to be suffering from sickness and vomiting, which had lasted two days, and was connected with a hernia. On visiting her, I found her suffering from bilious vomiting and constipation, the latter being almost an habitual state—her tongue was furred, and her pulse a little quickened. Her belly not at all tender or distended. She had been the subject of femoral hernia for many years, but had always kept the bowel in place by a truss—it was not at this time protruding. She was a nervous excitable person, fifty-five years of age, and a widow. She had had several children, but had ceased to menstruate some years. She complained of great pain in making water. It had lasted two years, and was equally bad whatever the state of her urine. She had never passed any blood. Walking occasioned severe pain in the vulva, in consequence of which she had confined herself to the house for many months previously. She had consulted two medical men, and taken various medicines, without benefit; and with the hope of procuring some relief from her sufferings, had entirely given up all stimulating food and drink. She had never been examined, or had any local treatment to the painful part.

I prescribed a few grains of calomel, to be followed a few hours after by a black draught, and directed that some urine should be kept for my next visit.

The bowels having been freely opened by the medicine given, the sickness and vomiting ceased, and the tongue became clean, but the pain in making water continued. I found the water kept for me at my next visit clear, of an amber colour, and quite free from deposit. On examining the vulva, however, which I obtained permission to do, I found within the orifice of the urethra a small bright scarlet body, of very delicate texture, highly vascular, and perfectly smooth, not much larger than a barley corn, attached to the lining of the under surface of the canal, from which, by pressing back its borders, the excrescence could scarcely be made to protrude. It was scarcely pediculated, but grew up like a large granulation. The borders of the orifice of the meatus had a similar florid appearance. The parts were excessively tender to the touch. There was little or no increase of the vaginal secretion.

A ligature was applied to the excrescence as in the preceding cases, but it was necessary to dilate the urethra, in order to expose the growth and enable me to transfix its base. This was effected by passing a pair of forceps into the canal, and separating the blades. After the ligature was secured, the borders of the orifice were rubbed with potassa fusa; a sponge full of vinegar being used immediately to prevent the caustic from spreading.

The urine was passed involuntarily that day and part of the next; and the evening succeeding the operation it was necessary to give a dose of laudanum, in consequence of the severe pain arising from the irritated parts.

The excrescence and ligature were thrown off in a few days; a considerable puriform discharge from the urethra and vagina was excited by the operation, but it readily yielded to the application of the goulard lotion and cold ablutions. She is now perfectly free from the complaint; walking and passing her water quite easily.

It is rather remarkable that of the four preceding cases, three should have occurred in patients of advanced years, who had ceased to menstruate. Since writing the above, I have been consulted by my friend, Mr Preston, on another.

Case V.—In this case, the patient, a married female, forty years of age, who has had one child, had for four years suffered so severely in sexual intercourse, that it was almost intolerable to her; and the disagreement between herself and husband arising out of the circumstance, had nearly occasioned a separation between them. On inspecting the vulva, a small florid excrescence, similar to those previously described, was seen within the orifice. It was excessively tender, but what is remarkable, *it had never occasioned pain in micturition.* There was some increase of the vaginal secretion. Mr Preston removed the excrescence by twisting it off at its root, and then applied the potassa fusa.

This excrescence has been the subject of a microscopic examination by my friend Mr Quekett, whose description of it will be given in another part of this paper.

The following cases, from various sources, will present to the reader some interesting varieties of the morbid productions now under consideration.

Case VI.[1]—A little girl eleven years old, came under the care of Mr Hughes, in 1760. She had suffered for three years from severe pain in micturition, and occasional darting pains as from penknives running into the part. She was unable to take horse exercise without inconvenience, but felt no inconvenience in walking.

She had been examined previously by another medical man, and by him, being supposed to have *prolapsus uteri*, had been ordered to wear a bit of cork as a pessary. This occasioned intolerable pain, so that it was obliged to be left off, and a bolster of linen was substituted.

On examination of the parts, a red softish substance of spongy texture, irregular jagged surface, sore to the touch, and giving issue to a little bloody serum, was found growing from the whole circumference of the orifice of the urethra, and protruding from it about half an inch. The urethra passed through the body of the excrescence, which was contracted at its attachment, but expanded externally. Mr Hughes removed the growth by excision. *Brisk hemorrhage* followed, but stopped of itself in a few minutes. A slight reproduction of the tumour occurred after some days, but its growth was arrested by the application of nitrate of silver.

Retention of urine followed the operation, and compelled the use of the catheter for some days. Bougies were afterwards introduced daily, and a complete and permanent cure was effected.

Case VII.—Occurred to Mr Jenner of Painswick, in Gloucestershire, in 1776.[2] M. B., aged thirty, had for fifteen years suffered from great and constant "irritation to make water." The desire returned by day every ten or fifteen minutes, and could not be put off; by night it left her scarcely an half-hour's interval for rest. As the disease came on soon after the commencement of the menstrual function, and that had been irregularly discharged, it was supposed to depend on this irregularity, and a variety of medicines were accordingly given without benefit. Four years before she applied to Mr Jenner, she had discharged one day about four ounces of blood after voiding her urine, and then her case had been pronounced a "stone in the bladder."

When Mr Jenner saw her, he found, on examination, a fungous excrescence filling up the orifice of the urethra. He immediately put a ligature around it, and drawing it forward, snipped it off with a pair of scissors at its origin, which was about a quarter of an inch within the orifice.

Four months after the operation, when the report was published, there had been no return of the disease.

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[1] Med. Facts and Observations; vol. iii, p. 26

[2] London Med. Jour.; Vol. vii., p. 160. 1776.

CASE VIII.[1]—Mr Warner relates the following interesting instance:—"A virgin, æt. twenty-seven, had suffered *from infancy* from frequent desire to make water, which she voided by drops, and with excessive pain. The pain and irritation had been frequently so great, as to cause convulsions, and to prevent her from gaining her livelihood. It was much increased at the menstrual periods, especially if the customary discharge did not appear. On dilating the urethra with a female catheter, two excrescences were discovered arising opposite each other, near its hither extremity, each as large as a silver penny-piece, and resembling the valves of a vein in their situation. Their colour was red, and their texture spongy, and they consisted *of a number of fibres*, as appeared upon examination after their removal, which was effected by dividing the urethra a little to its left side with a pair of scissors, and then, when the excrescences were exposed, snipping them off at their base. There was no immediate hemorrhage. *Afterwards it was severe*, but stopped of itself. The patient recovered without a bad symptom; and five months afterwards she was seen by Mr Warner quite free from the disease.

The same author relates the following case, in which a pediculated vascular tumour, appearing at the orifice of the urethra, was found to take its origin at the neck of the bladder.

CASE IX.[2]—A young woman strained herself by endeavouring to lift a heavy weight, and was immediately seized with pain in the small of the back, and a total suppression of urine, which lasted five days, when the catheter had to be used. During the suppression she had an acute fever, and for eighteen or twenty hours before the water was drawn off, she discharged by mouth a great quantity of saltish water tinged with blood. Three years after this, she consulted Mr Warner. From the time of the accident she had never made water without the catheter, and on several occasions its introduction had occasioned much loss of blood. Mr Warner examined the urethra and bladder with his forefinger, which he passed with difficulty through the canal. He discovered a considerable tumour, which seemed to be of fleshy consistence, and took its rise from the lower part of the bladder near its neck. Upon the patient's straining to make water when the bladder was full, the tumour protruded a little from the meatus, but on ceasing to strain it presently returned. It had maintained its present appearance from the time it was first noticed, and had been punctured eighteen months before in the expectation of its containing fluid. The patient was purged, and then the following operation was performed: The tumour being made to protrude by straining, was prevented from returning by ligatures passed through its substance by the help of a crooked needle; and an effort was made to draw it down through the orifice of the urethra. This being impossible, Mr Warner divided the urethra, by cutting it upwards about half way to the bladder, and then the tumour being pulled forward, he was enabled to pass a ligature around its base, which was *very large*. For three days after, much pain in the abdomen was felt; on the sixth day the ligature dropped off. From the day of the operation the water was voided naturally. The tumour was about the size of a turkey's egg. Recovery was complete.

CASE X.—Is recorded by the late Dr David Davis.[3] A lady, fifty years of age, of firm constitution, and the mother of several children, had suffered twenty-five years from distressing pains of the urethra, and had had an excrescence growing from the part all that time. It gave rise to dyspeptic symptoms, to incessant calls to empty the bladder, and occasionally to tremendous paroxysms of abdominal and uterine pains, but not to any morbid secretions of the vagina. On examination of the parts, the orifice of the urethra was found

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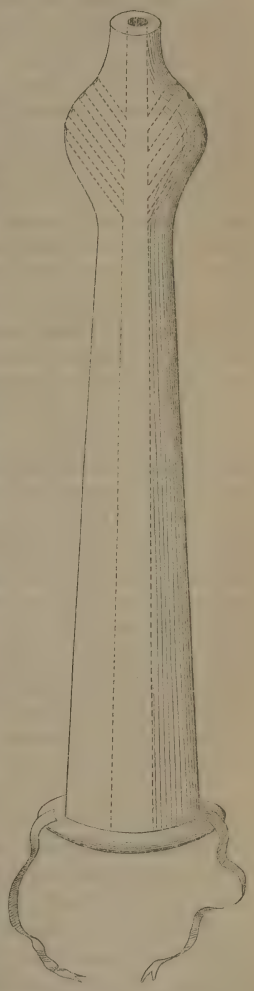
[1] Warner's Reports and Cases in Surgery, p. 309.
[2] Warner, op. cit.
[3] Obstetric Medicine. By David D. Davis, &c. Fourth edition, p. 86.

to be garnished with a bunch of fungoid excrescences, which, conglomerated together, amounted to about the size of a small raspberry—its colour that of arterial blood, not painful, nor even sensible to the touch, bleeding rapidly on pressure. The surfaces immediately contiguous were exceedingly sensitive, but free from any change of colour. The difficulty of passing a catheter into the bladder, and the sensation of its being obstructed by successive obstacles, gave rise to the suspicion of similar disease in the urethral passage. The external tumour was removed "by abscision."

Bougies were then introduced daily, of augmented size, and retained sometime in the canal; in about a month they had caused the expulsion of numerous bodies—fragments of similar character to the tumour removed, and the urine could be held and voided with greater facility than for years before. Finally, the finger passed into the dilated urethra and bladder, detected around the neck of the latter the existence of a circular ring, of structure foreign to the part, about half an inch broad; its superior limit was tolerably defined, and nearly equal at every part. The daily use of bougies was continued, but as they could not be retained constantly, in consequence of the expulsive efforts of the bladder, a hollow bougie, with a bulbed end, something like a drumstick, was invented; and this, with difficulty introduced at first, was worn for eight days constantly, except when removed twice to be cleaned. After each withdrawal of the bougie, large portions of the excrescence were expelled; and at the end of eight days, the whole mass was gone.



Dr Davis's Instrument.

From what has been now advanced, it will be seen, 1. That the urinary passage of the female is, in its whole extent from the external orifice to its termination in the bladder, liable to become the seat of excrescences; and that though the orifice is the most frequent seat of these growths, they are not very uncommon in the interior of the canal.

2. Though entitled to one common appellation, from their vascularity, they differ much in some other respects. Thus, in size, they may vary from a simple large granulation to a tumour as large as a date,[1] or, as in the case related by Mr Warner (case 9), as a turkey's egg; in consistence, from something scarcely exceeding the solidity of jelly to a considerable degree of firmness; in external surface they may be perfectly smooth or granulated; in form flattened and sessile, rounded, and growing irregularly from the whole margin of

[1] T. S. Lee, loc. citat.

the meatus, or pediculated and pendulous; sensitive to the highest degree, or as in one instance quoted (case 10), not even sensible. Their sensibility bears no relation to their size, unless it be in the inverse ratio; the smallest excrescence certainly gives equal pain and distress with the largest.

3. Little has been made out in relation to the intimate structure of these growths. Boivin and Dugés call them cellulo-vascular. Mr T. S. Lee says they "are entirely made up of vessels, and their connecting cellular tissue. Warner says one of his, which he examined, "consisted of a number of fibres," and the excrescences removed by Dr Davis, examined in water, showed a fibrous structure.[1] The following is Mr Quekett's description of the excrescence (case 5), which he has kindly examined for me. "The specimen was of an oval figure, about two lines in length in its long diameter. It was white, and had numerous small confervoid filaments attached to its outer surface, from having been some time in water. A thin slice from the external surface, when examined microscopically with a power of 200 linear, exhibited the same structure as ordinary cuticle; the epithelium of the outermost layer being composed of flattened scales, whilst the cut surface exhibited the same kind of cells, more condensed, and firmly adherent together; a vertical section through the middle of the mass showed several papillæ of various sizes, which were very vascular, and surrounded by an investment of cuticle, which, with the papillæ, made up the entire mass of growth; at the part where the papillæ were situated, the growth was smaller than at the opposite extremity, as though it had been attached by a constricted neck or pedicle. The papillæ, no doubt, were largely supplied with nerves, as well as blood-vessels, but their presence could not be detected by the microscope. The growth may be said, then, to consist of hypertrophied papillæ, invested with a thick layer of cuticle, which projected from the general surface of the mucous membrane in a wart-like form." This connection with the papillæ will serve to explain the extreme sensibility generally possessed by these tumours.

4. They are non-malignant, though, according to Boivin and Dugés, "they may doubtless become cancerous."

5. They have, for the most part, an extreme tendency to be reproduced, when cut off or otherwise removed; but this is hardly a constant character, and may generally be counteracted.

6. They are common to the married and unmarried, and to all ages from infancy to old age, but most frequent, probably, during the period of activity of the generative system.

7. They have been ascribed to syphilis, excessive venery, and masturbation, but I cannot discover any good ground for admitting

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[1] I think it very unlikely that any of these growths, from their situation, mode of growth, and general characters, possess any true *fibrous tissue* in their texture.

either of these causes, believing that they are causes only in the light of being occasionally coincident with effects.

8. The most constant symptom of the disease is pain in the urethra, greatly augmented during micturition,[1] which is sometimes suppressed by dread, and sometimes excessively frequent from the extension of the irritation to the bladder; augmented, also, by all movements of the vulva, by coition, and during the menstrual flux. A muco-purulent discharge from the vagina accompanies the growth, and there are occasionally severe sympathetic pains in the loins, pelvis, hips, and inside of thighs, and a discharge of blood leading to the idea that the case is one of calculus of the bladder. To prevent this mistake, which has been often made, whenever the more common symptoms are present, and *not accounted for by a morbid condition of the urine*,[2] careful examination of the vulva should be made before an opinion is given.

9. The external characteristics of these growths are sufficiently clear to prevent their being confounded with other swellings,—as verrucae, prolapsus of the bladder, eversion of the mucous membrane of the urethra, or varicose tumour of this part, and still more with prolapsus uteri; a mistake which, in one instance, we see has nevertheless happened—Case 6th.

10. The treatment required is strictly local, and consists of such means as entirely destroy the growths and are most likely to prevent reproduction. These are cauterization, caustics, astringent lotions, excision, ligature, and pressure by bougies.

They are all applicable in different cases. If the growth is single or double, and not attached by a very broad base, it may be removed either by excision or ligature. The former has, perhaps, the most numerous advocates, and among them Sir B. Brodie and Dr Ashwell, and was practised in several of the cases cited in this paper. To the latter I give a decided preference, along with Sir Charles Clarke, in consequence of the serious hæmorrhage that often follows excision.[3] In either case, a powerful caustic should be applied to the point from which the growth has been removed, to prevent its reproduction.

The actual cautery or caustics are only applicable primarily to very small excrescences near the orifice, and to those which answer to the description of Dr Ashwell,—*i. e.* having the appearance of several raised coalesced granulations, or two or more isolated ones; as a secondary means, they are applicable, as before mentioned, after excision or ligature. The more powerful the caustic, if proper care be used to prevent its spreading, the less suffering it produces, and the more quickly it effects its purpose. Potass, nitric acid, and per-

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[1] See, as an exception, case 5.

[2] See Appendix, p. .

[3] See cases 6 and 8, and T. S. Lee, op. cit., where a case is referred to. Mr B. Philips recently informed me, that he had lately seen very severe hæmorrhage follow the excision of one of these growths.

nitrate of mercury are the best. Nitrate of silver gives excessive pain, and is slow and inefficient.

Bougies, like caustics, have a primary and a secondary use. Primarily, they form the only efficient means of treating those growths which affect, in a general manner, the interior of the urethra, and their use is well illustrated in Dr Davis's case (Case 10); secondarily, they are, perhaps, when regularly introduced and worn for some considerable time, as useful as caustics, if not more so, in preventing reproduction of a tumour that has been removed. The pain which they occasion, in their primary use, must be met by anodyne local applications, and now-a-days anæsthetics may be brought into use for this purpose. In their use, as secondary means, they do not occasion suffering.

By one or all these means, this most distressing affection may, in general, be effectually cured; even the most formidable cases, as we have seen, may yield to bold and persevering endeavours, as illustrated in cases 9 and 10. The latter does great credit to the ingenuity and perseverance of the physician who records it, and the former to the ability with which an important operation was devised and carried out. The successful result, in both cases, leads me to hope, that some fungoid tumours of the bladder, especially in the female, generally supposed to be beyond the reach of medicine or surgery, may yet be made amenable to the latter at least. The ready distensibility of the female urethra admits of the examination of the bladder by the finger without difficulty, and thus can be ascertained the size, form, and position of any foreign body in its cavity; and the question of treatment can then be decided, whether, as in Mr Warner's case, the urethra should be divided and a ligature be placed around the growth; or it should be excised; or, as in Dr Davis's, some kind of bougie should be used; or, finally, it should be left to take its course. Mr Warren,[1] it appears, contemplated the excision of a fungous tumour of the bladder, in a case in which he was consulted, and he proposed to himself one of two plans,—viz., either "to make an incision through the vagina, and cut away the diseased portion of the bladder; or to cut open the urethra on its side, and, introducing a finger to examine the tumour, to seize it with a hook, draw it out as much as possible, and excise it." As he could find no precedent for either method, and dreaded, from the one an incurable fistula, and from the other effusion of urine into the cellular membrane about the bladder, he declined urging the operation. The patient, a delicate lady, æt. fifty, lingered about eight months, and dying in the country, no examination of the body was made. To his first plan the objection suggested is valid; to the second, I think not; and probably, had he been acquainted with Mr Warner's case, he might have embodied his idea in practice,—it may be, unsuccessfully; but the subject, I think, merits attention.

[1] Warren on Tumours, p. 393.

From any known treatment of such cases there is no hope of cure, and but little, if any, of benefit; they end almost uniformly fatally, either from the effects of hæmorrhage, or, as Mr Warren suggests, "from consequent derangement of the stomach and intestines."

Duchess Street, Portland Place.

Appendix.—On the 18th of September last, my friend, Mr Tickell, asked me to see with him Mrs P., a lady about thirty-eight years of age, residing in my neighbourhood. Mr Tickell had been attending her from the 6th inst. She suffered at first great pain in making water, and a very frequent desire to do it; her tongue was foul and her pulse rather quick, without other symptoms. Aperients were given, and a mixture of liq. potass, sp. æth. nit., tr. hyoscyam, and tr. opii. At the end of a week, during which time menstruation had come on, she was no better. Hip baths, aperient medicine, leeches to the urethra, and opiate suppositories, were now tried, with the same effect. The urine was examined and found to be alkaline, and to deposite a mucous sediment. The speculum vaginæ was used, and the uterus appeared healthy and in right position; a catheter was also passed. Both instruments gave much pain, as they pressed in passing against one part of the urethra, but no swelling could be discovered. On the 18th, when I saw the patient, she was no better. She suffered very severely in making water; there was marked tenderness at a point about midway between the orifice of the urethra and the bladder; but no swelling when examined by the finger in the vagina; no tenderness in the bladder itself, however freely the sound which I introduced was moved about; but at the part of the urethra felt to be painful in the vaginal examination, the sound also occasioned pain. She was taking infusion of buchu with dilute sulphuric acid. This was ordered to be continued, and a full-sized bougie to be passed, and worn as long as it could, daily. The passage of the instruments was persisted in for a week, increasing the size daily; it gave much pain, but by that time the patient was well. *The urine, however, had become healthy*, and it is probable that the affection was only due to a chronic inflammation of the mucous membrane of a portion of the urethra and neck of the bladder. I suspected at first a vascular excrescence, but nothing of the kind came away.

Note.—For further information on these tumours the reader is referred to the following sources, which have been brought under my notice since the foregoing essay was completed:—

Hosack on Sensitive Tumours of the Female Urethra, in the *American Journal of Medical Science*, vol. xxv, p. 509.

A Case, and Remarks by Dr Lever, in the *London Medical Gazette*, Jan. 9, 1846.

Theses, by MM. Bavoux and Asson, reviewed in the *Archiv. Gén. de Méd.*, fourth series, vol. ix., pp. 101 and 235.

ARTICLE III.—*Contributions to the Pathology of the Heart.* By HALLIDAY DOUGLAS, M.D., Fellow of the Royal College of Physicians, Physician to the Royal Infirmary, Lecturer on Clinical Medicine, Edinburgh.

FEW cases of disease of the heart exist without more or less hypertrophy or dilatation. At all events, the evil effects of heart affections never, perhaps, arise until one or both of these conditions are established, and the one probably never exists without the other. These morbid conditions, therefore, exert a very important influence on the progress of all cases of heart disease, and their mutual relations are very intimate; their frequency gives them an almost universal importance, which has been in some degree lost sight of. It is my desire in the following reports to show the frequency and intimacy of their natural connection, and the supreme influence which they, especially dilatation, exert on the progress of individual cases.

The *cases* selected for publication are average specimens of forms of disease which are not rare; there is nothing novel in the facts they contain; and they are narrated as shortly as is consistent with accuracy.

The *remarks* are intended to indicate the real value of the facts of the cases, and will form the groundwork of a comprehensive summary of general rules, which the cases appear to establish.

The propositions regarding the intimate connection and the supreme importance of hypertrophy and dilatation, will be demonstrated from the facts of almost every form of heart disease, and, at the same time, the details of the cases will afford many opportunities of considering other questions of practical importance.

It is difficult or impossible to convey a precise idea of the state of the heart, as regards the degree of hypertrophy or dilatation, and the relative proportion they bear to each other. An approximation only can be made to accuracy of description, by stating the weight along with the general bulk and capacity of the organ; and a statement of the degree of these conditions can be made only in terms that are indefinite. The description of the degree of hypertrophy or dilatation, in point of fact, necessarily resolves itself into an expression of opinion that this or that was the state of the organ.

CASE I.—*Dilatation of the Heart; Hypertrophy; Obstructed Circulation; Chronic Chest Symptoms; Cardiac Symptoms recent; Progress rapid; Death unexpected.*

Alexander Mackay, æt. 50, a coach driver, of robust frame, was admitted into the Royal Infirmary on the 27th November 1847. At the time of admission he had very urgent dyspnœa and tumultuous action of the heart; frequent harrassing cough, with scanty expectoration of mucus tinged with blood. He had the puffy and livid aspect characteristic of obstructed circulation; there was faint jaundice, and slight general œdema. The pulse was so small, rapid, and irregular, that it could not be reckoned. Urine was copious, high coloured, and turbid. The bowels were slow.

History.—He ascribed his illness to exposure to cold and wet, eight weeks previous to admission. His symptoms had been distressing for only four weeks —the dyspnœa and palpitation especially so, and profuse hæmoptysis had occurred. He had not suffered from any definite pain. After careful questioning, he admitted that he had been liable to cough from boyhood, and at times to slight difficulty of breathing—but never to such a degree as to lay him aside from work. He always was much exposed to the weather. He never had rheumatism nor palpitation, till the existing attack. He said his habits were not intemperate.

Physical Signs.—The impulse of the heart was feeble and undefined; the precise situation of the apex could not be determined. As already stated, the action of the heart was tumultuous. The sounds were faint and obscured by cooing and sibilant respiration, which was intense and general. No abnormal murmur accompanied the heart's sounds, but they were irregular.

The hepatic dulness was extended, and the dull space was tender, as was also the epigastrium.

Subsequent Progress and Treatment.—He was leeched in the scrobiculus cordis, and purged with medicine, with immediate and marked relief. He slept quietly; the tumult of the heart's action subsided; the pulse was reckoned readily, 108, synchronous with the heart's beat, and having an occasional irregularity corresponding to the remaining irregularity of the heart's action. He was blistered on the chest, and the urgency of the symptoms continued to decline, with the exception of the jaundice, which became progressively more decided. At the same time there was gradually developed a state of languor and depression; and on the third of December perfect prostration supervened, with tumultuous action of the heart, a fluttering pulse, &c. This state lasted only a few hours, when he died.

Post-mortem Examination.—The surface had a universal bright yellow colour. The volume of flesh was large. There was dropsy of the cellular tissue and cavities.

The heart was increased in its general bulk, and weighed sixteen ounces. It had the rounded form characteristic of dilatation. The left ventricle was distended by a soft recent coagulum, and was manifestly dilated. The thickness of the muscular walls was not very evidently increased at any given point. The right ventricle was dilated, not distended.

The aorta was very little dilated.

Both lungs contained masses of apoplectic condensation.

The liver and kidneys were loaded with blood.

Remarks.—This case is one of a large class which establishes the fact that all the effects of dilatation may arise in cases in which the heart is truly hypertrophied.

The phenomena indicating the dilatation were in no respect different from what have long been regarded as the signs of that state. Some of these signs, however, I am disposed to think, have a more specific relation to the dilatation than has been usually acknowledged.

Such conditions as lividity, dropsy, hæmoptysis, pulmonary condensation from hemorrhage, and the distended state of the left ventricle, have been very absolutely associated with disease of the heart; but they have not been so pointedly associated with dilatation, as this case and many others testify is the fact. No doubt these conditions occur in other cases than mere dilatation; but even in such cases, a very intimate relation may be traced to dilatation, or to a state of the organ causing the same obstructive effects on the

circulation. In the present case, the degree of dilatation was great, and no other lesion existed, unless hypertrophy, to account for the extreme degree of the conditions above referred to; and it will be found that these do not ordinarily exist without more or less dilatation. These symptoms in most cases exist late in the attack, and may be regarded as the index of a preponderating and formidable dilatation.

The post-mortem distension of the left ventricle in this case may be noticed. In the diseases of the heart I have never met with this, except in cases in which the capacity was increased, and the power of the organ consequently embarrassed. In such cases death is usually sudden, or rapid as in this man.

From the facts stated, and from the physical signs in the case, it is manifest that a very embarrassed and enfeebled state of the heart's action existed, but these are the very opposite of the conditions which exist when hypertrophy is in excess, which involves augmented power. We are therefore shut up to the conclusion that the embarrassment of the circulation, and its consequent effects, were due to the dilatation of the heart, notwithstanding co-existing hypertrophy, or increased muscular substance.

It is very difficult to determine what value we shall attach to the statements given by patients of the history of their diseases. In so far as the statement can be trusted in the present case, it is important to observe that his very old-standing chest symptoms were not of heart disease, but probably of a chronic bronchitis. This state of the lungs, by hindering the freedom of the circulation, was no doubt the primary cause of the cardiac changes which gradually and insidiously supervened; and only manifested themselves when, from accidental causes, the balance of the circulation was more disturbed; and then, the defective state of the heart incapacitated it for the work of restoring the equilibrium of the circulation; and the subsequent rapid progress of the case, with prominent *heart* symptoms, became remarkable. This rapid progress in the later stages is to be taken in connection with the very dilated state of the heart.

In the treatment of this case we see a well marked instance of the benefit of moderate leeching to the scrobiculus cordis. The ultimately unfavourable result of the case, with recurrence of all the worst effects of dilatation, only renders the temporary but decided relief afforded by leeching the more remarkable.

CASE II.—*Hypertrophy of the Heart; Pleuro-Pneumonia: Sudden Death.*

John Craig, æt. forty-nine, a blacksmith of large frame, but spare, and according to his own statement, of temperate habits, was admitted on the 19th May 1845, with urgent dyspnœa, cough, and expectoration, and œdema of the lower extremities and abdomen.

History.—He stated that these symptoms had existed more or less for three months, accompanied by attacks of palpitation; and that up to this time the state of his health had never attracted his attention.

Physical Signs.—The pulse was hurried, visible, synchronous with the cardiac impulse, but irregular. The cardiac impulse was diffused—communicated to

the upper part of the belly; it was irregular in force as well as in frequency. The apex beat in the sixth intercostal space, five inches left of the mesial line. The præcordial dulness did not exceed two inches in vertical diameter, and was bounded beneath by tympanitic percussion, which existed higher in the left front than is usual. The horizontal diameter extended from the mesial line, five inches leftwards,—the dulness was incomplete in the outer three inches. The heart-sounds were obscured by intense bronchitic cooing and sibilance.

The right side of the chest was dull on percussion, except in the subclavian space, with enfeebled respiratory and vocal signs.

Progress.—On the evening of the 20th, after rising from bed, he died suddenly.

No treatment of importance had been put in practice.

Post-mortem Examination.—The right pleura contained a large quantity of turbid serum, and lymph in loose flakes, and layers adhering to both pleural surfaces. The middle and inferior lobes of this lung were condensed in the second stage of pneumonia.

The heart had the rounded form of dilatation; it weighed 23 oz., and was much increased in general bulk. It was dilated in all its cavities. The valves were competent. The aortic valves were increased in their depth by a supplementary, membranous, translucent formation from the margin of each valve. The comparatively thickened and opaque margins of the original valves were well defined, where the new formation was attached; and to this margin on the ventricular surface there were attached small wart-like excrescences. The other organs were healthy.

Remarks.—This man's employment is to be noted in connection with the degree of hypertrophy of the heart. There is a very intimate connection between the higher degrees of hypertrophy in many cases, and those occupations in which violent and protracted muscular effort is called forth. This is the case, too, when the hypertrophy is secondary to valvular disease.

There is reason to believe that we were well informed, that cardiac symptoms had made their appearance only within three months. But it cannot be affirmed how far these symptoms depended upon the state of the heart; or how far they may have been influenced by the state of the right lung and pleura. In so far as they depended upon the altered state of the heart, the hypertrophy cannot be supposed to have been in any great degree their cause;—that hypertrophy certainly existed, during a long period antecedent to the date of the symptoms, when, indeed, his health was thought good. The origin and progress of the dilatation cannot be determined—it, too, may be supposed to have existed long previously, without having caused any embarrassment of the circulation; and the pleurisy may be considered the active agent, exciting the symptoms of his last illness. Be this as it may, the symptoms were mainly those of an embarrassed circulation, which neither pleurisy nor hypertrophy of the heart can of themselves explain; hence, we infer the importance of dilatation in influencing the progress of such cases. Cases, illustrative of this point are not rare; and while any organic disease modifies the progress of an accidentally developed acute inflammation, there can be no doubt that advanced dilatation of the heart complicates such a case most seriously—embarrassing the treatment—and ex-

posing the patient to the most imminent risk. Hypertrophy of the heart, on the other hand, when dilatation is slight or wanting, is in no degree inconsistent with speedy and complete recovery from an acute attack.

Sudden death in cardiac diseases is connected, in a large proportion of cases, with dilatation ;—though, as in this man, the dilatation may co-exist with extreme hypertrophy. Such a mode of death is one of the chief risks in all cases in which dilatation of the heart has an acute inflammation superadded to it.

The facts of this case afford very direct evidence of the value of certain physical signs. The pulse had neither the small size nor indistinctness which frequently accompanies dilatation, owing, probably, to the muscular hypertrophy of the heart.

Another condition of the pulse to be noted in this case is, that it was visible. In commenting upon other cases, the opportunity will occur of giving a more extended view of the value of this sign. All that can be said now is, that it is noticed chiefly in cases in which, as in this man, a large hypertrophy is accompanied by an increased capacity of the heart. This cannot be regarded as of itself an important sign; it occurs in many cases in which no embarrassment of the heart's action exists; and, from this fact, I am disposed to connect it more directly with hypertrophy than with the dilatation of the organ.

The impulse of the heart affords important data for determining the degree of dilatation. It is manifest that such hypertrophy as existed in this man, must greatly modify the impulse, which usually becomes *extended*, and more or less *forcible*. Now, from the relative degree of these two conditions, the degree of dilatation may be, in in some measure, determined. As the dilatation increases, the force becomes less in proportion to the extent; but still more striking will the contrast appear between the *feebleness* or undefinedness of the impulse, and the *extent* of the præcordial space dull on percussion.

The extended horizontal diameter of the præcordial space of dull percussion, is an important indication of dilatation. It cannot be affirmed to be of itself absolutely significant of dilatation, as will be illustrated in other cases, but in connection with other signs, especially the state of the impulse, it is the most invariable and important of the physical signs. It bears a very direct relation to the rounded form of the heart which occurs in dilatation.

The displacement of the apex of the heart to the sixth intercostal space is a sign, of whose value I am not confident. It has been regarded as very absolutely indicative of hypertrophy; and I have not met with it, except in such cases. But it frequently does not occur; and the apex still presents in the fifth space, in many cases of very excessive hypertrophy—even when the hypertrophy is of the left ventricle mainly; but still more certainly where the right ventricle is the seat of the hypertrophy. In the present case, the bulk of the heart was partly due to the hypertrophy, which was general;

though it must not be overlooked that the capacity of the cavities was great.

The supplemented state of the sigmoid valves is very unusual. I do not remember to have met with it in the aortic valves before; it is believed to occur more frequently in the pulmonic valves, though even here it is rare. We have no means of ascertaining whether these valves had at any former time been incompetent—they had not the aspect of having ever been sufficiently deformed for this.

The following case was under observation several months. The leading facts only will be reported; without a minute account of its progress and changes:—

CASE III.—*Hypertrophy of the Heart; Dilatation; Death from Erysipelas.*

Andrew Ormiston, æt. 44, a waggon-driver, was admitted on the 17th Dec. 1845. He was a robust man, of intemperate habits. He had a bloated countenance, slight general œdema, and urgent dyspnœa. His sleep was disturbed: and violent paroxysms of angina pectoris occurred. He had little cough, and no expectoration.

History.—He stated that these symptoms, with occasional fits of palpitation, had been troublesome only within a fortnight; they occurred suddenly, and without any definite pain. He had been unable for his work for ten days.

Physical Signs.—The heart's action was tumultuous. The impulse was widely diffused, and not proportionately forcible. The præcordial space dull on percussion had a horizontal as well as a vertical diameter of five inches. The sounds of the heart were free of murmur. The first was prolonged, but obscure; and at times reduplicated. The second was increased in loudness, at the base of the heart. The pulse at the wrist was always small, and more or less irregular; it was imperfectly synchronous with the heart's impulse.

The left side of the chest was extensively dull on percussion; with feebleness of the respiratory signs; and some obscure wheezing and sub-mucous rattle.

Progress.—In the progress of the case the symptoms underwent frequent variations. The physical signs continued very much as reported at the time of admission. His great distress resulted from the paroxysms of angina pectoris, with dyspnœa and tumultuous action of the heart; and during those attacks dropsy was usually considerable, and the urine scanty. Angina frequently existed without dropsy; but the most marked and continued relief to this, and to other symptoms, resulted when copious diuresis occurred—whether it was spontaneous or the effect of treatment. Up to the last, though dropsy had nearly disappeared, the paroxysms were severe and frequent. On the 28th April, four months after the cardiac symptoms had become troublesome, erysipelas of the head occurred, and he died on the 30th.

Treatment.—The principal treatment, and that whose effects were most apparent, consisted in small local and general blood-letting, and the administration of diuretics.

Up to a late date the application of three or four leeches to the scrobiculus cordis, afforded speedy relief to the anguish of the paroxysms; and was at times succeeded by copious diuresis. On one occasion, when the urine was nearly suppressed, dropsy was considerable, with dyspnœa, cough, and the signs of general bronchitis; and a tumultuous state of the heart's action; all treatment having proved unavailing, the remedies were omitted, and digitalis given in the form of infusion. Within twenty-four hours, diuresis commenced; and in a few days the daily quantity averaged 80 oz.,—at the same time the heart's action was calmed; and the pulse fell to 50, with a rare irregularity, and his distress became altogether less. The quantity of the infusion taken was under 8 oz. in the course of thirty hours.

Post-mortem Examination.—I regret this part of my report is very imperfect; the notes of the post-mortem examination having been mislaid. The following statements are made from memory; but may be relied on so far as they go:—

The heart was hypertrophied in all its parts: and dilated in both ventricles,—the left most decidedly, the walls of which were nearly an inch thick. No lesion of the valves existed. The left pleura contained several quarts of high-coloured serum, with much lymph in loose masses, and in membranous layers on both pleural surfaces.

Remarks.—This case contrasts with the others of this series in several particulars. One of the most striking of these is the manner of death, and its immediate cause. In the other cases death resulted more or less directly from the state of the circulation. This man was cut off by erysipelas. It has been asserted that there is a tendency to such a termination in cases of disease of the heart; but I have been quite unable to trace any consistent relation between them. They do not concur more frequently than may be explained by accidental causes.

The physical signs of the dilatation agree only in part with the signs in the other cases. It is important to bear in mind this partial disagreement; it will be observed more or less in every case; and arises from the different degrees of the dilatation and hypertrophy.

In this, as in the other cases, the signs of the greatest value were the horizontal extent of dull percussion; and the disproportionate feebleness of the impulse compared with its extent. The *smallness* and *want* of *synchronism* of the radial pulse with the heart's beat, are valuable signs; and are closely connected with the dilatation; though it may be seen, from other cases of this series, not always to occur, even when dilatation is great. The *redoubled* state of the heart's sound is a sign of which I have been unable to discover any satisfactory explanation. It usually affects the second sound; and I have been in the custom of attributing it to mere irregularity of muscular contraction, and consequent want of synchronism in the valvular and tendinous tension on which the heart sounds largely depend. The *muffled* state of the first sound demonstrates, that perhaps too much value has been attached to mere *clearness of tone* of this sound, as indicative of dilatation. It is true that the muffled tone arises from muscular hypertrophy; while clearness is connected with an opposite condition. Still the fact in this case must be borne in mind, that the muffled tone of increased muscular substance persisted, notwithstanding the supervention of dilatation and its gravest effects.

There are other points in this case suggestive of important questions. I shall not, however, occupy space in the meantime by taking up these, as the facts do not establish any specific and practical rule. I refer especially to the angina and its relation to the state of the heart; and to the effect of the diuretics, which act most capriciously under all circumstances, in the diseases of the heart; and appear to

be much hindered by the obstruction to the circulation arising from dilatation.

Case IV.—This case is necessarily imperfect, a post-mortem examination not having been permitted. The *dilatation* of the heart was, however, so manifestly indicated by the signs, that the case is instructive.

Hannah Henderson, æt. twenty-five, a domestic servant, was admitted into the hospital on the 2d February 1845. She was slender in her general development, and had a slight abnormal prominence of the sternum and costal cartilages. She was labouring under general dropsy, urgent dyspnœa, palpitation, and cough with expectoration; her face was swelled and livid. She also suffered from sickness and vomiting, with acute tenderness of the epigastrium.

History.—She could not give a definite account of the date and origin of these symptoms. She said she never had been strong; and had been liable to distressing paroxysms of dyspnœa and palpitation for years, but never such as to unfit her for work. Her existing extreme distress had occurred within a fortnight, when the swelling had first attracted attention.

Physical Signs.—Mucous and sub-mucous rattle existed in all parts of the chest. The apex of the heart appeared to beat in the fifth intercostal space. The impulse was neither diffused nor forcible. The space of præcordial dull percussion extended five inches leftwards from the mesial line; and from the third left cartilage to the costal margin. The sounds of the heart were hurried, regular, and free of murmur. They presented the clearness of tone which has been considered characteristic of dilatation. The radial pulse was small. The veins of the neck were permanently gorged.

Progress and Treatment.—Small cuppings from the chest were employed, with laxatives and diuretics. This effected a very slight and temporary benefit. The urine was always scanty; the pulse became irregular; dyspnœa and vomiting caused much distress; and latterly the dropsy greatly increased. She fell into a state of semi-stupor, which lasted several days, with gradual exhaustion, and she died on the 19th March.

Remarks.—The steadily unfavourable progress of this case is characteristic of that state of the heart in which the dilatation has disproportionately overbalanced the power or hypertrophy.

The horizontal extent of the præcordial dull percussion, the smallness of the radial pulse, and the clear tone of the first sound of the heart, corroborate each other as indicative of dilatation. It is useless to inquire what was the degree of hypertrophy in the absence of anatomical evidence. The form of the chest, the vertical extent of præcordial dull percussion, and the long duration of her symptoms, render it probable that a considerable increase of muscular substance had occurred; but these signs are not inconsistent with an extreme degree of dilatation, to which the apparent extent or bulk of the heart was probably in a great measure due, and whose effects were otherwise so manifest.

This very disproportionate dilatation satisfactorily explains all the phenomena of the case, as well as the total inefficacy of treatment. The hopelessness of such cases is in exact proportion to the dilatation or enfeebled power of the heart. Depletion can be of service only in so far as it disburdens the embarrassed heart of the excess of blood which it struggles to propel; and the instant the depletion causes any impairment of the power of the already enfeebled heart,

the embarrassment of its action, and the consequent obstruction to its circulation, is increased.

I am disposed to disapprove of the cuppings practised in this case;—it is rarely necessary to take blood in this way. When it is desired to remove any quantity of blood, a vein should be opened; but in the majority of cases, when the heart's action is embarrassed, four or six leeches will have the effect of allaying the tumult of the heart's action, and give immediate relief.

It is not necessary to multiply cases of this description. Those already narrated will serve to exemplify the direct and powerful influence of dilatation, even when hypertrophy exists; and in a large proportion of cases all the later and fatal effects of diseased heart will be found, as in the preceding cases, to depend upon dilatation.

The rarity of fatal cases in which the hypertrophied heart is as yet little dilated, depends upon the well-known difference between the effects of *hypertrophy* and *dilatation*. The *former* acts temporarily, and causes local effects upon some distant organ, which usually has been weakened by pre-existing disease—viz., rupture of the arteries or substance of the brain, and consequent apoplexy. The *latter* causes more permanent effects on the entire system, through the impeded circulation.

In considering cases of other forms of diseased heart and of the blood-vessels, we shall have the opportunity of indicating the more direct effects of hypertrophy.

The next series of cases will be aneurisms of the left ventricle of the heart, which exemplify in a remarkable manner the effects of dilatation.

(*To be continued.*)

ARTICLE IV.—*On Typhoid and Typhus Fevers,—An Attempt to Determine the Question of their Identity or Non-Identity, by an Analysis of the Symptoms, and of the Appearances found after Death in Sixty-six Fatal Cases of Continued Fever, observed at the London Fever Hospital from January* 1847 *to February* 1849. By W. JENNER, M.D., London, Licentiate of the Royal College of Physicians.—(*Continued from p.* 736.)

SYMPTOMS FURNISHED BY THE ORGANS OF THE SENSES.

Typhoid Fever.—Epistaxis occurred in one third of the cases, or in five of fifteen, the particulars of which were obtained from the commencement of the disease; in neither of them was it excessive, and in one case only a few drops of blood were lost. The hemorrhage took place in these five cases respectively on the 4th, 8th, 11th, 12th, and 12th day; it was repeated in the last on the 14th day.

In six cases it was not known whether epistaxis had taken place prior to the patients' admission into the hospital, which in four of the six was on the 6th, 13th, 14th, and 21st day, and in two at an undetermined period of the disease.

Hearing.—Almost a fourth, or six of the twenty-three patients had deafness more or less complete after they entered the hospital; and three tinnitus aurium; one of these three subsequently became deaf. The difficulty in hearing made its appearance on from the 12th to the 25th day.

Eyes.—The conjunctivæ were injected in three cases, and in one of the three continued so from the time the patient was first seen, *i.e.*, the 13th day of the disease, till the fatal termination on the 23rd day; the other two came under observation between the 21st and 27th days. In one case they were injected when the patient was first seen on the 9th day of the disease, but on the 11th day they were pale. In ten other cases the conjunctivæ were noted to be pale, the patients being under observation repeatedly at different periods of the disease; one of these ten cases was seen as early as the 5th, and one as late as the 30th day.

In two cases only were the pupils of the eyes noted to be smaller than natural, while in seven cases they were decidedly dilated; the pupils of the eyes in two patients first observed on the 5th and 14th day of the disease, were respectively at those dates noted to be normal in size, but on the 8th and 22d day respectively, they were larger than natural.

The exact day of the disease on which they were first observed to be greatly dilated, varied in six cases, respecting which notes on the point were taken, from the 8th to the 22d day; a seventh case came under observation after the fever had continued an uncertain time.

Typhus Fever.—Epistaxis.—There was not a single instance of bleeding from the nose in twenty-three cases, the particulars of which, before they entered the hospital, were obtained; nor did one of the forty-three patients, whose cases we are examining, suffer from epistaxis after their admission into the hospital.

Hearing.—Nine, or rather more than a fifth of the forty-three patients labouring under typhus, had more or less complete deafness as a consequence of the fever, and one tinnitus aurium.

The difficulty of hearing was first observed from the 10th to the 13th day of the disease.

Eyes.—The conjunctivæ were more or less intensely injected in twenty-five cases; and in all of those in which the opportunity occurred of observing the date of the first appearance of this increased vascularity, it began during the second week. In one case only, seen during the first week, were they injected at that time; in eleven of the twenty-five cases in which the conjunctivæ were more vascular than natural, the pupils were contracted.

THE TONGUE.

Typhoid Fever.—Notes respecting the physical condition of the tongue were made in twenty of the twenty-three cases here considered.

In six, it was moist during the whole time the patients were under observation, or respectively from the 10th, 11th, 12th, 15th, 18th, and 20th day, till their death; four of them dying between the 15th and 30th days, and two some time after the latter date. In four of these six cases, it was clean or slightly furred, neither of the four dying before the 24th day of the disease; in the fifth it was brown and furred, and in the sixth yellow and furred.

In another case, first seen on the 20th day, and which proved fatal on the 27th day, the tongue, with the exception of the 25th day, when it was dry in the centre, was moist throughout the disease.

So that the tongue may be said to have been moist during the whole course of the disease in a third of the cases in which its condition in that respect was noted.

In twelve cases the tongue was entirely or partially dry and brown, at some period of the disease; in two, however, the hue never exceeded in depth a pale yellow brown; in six, it could never be said to be dark brown; while in two it was almost black, one of the two, however, was at the time suffering from extensive gangrene of one lower extremity.

In about one-third of the cases, *i.e.*, seven of the twenty, it was more or less red; in one-fourth glazed entirely or partially; and in one-fifth of the twenty it was deeply fissured at some period of the disease.

In one case only was the patient unable to protrude the tongue when directed to do so.

There was no regularity in the order of the succession of these various conditions of the organ; thus, in one case seen on the 8th day, it was at that time covered with a thick layer of brown mucus, and rather moist; on the 9th and 10th days it was partially covered with yellowish brown flakes and fissured; on the 11th day moist, and covered with a thick white fur; on the 12th and 14th days it resumed the appearance it presented on the 9th and 11th days; on the 15th day it was again moist and white; on the 16th it presented a dry centre; on the 17th, 18th, and 19th days, it was pale brown over its whole surface, and fissured in the centre, while on the 22d day its centre only was pale brown, its edges being white, on which day the last note of the tongue was made; the patient died on the 25th day of the disease.

Typhus Fever.—The varying physical conditions of the tongue were recorded in forty-one of the forty-three cases here analyzed.

In one only was it moist during the whole time it was under ob-

servation; the patient, a female child, aged ten years, was first seen on the 15th day of the disease, the tongue being then moist and white; from the 18th to the 21st day it was moist and partially brown; the child eventually died from gangrenous tubercular abscess of the lung.

It was moist in ten cases when first seen on from the 8th to the 11th day, but grew darker as the disease advanced.

The tongue in thirty cases was dry when first under observation; the day of the disease on which twenty of the thirty were seen varied from the 6th to the 18th day; of the other ten, no history to determine the date of the disease could be obtained.

It was moist and slightly furred in a patient seen on the 9th day, dry and slightly furred in the same patient on the 10th, 11th, and 12th days; this was a very mild case, in which the fatal termination was caused by phlebitis.

In thirty-three cases the tongue was completely dry and dark-brown at some period of the disease.

The patients, in whom it was dry and brown when they were first seen, and the date of whose disease on their admission was exactly or approximatively ascertained, came under observation from the 6th to the 13th day.

It was moist and loaded in three patients on their admission the 8th, 8th, and 9th day of the disease, and became dry and brown in these three cases, respectively on the 10th, 10th, and 12th day.

In four only of the forty-one cases, or about one-tenth, was the tongue fissured, and in five only, or about one-eighth was there any redness of the organ. One-fifth of the patients were unable to protrude the tongue at some period of the disease.

ABDOMINAL PAIN, TENDERNESS AND PHYSICAL SIGNS.

Typhoid Fever.—Of eleven patients, respecting whose condition previous to their admission into the hospital, pretty accurate histories with reference to the point were obtained, five, or nearly one-half suffered pain in the abdomen, as one of the earliest symptoms of the outset of the disease.

Six of twenty-one patients, of whom notes on the point were made, suffered from pain in the abdomen after their admission into the hospital; these patients were received into the wards between the 8th and 16th day of the disease; in one case only was any pain complained of in the fourth week.

No tenderness existed in five cases while they were under observation, respectively from the 5th, 15th, 19th, and 21st day, till the 12th, 27th, 28th, and in two cases until after the 30th day.

Tenderness of the abdomen was present in three-fourths, or in fifteen of twenty cases, respecting which notes on the point we are considering were made.

In one of the fifteen, however, it did not arise till peritonitis, the consequence of perforation, had taken place.

The tenderness was sometimes limited to the right iliac fossa, and though frequently extreme, was occasionally trifling.

Gurgling, generally situated in the right iliac fossa, was present in five cases, or nearly a fourth; the bowels being relaxed in every one of the five cases.

The abdomen was full in three cases, resonant in five, full and resonant in ten cases. In one instance only did it preserve its normal form, and afford a natural percussion note. The size of the abdomen was often extreme, but whether much or little distended, its shape was invariably the same, and somewhat peculiar; its convexity was from side to side, and not from above downward; the patient was never "pot-bellied," but tub-shaped. The cause probably was, that the flatus chiefly occupied the colon ascending, descending, and transverse.

Typhus Fever.—One only out of twenty cases, of which histories on the point were obtained, had suffered pain in the abdomen among the first symptoms of the disease; this was a female, aged fifty-six, in whom the catamenia had for some time appeared at irregular intervals; she fainted suddenly nine days before her entrance into the hospital, and on recovery experienced severe headache and pain in the abdomen; on the following day the catamenia flowed freely, and all pain in the abdomen vanished.

Another admitted on the 8th day of the disease, had experienced griping pains in the abdomen from the 5th to the 7th day; there was no return of the pain in either case after admission.

Two patients, after they were received into the hospital, suffered from slight pain in the abdomen, respectively on the 8th and 9th day of the disease.

There was trifling tenderness of the abdomen in five of twenty-six cases seen during the second week of the disease; in two others there was tenderness at an uncertain period of the disease.

Of nine patients under observation, during the third week of the disease, and of whom notes were made with reference to the subject, one only experienced any tenderness of the abdomen.

Tenderness of the abdomen was then present at some period of the disease in nine cases, but in eight of them it was trivial and transient; in the ninth it was more decided, commenced gradually on the 12th day, and continued till the fatal termination on the 20th day; it was limited almost entirely to the hypogastric region. The subject of this anomaly suffered from retention of urine, and after death there was found minute capillary injection of the mucous membrane of the bladder, more marked on its anterior than on its posterior surface.

Gurgling was detected in one case only, and in that there existed at the time considerable diarrhœa.

The abdomen in twelve of forty-one cases, of which notes on the point were made, was full and resonant; in three only of these twelve cases was it unnaturally distended, and in neither of the three was it noted to possess the peculiar shape of the typhoid abdomen.

In twenty-two cases there was neither fulness, resonance, tenderness, nor gurgling; in these twenty-two cases the abdomen presented all the physical signs of health, and in two of them it was noted to be somewhat concave.

DISCHARGES FROM THE BOWELS.

Typhoid Fever.—Particulars were obtained from the patients or their friends, regarding the condition of the bowels before admission into the hospital, of sixteen of the twenty-three cases.

In two of the sixteen the bowels were confined, the patients being admitted respectively on the 9th and 15th day of the disease; in four, confined till after a dose of aperient medicine, from the moment of the action of which diarrhœa set in (these four were admitted into the hospital respectively on the 8th, 8th, 12th, and 29th day); in two cases which came under observation on the 4th and 13th day, the bowels were stated to have been regular up to those dates, one stool only having been passed daily.

The bowels were much relaxed before the admission of the remaining eight cases, in the majority from the outset of the disease, and in all before the administration of aperients.

The condition of the bowels was recorded frequently after the admission into the hospital of twenty-one of the twenty-three cases.

Of these twenty-one, two only needed aperients, one a single dose of castor oil, the second repeated doses of that and other aperients, while one case in the first week of the disease, three in the second, two in the third, and six during the fourth week, required the repeated exhibition of opiates to restrain diarrhœa. Of two patients seen during the first week of the disease, one passed from two to six watery stools daily, the other from three to four.

In the second week the number of stools passed each day varied from one to four. Of seven patients under observation during this time, five passed as many as three stools each, and one four stools in twenty-four hours. Of fourteen patients of whom notes were taken during the third week of the disease, nine passed three stools each, and two as many as four and six in one day, while in the fourth week fourteen out of fifteen patients passed three stools each on one day, and ten of fifteen from four to six stools. From the above calculations I have excluded the two cases which required an aperient after they came under observation. One of these, a male aged twenty-eight, entered the hospital on the 10th day of the disease. His bowels had been confined from the first, and for the twenty-four hours succeeding his admission he passed no stool; half an ounce of castor oil was given; on the following day he had three stools, on the

13th day two, on the 16th day he passed no stool, and from that time the bowels continued regular till the 25th day of the disease, when the patient died of profuse hemorrhage from the bowels. The other case was that of a female who entered the hospital on about the 15th day of the disease, but who had been under my observation for some days preceding her admission. From the first her bowels had been confined. On the 16th day she passed one stool, on the following day none, she then had half an ounce of castor oil, which acted three times; from this time till the 25th day, she scarcely had a stool without the aid of aperients. On the 28th she seemed nearly well, her bowels were quite regular, and she left her bed. On the 36th day she was suddenly seized with severe pain in the abdomen, took an aperient on the 37th day, and on the 38th passed four dark solid lumpy stools, on the 40th four equally solid stools; from this time she had one or more scanty solid stools till her death on the 46th day, from extensive ulceration and perforation of the agminated glands at the lower part of the ilium.

The consistence of the stools was watery in twelve cases at some period of the disease, and soft, pultaceous, or almost fluid in four others.

In eight cases only were stools of natural consistence passed after the patients came under observation, and in five of these the stools were watery during some period of the disease. Their colour varied from pale brown to almost black; when watery they were usually pale yellowish brown, with a sediment composed of small solid yellowish particles.

Seven out of twenty-one patients, particulars respecting whose stools were recorded, passed blood from the bowels. In one, discharges of blood took place on the 6th and 7th day of the disease. On the tenth the stools were healthy in appearance, and well formed, and although they afterwards became watery, there was no return of the hemorrhage. One man passed a small quantity of blood on the 8th, 9th, and 10th days of the disease, and again from the 28th to the 32nd—*i. e.*, the day of death—the stools between the two attacks of hemorrhage being watery, but free from blood. In one case blood was mixed or passed with every stool from the 14th to the 21st day, the patient dying on the 25th day.

In four cases hemorrhage from the bowels occurred during the last day or two of life, the patients dying respectively on the 17th, 23rd, 25th, and 28th day of the disease.

The blood lost varied in quantity from an ounce or two, to two or three pints; in hue from black to bright red; and in consistence, from a reddish watery fluid to the consistence of treacle, and even solid clots.

Typhus Fever.—The bowels were stated to have been more or less confined from the commencement of the disease till the admission of the patients, in ten out of nineteen cases, the histories of which were obtained with as much accuracy as possible; in six of

these nineteen cases they were regular from the first; five of the nineteen had taken one or more powerful doses of aperients, without producing any diarrhœa; three only of the nineteen had diarrhœa before their entrance into the hospital, and in two of the three it supervened after the use of aperient medicine; these nineteen cases were admitted between the 5th and the 12th days of the disease. Notes were taken of the condition of the bowels in the whole of the forty-three cases after the patient's admission into the hospital. Twenty-two needed while under observation the use of from one to four doses of aperient medicine, while in one case only was an opiate required for the purpose of checking diarrhœa.

In two of the four patients seen during the first week of the disease, the bowels were confined till an aperient was administered; and in neither of the other two did the number of stools passed in any one day exceed two.

Of ten patients seen during the second week who took no aperient, four only passed more than two stools on any one day; but one of these two had severe diarrhœa, passing from four to six watery stools daily.

Of nine patients under treatment during the third week, four required the use of aperient medicine, and in one only was there any diarrhœa.

Two of eleven patients admitted after the disease had lasted an uncertain time, required the use of aperients, and in one case only of the eleven was diarrhœa present; but in this case it was very severe, from two to six stools being evacuated daily for nearly a fortnight.

The consistence of the stools was noted in seventeen cases: in two only were they watery, in eight they were pultaceous, in seven solid. No note was made of their consistence in twenty-six cases; but in fourteen of these twenty-six aperients were required: so that in these the stools were at least as solid as natural, or scanty in quantity.

Hemorrhage from the bowels did not occur in a single case.[1]

APPETITE AND THIRST.

Typhoid and Typhus Fevers.—Loss of appetite was one of the earliest symptoms in both diseases, and continued till their termination. There was more or less thirst in every case of the two affections, but my notes do not enable me to state the relative quantities of fluids drank by the two groups of patients, though the point is one of interest, with reference especially to the large quantity of urine passed in many cases of typhus fever, compared with the amount of

[1] My friend, Dr Parkes, is at present engaged in a chemical analysis of the stools in typhoid and typhus fevers, and his results will appear in a future Number of this Journal.

the same excretion in typhoid fever, even though deduction be made for the loss of fluid by purging in the latter.

THE PULSE.

Typhoid Fever.—The frequency of the pulse varied very greatly in different cases during the same period of the disease. During the first week it ranged from 110 to 132; during the second, from 80 to 128; during the third from 60 to 160; and during the fourth from 96 to too rapid to count.

Its frequency varied much from day to day in the same patient. To show these variations, I have arranged in the following table the number of its beats per minute on particular days in four cases; these variations in its frequency were, so far as could be ascertained, independent of increase or diminution in the severity of local complications: [1]—

Day of Disease.	Beats of Pulse.	Day of Disease.	Beats of Pulse.	Day of Disease.	Beats of Pulse.	Day of Disease.	Beats of Pulse.
13th	120	18th	76	5th	132	16th	108
14th	100	19th	96	7th	110	17th	108
15th	120	20th	78	8th	120	18th	96
16th	100	23rd	96	9th	102	21st	96
18th	110	26th	108	10th	120	23rd	100
20th	96	27th	96			25th	100
22nd	120	28th	108			28th	90

In a man, who came under observation on the 15th day of the disease, and whose pulse was at that time 120, by the 20th day it had fallen to 96, and by the 21st day to 60; it then gradually rose again till it reached 120 by the 30th day; the fall in the pulse was in this case accompanied by profound coma, which disappeared as the pulse rose again.

The pulse in one case, which was first seen on the 11th day, and proved fatal on the 25th from hemorrhage from the bowels, gradually rose from 80 to 96, and never exceeded the latter number in the frequency of its beats. The skin in this case was cool, and the mind unaffected, throughout the disease.

The pulse of one patient admitted delirious on the 5th day, and who continued so till his death on the 17th day, varied from 102 to too rapid to count.

In another case—that of a female, who, on the day of her admission (the 12th day of the disease), was violently delirious, and the following day deeply somnolent—the pulse, while she was delirious, was 120; the first day of somnolence, 120; the second, 144, gra-

[1] For the particulars of these, as well as other cases of the diseases, I may refer to a series of papers now publishing in the *Medical Times*.

dually rose to 160; and was too rapid to count on the day of her death, although the somnolence still continued profound, and was uninterrupted by delirium.

The pulse was never hard or bounding; it was occasionally full and soft, but usually small and weak.

It was irregular in two of twenty-one cases. In one of the two, which proved fatal on the 12th day of the disease, the pulse was on the 5th day very weak but regular; on the 9th and 10th days it was 130, very weak and irregular in frequency and force; on the 11th day it was too rapid to count. After death, in addition to the intestinal ulceration common to all these cases, there was found a form of grey hepatization of the lung. The substance of the right ventricle of the heart was flabby—of the left, firm and healthy.

In another case the pulse on the 16th day of the disease was 130, small, weak, and rather irregular; on the 17th it was 160, and markedly irregular; and on the following day it was too rapid to count. The whole heart, eleven hours after death, was flabby and soft.

Typhus Fever.—In two patients, who were seen respectively on the 6th and 7th day of the disease, the pulse was 80 and 96 in the minute; in the former it rose on the following day to 120. During the second week it ranged, with the exceptions about to be described, from 100 to 150; and, with those exceptions, in one case only was it as low as 100, and that was seen on the 8th day of the disease—its usual range was from 108 to 120.

Omitting for the present six cases, in which the pulse was at some period of the disease very irregular in force and frequency, and even sometimes intermitting, the cases that recovered so far as the fever was concerned, and those cases that were only seen once, there remain twenty-four cases of which I have notes of the pulse, taken on separate days, the number of days varying from two to nine. In seventeen of these cases it either kept up at the rate it was beating when first observed, or rose till the patient's death. In three of the seven cases which deviated from this rule there was a slight effusion of blood into the cavity of the arachnoid, coincident with a fall in the pulse of six, of eight, and of forty beats in the minute.[1]

In two there was an unusual amount of congestion of the meninges and substance of the brain, with an effusion of serosity on the convex surfaces of the brain; and in one of these two the fornix was distinctly softened. In the sixth case the pulse, when the patient was first seen, was 90; during the night he had an attack of convulsions, and on the next morning it had risen to 96; but on the following day it had again fallen to 90: so that it is probable that

[1] In two of the cases in which the pulse continued to rise till the patient's death, there was meningeal apoplexy; in one of the two it is probable that the effusion of blood took place some days before death, in the other after the last note of her pulse was taken.

the rise in the pulse on the second day was owing to the effec t of the muscular exertion during the convulsions not having passed off at the time I saw him. The seventh—the only real exception to the rule—was an old woman seventy years of age, seen for the first time on the 6th day of the disease, when her pulse was 80; on the following (the 7th) day it was 120; and on the 8th—*i. e.*, during the last twenty-four hours of life—it fell to 110.

The pulse in six cases was irregular, or irregular and intermitting, when the first notes of its state were taken. In two of these cases it was respectively 80 and 90, and very irregular; in both cases it rose the following day about 20 beats, finally attained a rate of 140 to 160; and became from the time of its sudden rise regular in force and frequency. In another case it was, when the patient was first seen, 100—irregular and intermitting; on the next day it rose to 120, and was then regular, and free from intermissions. In a fourth case, admitted on the 8th day, the pulse rose during the four following days from 96 to 108, and continued regular in force and frequency; on the 13th day it rose to 120, and became irregular; it was intermitting on the 15th day; and the patient died on the 17th day of the disease. In the remaining two cases the irregularity of the pulse was uninfluenced by the rise or fall in the rapidity of the heart's beats; one of the two had hypertrophy and dilatation of the heart, with some amount of aortic and mitral valvular disease; the heart of the other was loaded with fat, though in general appearance healthy. Of the three cases in which the rise in the pulse was accompanied by a change from irregular to regular, the hearts of two were perfectly healthy; that of the third was somewhat hypertrophied, and in it the mitral valve was very slightly diseased; the heart of the patient whose pulse became irregular as it rose in frequency was by some accident not examined.

Of the patients who were convalescent before the disease began which caused their death, the pulse in one was 120 on the 8th day, and fell by the 14th to 76; in another it was 120 on the 10th day, and by the 14th it had fallen to 84; while in a third, a very mild case, it reached its maximum, 96, on the ninth day, and by the 12th day was only 84.

From the earliest period of the disease that any of the forty-three patients came under my observation, the pulse was decidedly soft, gradually became weak, then very weak, and in many cases during the last few days of life imperceptible. Generally small, it was occasionally full, but still retained its extreme softness.

COUGH AND PHYSICAL CHEST SIGNS.[1]

Typhoid Fever.—Twelve of the twenty-three patients whose cases I am analyzing had cough. Sonorous rale was heard more or less

[1] I have, of course, omitted the physical signs of pneumonia, pleurisy, &c., as the different proportion in which they occurred in the two diseases we are

extensively in the chests of eleven of the twelve; in the majority, mixed with a little mucous ronchus, two expectorated a little colourless mucus. These rales were present when ten of the eleven were first examined on the 5th, 7th, 13th, 13th, 14th, 14th, 14th, 14th, 16th, and 28th day. In one case there was no sonorous rale till the 12th day, the patient being first seen on the 6th day.

Typhus Fever.—Twenty-one of the forty-three patients labouring under typhus fever had cough, generally slight in amount, and accompanied by little expectoration. Sonorous rale was present in seven of these cases; three had mucous rale more or less abundant, without sonorous; and in nine there was during life some want of resonance of the most depending part of the chest, *i. e.*, the portion corresponding to the most depending portion of the lung, the patient being in the recumbent position, and on his back; this region does not, it will be observed, include the extreme base, the root or apex of the inferor lobe; the respiratory murmur at the same point was feeble and coarse, seemed muffled or veiled, and was accompanied with either a few mucous clicks or sub-mucous rale. After death these physical signs were found to be accompanied by an intensely congested condition of the corresponding pulmonary tissue, occasionally passing into absolute consolidation. Doubtless the same physical signs were present in many other cases, but from the state of the patients very many were unable to be raised in bed sufficiently for the posterior parts of their chest to be examined.

SLOUGHING.

Typhoid Fever.—Sloughs formed over the sacrum in two cases; in one toward the termination of the second, and in the other at the end of the fourth week. In a third case, the left lower extremity as high as the knee, sloughed. That it was not owing to pressure, was proved by the discoloration first showing itself on the dorsum of the foot, and on the anterior as soon as on the posterior part of the ankle. The discoloration commenced on the 24th day of the disease.

Typhus Fever.—An oval portion sloughed out of the cornea of both eyes in one patient, and of one eye in another; in the latter case there had been opacity of the cornea for some years before the patient's admission into the hospital.

In one case, on the 11th day, the dorsum and ankle of either foot assumed a purplish hue, and appeared mottled, exactly resembling the corresponding part in the case of typhoid fever, in which the lower extremities eventually sloughed; this patient died the day after the discoloration showed itself.

In three cases sloughs formed over the lower part of the back, in one of these a second slough formed on the shoulder, and in another

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considering will be *demonstrated* by the lesions found after death. I need scarcely remind the reader that I am here speaking only of cases that proved fatal, and were examined after death.

on the occiput, shoulder, malleoli, trochanters, condyles of the femur, angles of ribs, and in fact in the progress of the case, on every part exposed to the slightest pressure. The sloughs in these cases began to form on from the 13th to the 16th day of the disease.

ERYSIPELAS.

Typhoid Fever.—Erysipelas occurred in seven of the twenty-three cases. In four of the seven it was very trifling in amount and extent, in none spreading beyond the upper lip and nose. In a fifth case it was by no means severe, and was limited to one side of the head. In these five cases it commenced respectively from the 12th to the 34th day of the disease. In two others it made its appearance after the fifth week, dating from the outset of the fever. In these two cases it affected both the head and face, and caused the death of the patients.

Typhus.—In two cases only was erysipelas a complication. In both it was very severe, and confined to the head and face; in one of the two it commenced on the 18th day of the disease; in the other at an uncertain date, but certainly near the termination of the fever.

Of the nine cases of erysipelas above referred to, seven were females.—(*To be continued.*)

ARTICLE V.—*Observations on some Alleged Signs of Incipient Phthisis.* By THEOPHILUS THOMPSON, M.D., F.R.S., Physician to the Hospital for Consumption, &c.

(*Read to the Harveian Society of London,* May 3, 1849.)

THOSE who are familiar with the progress of pulmonary consumption, will be ready to acknowledge that it is at the outset of the disease we may expect most encouragement in applying the resources of our art. The earliest indications of the malady have therefore a special claim on our careful study, and there are two signs appreciable to the ear, which have, on this account, particularly engaged my attention. One of these signs is an increased duration of the sound heard during expiration as compared with that of inspiration. Following the investigations of Jackson and Fournet, I have been led to the conclusion that prolonged expiratory murmur is frequently the earliest indication of phthisis. When the prolongation is considerable, and chiefly or exclusively on one side, it implies obstruction, and with certain restrictions, such as the absence of emphysema and bronchitis, may often be considered decisive. It is not intended, however, on the present occasion, to expatiate on this sign. The symptom which it is the principal object of this communication to illustrate is a particular modification of the inspiratory sound, designated "Inspiration Entrecoupée," by Laennec; "Inspiration Saccadée," by Fournet. By some English writers, it is called "jerking inspiration;" a term liable to objection, since it con-

veys to the mind the idea of the spasmodic jerk of asthma rather than of the delicate and uniform peculiarity under consideration. It is difficult to describe the phenomenon in words. To some it might seem appropriate to compare it to the movement of a cogwheel—two or three cogs advancing in each act of inspiration. To others the idea is better conveyed by representing inspiration as occurring by a succession of waves rather than a continuous current. The inspiratory sound, instead of being equably sustained from the commencement to the termination, is divided into several parts; being sometimes more feeble than natural, at other times rather harsh, but not necessarily altered in character. Having, during the last few years, recorded some of the chief particulars regarding more than a hundred patients in whom this peculiarity was observed, it seemed desirable, dismissing from the mind all bias to any preconceived explanation, to make a short analysis of those cases in the hope that it may assist us to measure the importance, and determine the significancy, of the sign.

Barth and Roger entertain the opinion, in some degree supported by Dr Walshe, that this peculiarity of inspiration is dependent on obstruction to respiration, occasioned by pleuritic adhesion. I have certainly from time to time observed interrupted inspiration associated with indications of pleurisy, but it is my conviction (irrespective of certain distinctive qualities of the sound, when thus produced) that pleurisy is by no means the most common cause. In six of the cases examined with a view to this communication, there was reason to suspect pleurisy. In all of these six the sound was more distinctly developed by pressure—(a circumstance not generally observed in the other instances). It occurred twice on the right side, twice on the left, once in both sides, and in the sixth instance, after disappearing from the right side, where it was first observed, became audible on the left.

There is another occasional cause of interrupted inspiration, of the adequacy of which I am convinced, by repeated observation, but which, as far as I am aware, has not hitherto attracted attention. In March 1848, I saw with Mr Headland of Guilford Street a young gentleman of somewhat delicate appearance, suffering from slight bronchial affection, occasioned by exposure to damp. After the bronchial disorder was relieved, I remarked the interrupted character of inspiration, and was, in consequence, rather apprehensive of pulmonary mischief. The patient's heart seemed irritable, and Mr Headland suggested the inquiry, whether its action, although unaccompanied by murmur, was not rather indicative of rheumatic diathesis. The progress of the case confirmed the accuracy of this suspicion. Various parts of the chest were in succession affected with pain, and the cog inspiration accurately accompanied the pain in every change of situation. In a few days a gouty blush manifested itself on the right foot, and the pectoral symptoms disappeared. It may be interesting to mention that this patient was similarly affected

in the spring of the present year, and that the symptoms were in a few days removed under the administration of lemon juice. Since this case occurred, I have met with about ten other instances of interrupted inspiration, associated with rheumatic symptoms, and which have for the most part yielded to appropriate treatment. I have also occasionally observed the symptom in patients affected with bronchitis, unaccompanied with much rhonchus, and in whom it has been transient. The existence of these varieties suggests an explanation of the opinion expressed by Dr Stokes, that the "Inspiration entrecoupée" may be removed by treatment. These modifications are distinguishable from the common form to which this communication has particular reference. The wavy inspiration accompanying rheumatic affection, is rather high in tone, more widely diffused, and more variable in its situation; whilst the bronchial variety may probably be distinguished by the presence of rhonchi, and by other characteristic symptoms of that disease. Interrupted inspiration, however, according to my observations, in a great majority of instances, has no necessary relation to either of these conditions. I have recorded particulars in a hundred and five instances. Seventy-two were men, thirty-three women. The number of the former decidedly exceeding the proportion of male to female consumptive patients, whose cases I selected with a view to these observations. The sound was observed far more frequently on the left than the right side; occurring on the right side only in ten, on both sides in three, of the seventy-two men. Four times on the right, once in both, amongst thirty-three women. This very marked preference of the left side is obviously one objection to any theory which would suggest a necessary connection of the symptom, either with pleurisy or consumption. The usual situation of the sound was near the apex of the lung in front, but in a few instances I observed it at the posterior part, where indeed it may in some other cases have existed, but escaped observation. As regards age, it may be mentioned that the youngest patient was a little girl of about six years. The eldest, if a rheumatic woman, aged sixty-eight, be excepted, was a man aged fifty-five.

The first important inquiry respects the relation of the symptom to phthisis. I have sometimes observed it at one part of the chest when pectoriloquy or cavernous respiration could be elsewhere detected; or when other indications, local or general, of advanced consumption, have been present. In a greater number of instances the wavy inspiration has ceased on the occurrence of bronchial respiration, dullness on percussion, dry crackling, or other more or less distinct evidences of the establishment of phthisis; but in a still greater proportion of cases it has continued for a considerable period to be the only important deviation from the natural state. Often, indeed, I have watched it for years without detecting any evidence of transition to serious disease. Of 105 cases, when first observed, thirty-two afforded grounds for suspecting tubercular disease, such for example, as dullness on percussion, or prolonged expiratory murmur. In twenty-two,

hæmoptysis had occurred. In three, a murmur could be heard over the pulmonary artery. Of the remainder many were not affected even with cough, and complained chiefly of delicate health, easy fatigue, and sometimes of slight oppression of respiration, as if, to use the words of a patient, "breathing through thin cambric." It is therefore reasonable to conclude, that if the interrupted inspiration be an indication of tubercular disease, it is the earliest local sign with which we are acquainted. But the question naturally occurs, can this symptom depend on mere functional disturbance, and disappear, leaving the subject of it in apparent health. To this inquiry I have devoted some consideration, and must acknowledge, that although interrupted inspiration, when dependent on pleurisy, bronchial affection or rheumatism may disappear, I have not satisfied myself of its removal (unless when superseded by other signs) in any instance unconnected with any of these conditions. To what cause, then, can the production of so persistent a phenomenon be referred? Its persistency points to some organic change, whilst its frequently stationary character encourages the belief that the change is not necessarily tubercular. The sensation conveyed to the ear is that of bronchial obstruction, yielding in gradual intervals to the admission of air. Sufficient opportunities have not occurred for conclusively testing any hypothesis, but the explanation which seems to me best calculated to meet the conditions, would assume the existence of an exudation less albuminous than tubercle, less fibrinous than the product of common inflammation, about the walls of the small bronchial tubes, and the interstices of the pulmonary substance, impairing the elasticity of the surrounding structure. Such morbid product of low inflammation may sometimes be sufficiently plastic to become organised, and to constitute hypertrophy of the cells and lobules, without involving any serious detriment to the general health; or it may become associated with the germs of tubercular deposit. When, indeed, distinct manifestations of phthisis once appear in such subjects, the disease advances with remarkable rapidity. I would remark, as tending indirectly to confirm the hypothesis just offered (although the numbers may be insufficient to authorise a positive conclusion), that, comparing the patients thus affected, with those suffering from undoubted phthisis, I am struck with the large proportion of those engaged in the open air, such as gardeners, policemen, coachmen, sailors, and toll-collectors. If, for example, persons engaged in the open air constituted a third only of my consumptive patients, they constitute one-half of those presenting the "Inspiration Saccadée." I have not made spirometer observations to any great extent in this selection of cases, but as far as they were made, the vital capacity proved rather above the average which I have observed in the first stage of consumption. The more frequent occurrence of the sign on the left side is worthy of notice, and the cause not easy to assign. If a morbid deposit be engaged in its production, it would seem that such deposit must be

most apt to occur on that side of the chest; but the more direct course of the right bronchial tube, and the excess of diameter of the three branches on the right side over that of the two branches on the left, by facilitating the ingress of air, may render a greater amount of obstruction necessary to establish an interrupted murmur on the right side.

Amongst the conclusions apparently deducible from the above data, may be mentioned—1st, That interrupted inspiration may be observed in bronchitis, pleuritis, and rheumatism, but that the most common variety is unconnected with those affections; 2d, That the variety most frequently observed (and which may be distinguished from those just specified) may occur at various parts of the chest, but far most commonly at the upper part of the left lung; 3d, That this variety rarely disappears, except when superseded by signs of phthisis, but that it often continues for many years without the supervention of that disease; 4th, That when phthisis does succeed, there is reason to apprehend great rapidity in its progress.

Assuming these views to be correct, this symptom cannot be regarded as conclusive evidence of tubercular disease, but it does not the less retain its interest and value. In reference to diagnosis, it is gratifying to me to be able to assure many patients thus affected, that there is no evidence of tubercular disease, and that, under favourable circumstances, we may hope to avert any more serious impairment of health. Such patients are never robust, generally delicate in appearance, and easily fatigued. Cough is not necessarily present, and rarely severe. There may be some hurry of breathing on exertion, and occasional uneasiness of chest; but symptoms calculated to attract attention to the lungs being often absent, it is expedient in all obscure cases of impaired health in patients whose countenances are expressive of deficient or imperfectly ærated blood, to make the question regarding the existence of interrupted inspiration a special subject of investigation. Such individuals may be considered on the verge of phthisis, and every appropriate measure should be adopted in order to prevent their crossing the threshold.

Depressing mental emotions should be in the utmost possible degree avoided. Careful attention to the promotion of healthy nutrition is of indisputable importance, and it is essential to correct hepatic congestion. Iodide of potassium may be advantageously employed, and if there be an anæmic and debilitated condition, iodide of iron. Counter-irritation is occasionally useful.

Above all, it is essential so to adjust clothing and select climate, as to secure, without risk of unseasonable exposure, much easy exercise in the open air. Such appear to be the leading principles of treatment; the details must be regulated by the particular modifications of the patient's constitution, and the judgment of the practitioner.

Part Second.

REVIEWS.

Lectures on the Diseases of Infancy and Childhood. By CHARLES WEST, M.D., Fellow of the Royal College of Physicians; Senior Physician to the Royal Infirmary for Children; Physician Accoucheur to the Middlesex Hospital; and Lecturer on Midwifery at Saint Bartholomew's Hospital. London. 1848. 8vo. Pp. 488.

THIS is an excellent work; and we should have been sorry had it proved otherwise, as the subject is one of great importance, and one which we think has been much neglected in this country. Germany and France have had the advantage over us in this department of medical literature; but now we are happy in adding to the treatises of Underwood and his commentators, and of Maunsell and Evanson, —truly meritorious works of their kind—the lectures of Dr West. There are, no doubt, a number of valuable dissertations in our own language on particular diseases, or classes of disease, to which childhood is liable; but it has often surprised us that there were not more cultivators of this rich and interesting field—this *nursery* garden of science—to bring before the profession the fruits of their experience in a complete and well arranged form. Why should the tender sapling, amid the perils of its new existence and rapid development, not receive even as much care and attention as the plant of hardier growth, which has surmounted the chief dangers of life, and has arrived at perfect strength, or which, having passed its maturity, is about to wither and die? Ought we not rather to expect that many generous minds would be found directing the less experienced how to detect the seeds of disease, how to protect from early blight, and how, by imparting healthful tone and vigour to the young life, to preserve it unimpaired to a green old age. In obstetric treatises there is a show of attention bestowed upon the complaints of infancy, but in fact we are presented with little better than a catalogue of "the ills which flesh is heir to"—of which the greater number are congenital defects, and affections of a very evident, generally of a surgical, nature. Thus, too, in our lectureships on midwifery, though understood to comprehend teachings on the diseases of children, the first division of the course, pregnancy and parturition, is necessarily so extensive, that in almost every instance we believe the session is nearly brought to a close before the second branch of the subject is entered upon. Hence it too frequently occurs in practice, that while the mothers receive all due care and attention, the infants, on being placed in the nurse's lap, are left to shift for themselves, the medical

attendants being nearly ignorant of the provisions which should be made against the numerous dangers to which their tender lives are immediately to be exposed.

The benefits which science and humanity are denied by the want of proper clinical instruction on the diseases of childhood, are indeed incalculable. In the dispensary at which Dr West chiefly obtained his experience, and to the pupils attending which he delivered the course of lectures now under review, such instruction appears to be given—we doubt not, profitably; and in other out-door charities something of the kind may also be attempted; but a dispensary is not the place for properly accomplishing that end, because children are seldom brought to it when labouring under acute disease, and in fact in any form of illness regularity of attendance is rarely secured. It is obvious, therefore, that the pupils of such institutions can obtain a very imperfect knowledge only of morbid phenomena, the effects of treatment, and post-mortem appearances. It appears to us that hospital accommodation for children, connected with our principal medical schools, will alone afford the requisite opportunities for the clinical study of infantile diseases. We are strongly of opinion that such advantages will never be secured for medical science, until government, in imitation of our continental neighbours, endows sick charities with a liberal, though discriminating hand, not leaving them to struggle on, supported only by the uncertain droppings of private benevolence. Few, we imagine, would quarrel with this judicious application of the public funds, whether raised by a general or local assessment, or a divisional arrangement of both, according to the interests of district or immediate vicinity. While such an arrangement would greatly promote the interests of science, it would certainly attain the most important sanitary ends, keep multitudes from falling as burdens on the poors' roll, and effectually draw from the large majority of the upper and middle classes a due share of contributions towards the maintenance of these humane institutions, which at present devolve entirely upon the benevolent minority of the community.

But to return to our subject. With the disadvantages above noticed, it is not surprising that the physician generally commences practice with a mere smattering of knowledge regarding the ailments of infancy, and that his experience is to be acquired under many discouragements; with doubts, difficulties, and annoyances to himself, and in all likelihood at the expense of no small sacrifice of human life or health. Dr West very truly remarks, of the instruction required by the student of medicine in the diseases of children, that "it is not mere hyperbole to say that you have to study a new semeiology, to learn a new pathology, and new therapeutics,"—P. 2. Many, probably from the difficulties which obstruct the way, adopt a routine practice, and give themselves up to the guidance of chance for the recovery of the little sufferers, or are brought to a complete standstill, and look on bewildered with folded hands while disease is run-

ning its rapid and fatal course. How often do we hear it said of such a one, "he is an experienced physician, but I do not think he understands the complaints of children!" This is doubtless often true. A double portion of tact and acuteness are required for the investigation and interpretation of signs and symptoms in those who can so ill, or not at all, express their own feelings and wants, and whose organism is so delicate, and so rapid in its development and changes; and a double portion of caution and consideration is required in accommodating remedial measures to the peculiar physiological condition of infant life, with its many tender sympathies. Some practitioners may be constitutionally disqualified for such a delicate task, for, besides the ordinary requirements of mind and gentle disposition, it seems to us that the love of children must form an indispensable trait in the character of the successful infant doctor. We would even go further, and affirm—without any disparagement of the acquirements and skill of our bachelor readers—that he who has children of his own, who can feel as a parent, who has, with a parent's interest, watched the phenomena attending the development of mind and body in the early months and years of existence, who understands the art of dealing with infantile impulse and caprice, and the stealthy tactics which are requisite to calm and soothe the childish temper when rendered especially irritable by sickness or pain—that such a one is most likely to detect the first approach of an insidious malady, and least likely to encounter difficulties in rescuing a little sufferer from impending danger. Thus it is that mothers and nurses who have no knowledge of animal textures and functions, are seldom wrong in forseeing a real danger, and generally anticipate the physician in a prognosis. Hence it is well, as Dr West properly observes, to give due attention and consideration to the opinions of her in whose lap the sick infant is lying, and whose affectionate eye is ever vigilantly on the watch.

In the *Introductory Lecture* Dr West remarks, first, on the difficulties usually encountered in the study of the diseases of childhood, and then he suggests the best modes of conducting the examination of sick children. These are penned in an excellent spirit—as, indeed, the book is throughout—and are highly judicious, worthy the attention of the most experienced, as well as the earnest study of the young practitioner:—

"The quiet manner and the gentle voice, which all who have been ill know how to value in their attendants, are especially needed when the patient is a child. Your first object must be not to alarm it; if you succeed in avoiding this danger, it will not be long before you acquire its confidence. Do not, therefore, on entering the room, go at once close up to the child, but sitting down sufficiently near to watch it, and yet so far off as not to attract its attention, put a few questions to its attendant. While doing this you may, without seeming to notice it, acquire a great deal of important information. You may observe the expression of the face; the character of the respiration, whether slow or frequent, regular or unequal; and, if the child utter any sound, you may attend to the character of its cry. All your observations must be made

without staring the child in the face: little children, especially if ill, seem always disturbed by this, and would be almost sure to cry. If the child be asleep at the time of your visit, your observations may be more minute: the kind of sleep should be noticed, whether quiet or disturbed; whether the eyes be perfectly closed during it, or partly open (as they are in many cases where the nervous system is disordered). You may, too, if the sleep seem sound, venture to count the frequency of the respiration and the beat of the pulse, but in doing this you should be careful not to arouse the child. It should be awakened gently by the nurse or mother, and a strange face should not be the first to meet its eye on awaking. If it were awake when you entered the room, it will probably in a few minutes have grown accustomed to your presence, and will allow you to touch its hand and feel its pulse. This must always be done at as early a period in your visit as possible, in order that you may count it while the child is undisturbed, since the pulsations of the heart vary in young children as much as twenty in a minute, under comparatively slight disturbing causes; and any inferences that you might draw from the pulse of the child, when frightened or excited, would almost certainly be erroneous. Besides the pulse, the frequency of the respiration should, if possible, be noticed, since the results obtained by a comparison of the two are always more valuable than those of either taken alone. But, if this be your first visit to the child, do not, for the sake of ascertaining either of these points exactly, persevere in attempts which irritate or frighten it; probably you would, after all, be unsuccessful, and, even though you were to succeed, the knowledge would not repay you for the loss of the child's confidence, which it must be your grand object to acquire and keep."—P. 3.

Some excellent general directions and practical hints follow as to the examination of the temperature of the body, the state of the skin, mental powers, gums, mouth, throat, chest, abdomen, &c.; for eliciting information from the attendants regarding hereditary tendencies, the history of symptoms; and for correct case-taking. We perfectly agree with Dr West in preferring the back of the chest in children for auscultation and percussion, as its upper and under parts will in general be found a correct index to the conditions in front. We also much prefer immediate auscultation, unless it be that we are anxious to examine in front of the chest, and the child is not frightened. In general, however, the stethoscope should be given up to the little one as a plaything for the time being.

In consequence of the arrangement adopted in the lectures, as well as the excellence of the matter and the freedom and perspicuity of the writing, Dr West has produced a book more readable, and likely to be much more practically useful, than the well-known treatise of MM. Rilliet and Barthez. The volumes of these authors are, indeed, of high value, and justly occupy a standard position in medical literature; but, from the effort made to follow out a strictly scientific classification, there is much needless repetition throughout the work; and, as every subject is treated monographically, the literary part being also somewhat prolix, the work, altogether, is heavy and tedious. The simple fact that the subject of tuberculisation extends over no fewer than 600 octavo pages of the third volume, is sufficient to indicate wearisomeness. Our author's treatise is also calculated to be more generally useful, as it comprehends the ailments of children prior to the age of fifteen months, as well as after

that period, thus including in its survey the most delicate, changeable, and precarious epoch of a child's existence. He has, therefore, taken the same extended view of infantile disease that Drs Maunsell and Evanson have done in their excellent, though now rather antiquated, manual.

The arrangement chosen by Dr West is partly regional and partly nosological. In his thirty-nine lectures he discusses successively *Diseases of the Brain and Nervous System, of the Respiratory Organs, of the Heart, of the Organs of Digestion and Assimilation*, and *of the Urinary Organs;* and then follow, as disjointed topics, *Syphilis* and *Fevers*. The plan is comprehensive and convenient; but we regret that he did not make some provision for the introduction of a few subjects which are not at all noticed. We refer especially to some *strumous affections*, such as are manifested in infancy in the form of *ophthalmia, swelling and degeneration of the external glands*, and *rachitis;* and also to *erysipelas, purpura*, and the *cutaneous affections*, so common in childhood. Rachitis, particularly, is a serious omission; and the meagre notice taken of the eczematous and impetiginous eruptions, which occur so often during the period of dentition, is unpardonable. In a new edition we hope to have a few additional lectures, embracing these subjects; and we would suggest to Dr West, that his work would be still more complete and satisfactory to the general practitioner—for there is no pure physician ever engaged in practice among children—if he would add a few pages (as we have no doubt his knowledge would enable him to do) on *hare-lip, umbilical hernia, hydrocele, spina bifida, club-foot*, and *nævi materni*.

The *Second Lecture* is principally occupied with statistics and general statements, showing the extreme frequency of diseases of the nervous centres in early life, and the symptoms most characteristic of them. The liability of children to these ailments is accounted for chiefly by the rapid development of the brain—doubling its weight in the first two years, and attaining nearly its maximum at the end of the seventh year; and also by the wide variations in the cerebral circulation leading to greater degrees of congestion and exhaustion than in adult life. The grand reason, too, assigned for the frequency of convulsions in childhood is the predominance of the spinal over the cerebral system during that epoch. The following short notices of the general symptoms of cerebral disturbance, besides being instructive, will serve to convey an idea of the felicitous manner in which Dr West usually describes morbid phenomena. His delineations are drawn in simple easy language; they cannot fail to be recognised by the experienced practitioner as graphic sketches, and to be attractive, memorable, and profitable, to the beginner:—

"The painful sensations which the infant experiences soon show themselves in the haggard, anxious, or oppressed look which takes the place of the naturally tranquil expression of its countenance. It often puts its hand to its head,

or beats or rubs it, or, while lying in its cot, bores with its occiput in its pillow, owing to which, in children who have suffered for any time from uneasy sensations in the head, you often find the hair worn quite off the occiput. It turns its head away from the light, and lies much with its eyes half-closed, in a state of apparent drowsiness, from which it often arouses with a start, and cries. The cry, especially in inflammatory disease, is peculiar; it is generally a low, almost constant moan, very sad to hear, interrupted occasionally by a sharp, piercing, lamentable cry, almost a shriek. If the child be young, it will often seem relieved by being carried about in its nurse's arms, and while she is moving will cease its wail for a time, but begin the moment she stands still. You will sometimes observe, too, that, if moved from one person's arms to those of another, or even if its position is but slightly altered, a sudden expression of alarm will pass across its features: the child is dizzy, and afraid of falling." —P. 14.

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"Something may be learned of the state of the mental powers and of the feelings, even in early infancy. Have you never watched an infant in its mother's lap, and noticed the look of happy recognition with which its eye meets that of its mother? An early result of cerebral disease is to interrupt this intercourse: the child now never seems to catch its mother's eye, but is sad and listless, as if all persons were alike indifferent to it; or at other times even familiar faces cause alarm, the child apparently not recognising those who yet have always tended it. This disturbance, however, is but momentary, and the child subsides into its former condition, and allows itself to be taken by those at whom a minute before it seemed frightened."—P. 15.

After noticing, in a similar strain, the indications of other symptoms—such as heat of the skin and head, flushings of the face, prominence of the fontanelle, fulness of the veins of the scalp, action of the carotids, the pulse and pupil, he goes on to speak of the valuable information to be derived from inquiring into the state of the digestive functions:—

"The bowels are almost always disturbed, usually, though not invariably, constipated, while nausea and vomiting are seldom absent. I am not acquainted with any one symptom which should so immediately direct your attention to the brain as the occurrence of causeless vomiting, and especially its continuance. At first, perhaps, the child vomits only when it has taken food; but before long the stomach will reject even the blandest fluid, and then the efforts at vomiting will come on when the stomach is empty, a little greenish fluid being rejected, with no relief, the retching and vomiting soon returning. I shall have occasion again to dwell upon the importance of this symptom, which I have known continue for several days before any other indication of cerebral disease could be discovered. In children of three or four years old, this occurrence would scarcely be overlooked; but the case is different with infants, who so often vomit the milk when ill, that the mother or nurse might fail to mention it to you, if you did not make special inquiries with reference to that point."—P. 16.

In the subsequent pages the importance to be attached to vomiting, as a sign in the early stage of cerebral disturbance, is again and again enforced; and no less so in certain circumstances is another class of symptoms, occasioned by respiratory sympathies, and evinced generally in unequal, irregular breathing; a short, hard, hacking cough; and croupal inspirations. The indications afforded by the

occurrence of convulsions are then pointed out, and these we recommend to careful perusal.

The next nine lectures are devoted to a detailed consideration of the various diseases, organic and functional, of the brain and nervous system. Our limited space prevents us from following Dr West through these interesting and important prelections, or attempting anything like an analysis of the statements and opinions advanced; we can only notice a topic here and there which seems to us of peculiar interest or novelty, or demanding criticism.

Congestion of the Brain. Lecture III.—This is considered by Dr West as occurring in two forms, *active* and *passive:* the former when the flow of blood to the head is increased, and the latter when its reflux is impeded. The head symptoms, which frequently usher in eruptive fevers, which attend dentition, exposure to the heat of the sun, and blows on the head, are examples of the first condition of the circulation; and the convulsions occurring in fits of hooping cough, in the laryngismus stridulus, the pressure of hypertrophied thymus or tuberculated bronchial glands, or resulting from a languid circulation, or the want of pure air or nourishing food, are examples of the second. To a variety of the latter form of congestion is also referred the condition of children who have been debilitated by protracted illness, and in whom, as Dr West remarks, "the streams are stagnated, because the vital powers are too feeble to keep them in motion." In the exposition of the above statements there is not the same clearness and persuasiveness which we find in other parts of the work. This seems to have arisen from the attempt made to treat what is here assumed to be congestion of the brain as a special affection, to mark out its stages, and to indicate the treatment suited to the various forms in which it may occur. We do not for a moment question the frequent occurrence of this condition of the circulation. We believe that it may take place in the three following ways—first, by a determination of blood to the head, and consequent fulness or plethora of the arteries; second, by a mechanical arrest in the veins merely; and third, by an engorgement of the capillaries. With due deference, however, to the opinions of Dr West and others who speak confidently on this subject, we suspect that much remains yet to be proved regarding it. We really know little regarding the laws which produce, or the phenomena which characterise, congestion of the brain. We are sure that medical men are continually making errors in diagnosis regarding this condition of the circulation, supposing that it exists where there is perhaps only functional disturbance on the one hand, or when inflammation is fairly established, or its products already formed, on the other. Congestion undoubtedly is often as much an effect of morbid action, as a cause of disease; and there can be no doubt that it very frequently gets the credit of manifesting itself in certain positive symptoms during life, which cannot be proved after death. The same symptoms are observed

under the most opposite circumstances, and anatomical appearances are still more contradictory and uncertain. Intellect, sensation, and motor power have been frequently much affected, while nothing unusual could be discovered in the encephalon; and, on the other hand, the cerebral vessels are often found excessively engorged with blood, when no symptom had been present during life to indicate that the functions of the brain were in any way implicated. Every one who has had much experience in post-mortem examinations must have observed that, whatever has been the cause of death, the cerebral vessels are often full of blood under the most opposite conditions of the system, whether considered with relation to symptoms or to the other post-mortem appearances. In pathology, therefore, the term congestion of the brain has no definite signification, and is often applied in as valueless and as unsatisfactory a way towards the exposition of morbid phenomena as the phrase "atmospherical influence," in common parlance, is employed to explain whatever cannot be otherwise conveniently accounted for. It has yet to be understood, and to be intelligibly connected with rational signs; and we think Dr West should have accompanied the statements in this lecture with some caution as to their application, or have reserved them for his descriptions of apoplexy, the early stage of the inflammatory affection of the brain, and the accidents which occur in the course of various other diseases. Suppressing the term "congestion" altogether, as being, in the present state of science, inapplicable to the phenomena he describes, he might thus have avoided entangling himself in pathological difficulties and vague generalities, and thus holding out uncertain lights for the guidance of the yet inexperienced practitioner.

Cerebral Hemorrhage or Apoplexy. Lecture IV.—The truly excellent observations made on this subject owe much of their value to the researches of MM. Rilliet, and Barthez, and M. Legendre, which Dr West very properly acknowledges. Effusion of blood into the substance of the brain appears to be very rare in children, but into the cavity of the arachnoid, by no means uncommon. It does not proceed from any large vessel, but is capillary; it occurs chiefly in weakly children, and is often associated with tubercle of the brain; and the symptoms are obscure, for paralysis, which would mark an effusion in the adult, is no sign in children, and coma and convulsions, which are always present, may originate from other pathological conditions. The suddenness of convulsions, and their frequent recurrence alternated with spasmodic contractions of the fingers and toes, seem to be the most frequent indications of this affection; and, under careful treatment, regulated by the general principles applicable to cases of vascular determination to the head, recovery is occasionally brought about.

The *asphyxia of new-born infants*, is here noticed as often resulting from cerebral hemorrhage. As Dr West, however, does not give a very clear account of the different ways in which suspended anima-

tion occurs, we shall endeavour to supply the defect. The greater number of children born asphyxiated or comatose, are not so from hemorrhage, but simply from cerebral plethora andd iminished nervous power, owing to the long continued impaction and pressure to which the head has been subjected in the last stage of labour, to which may be added, perhaps, the mechanical injury inflicted by the compression of the forceps. A deep stupor is the result; and the nervous system is so oppressed as to be roused with difficulty to perform the first respiratory effort which is so essential to the life of the child, now that the course of the circulation is changed. This embarrassment of the circulation, so far as the head is concerned, is sometimes characterised by excitement—as is indicated by the turgid face, the heat and redness of surface, and the strong pulsation of the heart and funis; sometimes by depression, as is conjectured from the paleness or lividity of the face, the relaxed jaws, and the feeble pulsations of the heart and cord. Very often, too, the suspended animation, presenting such appearances as those described, is owing to the constriction of the funis, twisted tightly round the neck. This must, no doubt, in some circumstances—more especially when the head is born long before the body—obstruct the current of the circulation through the jugulars; but the chief injury occurs from pressure on the cord itself, occasioning a stoppage of the circulation through it. We must also not forget to mention, as another cause of suspended animation, the loss of blood which the mother may have sustained.

When there is manifest overloading of the vessels of the brain at birth, after a little blood has been allowed to flow from the cord, Dr West recommends the hot bath, and at the same time an occasional dash of cold water on the face or chest, in order to incite the respiratory muscles to action. We have found by ample experience, that to establish respiration, no plan answers so well as that recommended by Dr Simpson, namely, a sudden plunge of the lower part of the body alternately into baths of hot and cold water. After a few repetitions of these immersions, quick and deep inspirations are produced, and there is generally no occasion for artificial respiration, or any of the other very proper means referred to by Dr West. When the respiration is fairly established, and the child breathes freely and equally in less than half-an-hour, and cries intelligently—for the sound of voice is quite peculiar when the brain is oppressed—there is commonly no more occasion for treatment or anxiety, for the circulation flows on in the proper channels; but when the child becomes weaker and weaker, and the respiratory acts occur only at long intervals of time, accompanied with a sudden convulsive twitch, and an occasional low groan, or when general convulsions come on, then we have every reason to fear that sanguineous effusion has actually taken place, and that recovery is almost hopeless.

The phenomena attending the formation and disappearance of the bloody scalp tumour—*Cephalhæmatoma*, which results from pressure

during parturition, are well stated. Dr West published a paper on this subject in vol. xxviii. of the Medico-Chirurgical Society Transactions. Having expressed confidence in the restorative power of nature, it is a pity that he should have yielded any sanction to the meddlesome practice of applying lotions to the tumour, or, after the lapse of a few days, puncturing and poulticing it. In our judgment, implicit trust may be placed in the kindly aid of nature. We never interfere with these tumours; and they always disappear in a short time.

Lectures V., VI., VII., and VIII., contain a most masterly view of the various *Inflammatory Affections of the Brain*—more especially of *Hydrocephalus*. We have never met with anything on the subject, in all respects so good; and nothing better could be desired. Trusting that our readers will peruse every paragraph of this part of the work, we shall pass on to notice the ninth lecture.

Hypertrophy of the Brain appears to be met with in rickety infants, living in districts where goitre and cretinism prevail, and among the ill-fed inhabitants of narrow lanes in crowded cities. So far as we are aware, this affection of the cerebral substance is almost unknown in Scotland, and we therefore refer to Dr West's account of its symptoms, its diagnosis from hydrocephalus—which it resembles in some respects, its morbid changes, &c. An opposite state of the organ is however sometimes met with, in which it appears to be reduced much below the normal size. The *Atrophy of the Brain* here described, is not that congenital defect which constitutes idiocy, but a subsequent change. It appears to occur under two circumstances, premature closing of the fontanelles and sutures, and wasting of cerebral matter during long illness. Intellect, sensation, and motor power seem annihilated, and all remedial measures are in vain. When wasting has taken place, the scalp is bloodless, fontanelles collapsed, and the process of ossification tardy or altogether arrested. Fluid is found in the cavity of the arachnoid, in the ventricles, and at the base of the brain. The sulci of the convolutions are unusually deep, the entire cerebral substance does not fill up the cranial cavity, and it is pale and of abnormal firmness. The only practical advantage of knowing these facts is to enable us to distinguish a class of cases in which fever or other severe ailment enfeebles the brain, and for a while renders it incapable of performing its ordinary functions. In such a condition it would be a serious error to relax any efforts in the use of nourishing and tonic treatment, from an impression that cure was hopeless. Dr West says:—

"It is only in infants that accidents of this grave nature are likely to ensue from the imperfect nutrition of the brain consequent on protracted illness; but symptoms arise in older children, and in similar circumstances, well calculated to excite the apprehension of parents. In children who had but lately learned to talk, I have sometimes known loss of speech follow a long illness, the child being too weak to talk, just for the same reason as it is too weak to walk. Occasionally, however, the child apparently regains its previous health, and yet makes no effort to articulate even for two or three months. In cases of this kind I have

seen parents thrown into great anxiety from the fear lest the child's continued silence should be the result of the intellect having become impaired during its illness. I imagine that in many of these cases the child has forgotten, during its illness, much of its newly-acquired knowledge, and that it is some time before it again feels equal to the mental effort of shaping its ideas into words. Usually, however, when it begins to make the effort, it recovers its speech rapidly, and you may therefore console the parents with this prospect.

Even a manifest retrocession of the intellectual endowments should not be regarded with too much anxiety, when it has followed some long-continued disease, for it may be the result of mere weakness; the vacant look, the unmeaning laugh, and the silly manner, gradually disappearing as the child gains strength. The brain seems to regain its lower powers, and to perform its humbler functions, before it resumes its nobler office as the organ of the mind."

We have seen instances of this kind. One case occurred to us lately, in which a boy five years old, after an attack of fever, lost the power of speech, and the use of the upper extremities for nearly a month, and again became in all respects like an infant. When articulation began to return, the monosyllables of a child were at first employed, as if language had to be acquired anew. In a few days, however, rapid progress was made; and ultimately there was a complete restoration both of the intellectual and physical powers.

Lecture X. commences by the discussion of the affection known by the name of *Hydrocephaloid Disease*. Our own lamented Abercrombie noticed this affection,[1] but to Dr Marshall Hall belongs the merit of giving it a name, and drawing special attention to its nature. The most marked symptoms are—at first rapid but not powerful circulation, general irritability, nervous sensibility to light and sound, and startings and screamings in sleep; ere long such symptoms occur as pallor and coldness of face, and imperfect closure of the eye-lids; ultimately the pupils are unaffected by light, the respiration is quick and sighing, general sensibility and strength diminished, and coma, more or less complete, supervenes. These symptoms very strikingly simulate inflammatory disease of the brain; and hence its discrimination is of great importance, as cordials and nourishment may be required instead of lowering remedies, and the attention may be more needed to the organs or functions inducing the disease, than to the head. The affection often succeeds to sympathetic disturbance of the brain in the course of various affections. It supervenes on diarrhœa and pneumonia; and Dr West very properly calls attention to the fact that it may be produced by the over-treatment of the cerebral excitement which occurs in the course of various affections. He says:—

"Under no circumstances are mistakes more easily committed, and never are their results more mischievous, than when real congestion of the brain (?) has been somewhat over-treated, and the consequent symptoms of exhaustion are supposed to be those of advancing disease. In such a case, however, it would usually be observed, that great faintness had been induced by the first

[1] Edin. Med. and Surgical Journal, vol. xiv., p. 581.

depletion, and that the quiet which succeeded it was that of exhaustion as much as of mitigated suffering. If so, the returning restlessness would probably be the index of the feeble power of the brain, no longer adequate to the performance of its wonted functions, rather than the evidence of active disease of the organ. Nor would the history be the only safeguard from error, but the fontanelle, sunk below the level of the cranial bones, instead of being tense and pulsating, the cool surface, and the pulse presenting no other characters than those of frequency and feebleness, would all point to the real nature of the case. You do not need to be told that to deplete under such circumstances would be to destroy your patient—that food is needed, not physic. The sunken powers of life are to be rallied, and as their strength returns the functions of the brain will again go on harmoniously."—P. 108.

With the subject of *Tubercle*, Dr West closes his discussion of the affections more strictly connected with the brain. Of this not uncommon disease he gives an excellent pathological account, but does not furnish us with an easier diagnosis, or hold out better prospects of effecting a cure, than those who have preceded him.

(To be continued.)

A. P.

Elements of Chemistry, Theoretical and Practical. By Sir ROBERT KANE, M.D., M.R.I.A. Second Edition.

THE students of no science are better provided than those of chemistry with text-books, suitable for pupils at all stages of progress. So good, indeed, are the majority of the current treatises on chemistry, that a beginner may safely indulge his taste in selecting a text-book, without fear of purchasing a useless volume.

Sir R. Kane's work is deservedly a great favourite with students, and this not only in Great Britain but in the United States, where it has been generally adopted by the colleges as their text-book. He has avoided the error into which many authors of text-books fall, of making their volumes exhaustive discussions of the science they expound. Our authorities in chemistry have persisted longer in this mistake than the preceptors of any other department of knowledge. No naturalist insists on discussing, in an elementary book, every species of every genus, but deliberately and of set purpose discusses only the more important species of the more important genera. The chemist, however, still makes the vain attempt to include the rarest chemical compounds in his elementary treatises, and endeavours to comprehend in his discussion all the members of groups comprising substances exceedingly like each other in every property. Such a group, for example, we have in the compounds of Ethyle, or Ethers.

For the majority of students, a knowledge of the method of preparing and purifying some three or four of the ethers would suffice, and it would enable them at any time to extend their acquaintance with this class of bodies indefinitely. But it only distracts or disheartens the beginner, to present to him twelve or twenty compounds

as equally deserving his study, of which only five or six are so much as to be seen in university museums, and these only as sealed specimens, too precious to be spent on experiments. It is high time that our academical prelectors should either justify the titles of their works, which are styled "Elements of Chemistry," by making them elementary; or give them new titles more expressive of their true character. As it is, some of them have been commenced as Students' Manuals, and have perhaps been such in their earlier editions, but their later issues insensibly departing from their original scheme, and endeavouring to keep pace with the progress of a science which only periodicals frequently published can overtake, have ended in realising neither the idea of a good text-book nor that of a Cyclopedia or Dictionary of Chemistry. They are supplanted as text-books by such works as Professor Fownes' Manual; and as cyclopedias by such productions as "Gmelin's Hand-book."

Sir R. Kane's work is happily free from such faults as we have been condemning. Written for students, it avoids systematically all the minor details, and isolated or less interesting departments of chemistry. A broad, bold outline-sketch is given of all the great laws, doctrines, and problems of the science. The outline is filled in, more or less completely, as the object of the sketch is related to settled and important sections of physics, or belongs to still doubtful, purely theoretical, or merely curious, objects of chemical investigation. The style is graphic and clear, the illustrations well chosen, and the work, as all text-works for students should be, eclectic in its character, and discriminating in its judgment. Sir R. Kane, too, we need not tell our readers, is not a mere judicious compiler, or condenser of other men's thoughts and speculations. He is esteemed wherever chemistry is cultivated, as an original observer and independent thinker, whose criticisms on disputed questions in his science are always worth listening to, and often decisive of the merits of the case under discussion.

The progress of chemistry has been such, within the last ten years, that we cannot expect any single volume to prove sufficient for even the briefest consideration of its triumphs. Upon the whole, Sir R. Kane has judiciously followed the march of his science since the first edition of his work, although we cannot say that all the departments of chemistry are equally represented in his notices of new discoveries. We had marked several examples of what seemed to us disproportionate discussion of improvements in some branches of the science, and neglect of advances in others; we omit these, however, as we are more anxious to point out the great merits, than the few defects, of an excellent work. We notice, accordingly, only one of the latter. The first edition of Sir R. Kane's "Elements" contained by far the best account which had then appeared of crystallography, discussed in a mode suitable for general students. We do not find, however, that the second edition represents the present state of crystallography, or takes advantage of the changes in nomenclature, and alterations

of views, which have taken place within the last very few years. Sir R., *ex. gr.*, still describes the rhomboidal system as possessed of four axes, although this mode of representation, which has been styled by an eminent Cambridge professor "a geometrical absurdity," is now abandoned, and all the crystalline systems are represented as possessing each only three axes.

We willingly, however, resume commendation. We possess at present no work which can come into competition with the one we have been reviewing as an ample, judicious, and independent summary of the progress of chemistry during the last ten years. As such, we recommend it to all interested in that science who are still willing, whatever amount of progress they may have made in it, to be styled students.

A Practical Treatise on Morbus Coxarius. By WILLIAM C. HUGMAN, Surgeon to the Verral Institution for the Treatment of Spinal Disease and Distortion, &c. Pp. 81. London: 1849.

AFTER giving an outline of the symptoms and progress of this disease, and a rather interesting account of its pathology, the author, under the head of "Treatment," confines his observations to the mechanical management. The principal object of the treatise is the recommendation of the prone position for preventing the drawing up and shortening of the limb which occur in the advanced stages,—the method being an improvement on that "originally employed by the late Dr Verral in the treatment of vertebral disease and distortion." The author is of opinion, after having enjoyed "unusual opportunities" of making himself acquainted with the disease, that in its advanced stages, when "the position of the limb becomes one of the most important considerations in the treatment," all the plans of mechanical management hitherto proposed, including the use of the long splint, "have proved useless, or nearly so, from the simple fact that they can seldom be borne." This observation, however, does not apply to the use of the long splint in the early stage of the disease, by which perfect rest is secured, and drawing up of the limb prevented. The "prone couch" recommended by the author is a very simple one. It consists of a horizontal board, two feet in breadth, and of the length of the patient's trunk, supported in front at a height of from two to three feet, and behind by another and longer board which slopes gradually to the ground, on which it rests; and on these a hair mattress is laid. The horizontal part supports the trunk, the chest being slightly raised; the lower extremities rest on the depending part, a cushion being placed below the ankles; and the angle between the two planes corresponds to the front of the hip joints. In regard to the apparent awkwardness of this position, the author alludes to the fact that, in the advanced stage, there is always a

tendency to assume the prone position in bed, in order to take the pressure off the affected hip; and the apparent objection, that "the play of the ribs and abdominal and thoracic muscles is restricted," is met by the remark, that the author, after eight years' experience, has "never known it produce such effects, or it would long ago have been abandoned." On the contrary, it would appear, from the author's observations and cases, that after the position has been maintained for a few hours, it becomes an extremely easy one, and is much preferred to any other by the patients; who are described as having been able variously to engage in reading, drawing, sewing, and even in practising on the piano. The advantages attributed to this position, in addition to the relief it affords, are, that it does not admit of contraction or shortening of the limb taking place; the joint is kept at rest in a partially flexed condition, by which more especially the psoas and iliacus and rectus muscles are relaxed, and all pressure is removed from the affected part; and facility is afforded for the application of any topical remedy that may be required. This position has been also found very advantageous in cases where there was sloughing of the back or nates, from long-continued pressure on these parts in the supine position; and it will prove no doubt a useful expedient when there is a tendency to that occurrence in any case in which there has been long confinement to bed.

If the limb is already shortened, unless there is contra-indication from the condition of the affected parts, extension may be gradually made by a simple contrivance appended to the couch, and cases are given from the journals of the institution in which several inches of shortening—arising from pelvic obliquity, accompanied by corresponding spiral curvature—existed, in which the use of the extension apparatus restored, without pain or difficulty, the limb to its natural figure and length. Many such cases of distortion from this disease are said to apply at the Verral Institution, having been elsewhere informed that their cases admitted of no farther relief; of some of these a permanent cure has been effected, and a great number have been much relieved.

Mr Hugman has also given a translation of some observations and experiments recently made by several continental surgeons on this subject, of which those of Heine, on the reduction of the bone in several cases in which the disease had ceased, on the occurrence of spontaneous luxation, are more especially interesting; but for farther information, we must refer to the treatise itself, which is deserving of perusal, and is in every way creditable to the author.

A Manual of Botany; being an Introduction to the Study of the Structure, Physiology, and Classification of Plants. By JOHN HUTTON BALFOUR, M.D., F.R.S.E., &c., Professor of Botany in the University of Edinburgh. 12mo, pp. 641. London: 1849.

PROFESSOR Balfour is well known as a pre-eminently zealous and successful teacher of botany, and the work before us, which he has just published on that science, will not fail to confirm and extend the reputation which he has always possessed. It is a comprehensive manual of botany, giving a condensed view of all the departments of the science. It forms an introduction to the study of the structure, physiology, and classification of plants. Besides the usual subjects treated of in the introductions and manuals put into the hands of students, it embraces the application of vegetable physiology to agriculture, as regards the cultivation of plants and their diseases; the properties of plants in a medical and economical point of view; an account of the geographical distribution of plants; and a short sketch of fossil botany. We highly approve of our author's plan in thus presenting to the student, in one view, and in a simple volume, an outline of all the subjects which botany, taken in its largest sense, comprehends. We here speak chiefly with reference to the wants of the student of medicine. Botany is not one of the essential subjects of his studies. He must be allowed, when he has no particular turn towards botany, to content himself with the most general views of the science. We are far from thinking that botany should be excluded from the circle of medical studies; but we cannot admit that it should receive the same importance as anatomy, physiology, and chemistry. When we look to the history of botany during the last hundred years, we may remark that the most important additions to our knowledge of the flora of distant countries have been made by medical practitioners visiting those countries, though possessed of but slender attainments in botanical science. And while it is plainly impossible that any large proportion of medical men should be profoundly versed in botany, we cannot but insist that any discouragement in the medical profession of such slender attainments as are here referred to, would prove detrimental to the progress of botany, as far as concerns the knowledge of the vegetable productions of remote countries. We have made these remarks, because we think there has been a growing disposition of late among some of our most eminent authors of elementary treatises on botany, to repel from taking part in the cultivation of that subject, all who are not disposed to make it the chief business of their lives. We think Professor Balfour's work eminently calculated to counteract the influence of this exclusive spirit,—it is well fitted to be the first treatise put into the hands of the student, because if he has any turn for the study of nature at all—and no student of medicine should be destitute of such a faculty—he will be sure to find

some topic to interest his curiosity, even though he may not be able at once to summon up the resolution necessary to encounter the details of minute structure, or the puzzling grounds of affinity in natural families.

But our readers must not misunderstand us so far as to suppose that we wish to represent our author's work as a superficial treatise. On the contrary, it contains everywhere the most exact views on the several subjects of which it treats—those sanctioned by the highest and most recent authorities. And as the newest book on a subject advancing at present with unusual rapidity, it commands the signal advantage of giving the anatomical description of plants, hitherto so difficult a branch of the study, all the precision derivable from the latest important discoveries in structure and function. Every one conversant with the elementary works on botany, which have come into use within the last twenty or thirty years, must have remarked with delight how easy it becomes, as the knowledge of structure and function advances, to apprehend descriptions of parts which were before perplexing and almost unintelligible. And on this account we cannot but condemn the attempts so often made at the instigation of publishers, to retain in use the older text-books on elementary botany, by the help of new editions, furnished with notes and corrections. A book of this kind, composed at a time when extensively applicable principles of structure and function were imperfectly, or not at all, understood, cannot but embarrass the learner often in the most difficult part of his task, and is of no farther use but to supply materials for the history of the science.

We should add, that the work is abundantly illustrated by woodcuts, namely, those employed in Jussieu's "Cours Elementaire," some from Beudant's Geology, and others from Raspail, St Hilaire, Schleiden, Amici, and Maout. There is also an appendix, containing a short view of the use of the microscope in botanical researches, and directions for collecting and examining plants, and on the formation of a herbarium.

A Physician's Holiday, or a Month in Switzerland in the Summer of 1848. By J. FORBES, M.D. London, 1849. 8vo. pp. xii., 520.

FEW men living have done so much for their profession, and that so well, as the "healthy Sexagenarian" whose pleasant book we have just finished. It would give us great pleasure to make good this assertion; but this is not our immediate object, and deserves, moreover, ampler and different treatment than is now in our power;—and besides, our readers will find, under Medical News, in the account of the presentation of his testimonial to Dr Forbes, many things which we might have said, and in which we cordially concur. Our object now, is to recommend the "Physician's Holi-

day," as a vigorous, pleasant, and informing book, having all its excellent author's well known characteristics, and now and then some things, it may be not better, but more subtle and delicate, —more "informed with phantasy,"—than we had given him credit for. But we beg to send our friends to the Month's Holiday itself. It would have been very pleasant to have given some extracts from his introduction, in which there is a lively account of the various forms of the London Doctor's Holiday; this one, for instance, is droll enough :—

"A philosophical friend, whose active brain will not allow him to desert his books and his apparatus, even for the woods and fields which he loves so much, takes his holiday sometimes in quite a different style: he sends his horses to grass, shuts up his front-windows, retires to his library in the rear, and leaves strict injunctions with the footman to inform all inquirers, patients especially, that he has gone on his annual holiday."—P. 3.

We should also have liked to have gone at some length into the chapters upon the benefits of travelling, upon the glaciers and moraines, the warm baths of Leukerbad, Bad-Pfeffers, Stachelberg, &c., and especially the visit to Dr Guggenbühl's Cretin establishment, so well known now, through Dr Coldstream's interesting little work and otherwise; but our limits forbid all or any of this. The two following extracts are given merely as being in themselves curious, and as showing that under his umbrageous *wide-awake* (in praise of which we can from experience speak), the Doctor carried a pair of fresh, shrewd, and honest eyes.

"The nearly total absence of birds of every sort, was a thing which struck us as very remarkable in our Swiss tour. Not a rook, not a magpie, not a sparrow, not a wood-pigeon, not a blackbird or thrush, lark or linnet, not a partridge or quail, did we see; not a song, caw, croak, scream, or chirp, did we hear from one end of the land to the other, with the following exceptions, which I noted as mark-worthy for their rarity:—On the top of the Col de Fours, in the Alps of Savoy, we saw a raven; in the Munsterthal in the Jura we saw an eagle; in some other place, but exactly where I now forget, we saw a hawk; on the brow of the mountains which bound the Klausen Pass we saw a few white game or ptarmigan; and on the slaty slopes of the Riffelhorn and Mount Breven, I saw a couple of stone-chats."—P. 281-2.

Did our readers ever taste or hear of boiled butter, *beurre cuit?*

"In looking at the horrid compound sold in England as *salt butter*, at least the cheaper sorts of it used by the poorer classes, I cannot but believe that its supercession by the boiled butter of Switzerland would be advantageous both to the comfort and health of a large proportion of our countrymen. It can hardly be believed that such an offensive, briny, and semi-putrid mass as the cheaper sorts of our salt butter, can be without serious detriment to the health of the consumers, any more than the salted meat formerly issued to our seamen was so. The only difference in the two cases, is the comparative quantity consumed in each case; in itself, I am disposed to regard the rancid butter as the more unwholesome of the two. The boiled butter, while infinitely more palatable, is neither saline nor rancid, and, consequently, is calculated to be more easily digested, and to produce a more wholesome material for absorption into the system.

"I give the receipt for the process of making the boiled butter in the words I took it down from the mouth of my guide from the valley of Entremont, with

the addition of some little variations in the process, as I obtained them from others learned in the same art.

"*Formula.*—Into a clean copper pan [better, no doubt, tinned] put any quantity of butter, say from twenty to forty pounds, and place it over a very gentle fire, so that it may melt slowly; and let the heat be so graduated, that the melted mass does not come to the boil in less than about two hours. During all this time the butter must be frequently stirred, say once in five or ten minutes, so that the whole mass may be thoroughly intermixed, and the top and bottom change places from time to time. When the melted mass boils, the fire is to be so regulated as to *keep* the butter at a *gentle boil* for about two hours more, the stirring being still continued, but not necessarily so frequently as before. The vessel is then to be removed from the fire, and set aside to cool and settle, still gradually; this process of cooling being supposed also to require about two hours. The melted mass is then, while still quite liquid, to be carefully poured into the crock or jar in which it is to be kept. In the process of cooling, there is deposited a whitish cheesy sediment proportioned to the quantity of butter, which is to be carefully prevented from intermixture with the preserved butter."[1] * * *

"Everybody agreed in asserting that butter so preserved will last *for years* perfectly good, without any particular precautions being taken to keep it from the air, without the slightest addition of salt."—P. 493-495.

We were particularly pleased with the description of the Falls of Schaffhausen, and with some writing, which harder heads than ours may call sentimental, but which pleased us greatly. We yield to the strong temptation to insert a specimen, hoping that our "constant readers" may be gratified by dwelling for a moment, in Dr Forbes' company, on matters not commonly thrown in their way in the pages of the "Monthly Journal."

"The huge white mass of tumbling foam lay straight before us, the only bright spot in the dimly-lighted landscape, and attracting and fixing the eye exclusively on itself. No sound was heard but the one continuous roar of the water, softened by the distance, and seeming to fill the whole air like the moonshine itself. There was something both wild and delightful in the hour and its accompaniments. The mind yielded passively to the impressions made on the senses. A host of half-formed, vague, and visionary thoughts crowded into it at the same time, giving rise to feelings at once tender and melancholy, accompanied with a sort of objectless sympathy or yearning after something unknown. The ideas and emotions most definite and constant were those of Power and Perpetuity, Wonder and Awe. What was now impressing the senses and the mind seemed a part of something infinite, which they could neither comprehend nor shake off; the same mass, the same rush, the same roar, day and night, year after year, age after age, now and for ever." —P. 70.

"Every one must have felt that on looking stedfastly on a spectacle like this, the eye at length seems to become fixed on it with a sort of fascination, and that it is impossible to avoid falling into one of those dreamy half-conscious moods, in which the common trains of thought take their own way without leave or licence from the higher powers. This must have been my case on the present occasion; as, all at once, while still sitting in the sun and looking on the mountains, I felt that every impression of what was actually present had vanished, and the mind was exclusively filled with ideas of a totally different cast, ideas of desertion and desolation, of utter solitude and silence. I presume that fancy had been playing one of her ordinary tricks

[1] These caseous grounds are very palatable and nutrient, and are constantly used as food.

with the unconscious spectator, placing him in imagination amid the very scenes he was contemplating, and making him feel, for the moment, as if he actually suffered what he merely thought; otherwise a prospect of such grandeur, brilliancy, and beauty, ought to have excited feelings of quite a different kind."—P. 298.

We are glad to see that Dr Forbes is able, and not ashamed, to quote Seneca and Dante, Wordsworth and Milton, Coleridge and Crabbe, as well as Schiller and Lord Byron. The book is beautifully printed, and has, what so few modern books have, a good index —the want of which seems to us at once a wrong and an affront to the reader, and must ultimately prove a serious obstruction to the only worthy object a man can have in writing a book—the rendering it intelligible, useful, and easy of reference. We shall be happy to meet Dr Forbes soon again on the mountains, or wheresoever he may spend his next month, and his "guinea a-day,"—"*in* (to adapt his own felicitous motto) *apertis ambulationibus vagans, cœlo libero et multo spiritu, sese ac nosmet, augens simul atque attollens.*" By-the-bye, he gives two advices, one of which struck us as new, and the other as very judicious,—the carrying a good opera glass instead of a telescope, and the being provided with a competency of "Brown Windsor."

Part Third.

MEDICAL NEWS.

EDINBURGH OBSTETRIC SOCIETY.

MEETING III.—*February* 14, 1849.—(Continued.)

CASE OF INTERNAL HEMORRHAGE, AND ALL BUT COMPLETE INVERSION OF THE UTERUS WITH MORBID ADHESION OF A PORTION OF THE PLACENTA. BY MR SIDEY.

Mrs P., during the first four months of pregnancy, suffered much from flooding, so much so as to render pregnancy doubtful; the os uteri being so fully open as to admit the finger within it. On the first of February, 1848, at half-past 9 P.M., labour set in, and three or four pains finished it. Mr S. arrived immediately afterwards and found the mother in a faint. Putting his hands on the abdomen, the uterus felt as if there was a second child.—No pulse at the wrist; face pallid; and, uttering a groan, the patient said she was dying. Mr S. gave her a large dose of whisky, and rubbed the uterus firmly; a violent pain, which now came on, expelled a large mass enveloped in membrane, which, dragging the uterus with it, thus caused all but complete inversion. Upon examination, Mr S. found a portion of the placenta morbidly adhering to the posterior wall of the uterus. Having separated this portion with steady pressure, Mr S. returned the fundus. This was followed by uterine contractions, and he found it necessary to keep the hand in the uterus for some minutes. The patient required, for three full hours, firm pressure over the uterus by the hand. Nothing but fatal sinking appeared to be her lot. Universal coldness came on, with gasping for breath every now and then. The pulse left the arm for some minutes, returning with

deep inspirations and moaning; she was constantly tossing the arms when not in a state of syncope, and deafness and blindness came on. This state of things lasted for nearly two hours. Laudanum in large doses was given, with strong camphor mixture, and spirits. Repeated sprinkling of cold water on the face kept up the respiration. Warm applications were made to the surface of the body to restore warmth to it, and at the end of four hours the pulse became steady, but continued weak, and the fainting fits subsided. On his next visit Mr S. found his patient stronger, and she had had some sleep during the night. The patient continued blind for some weeks, but after a residence in the country the sight gradually returned.

EXPULSION OF A BAG OF WATERS BEFORE THE RUPTURE OF THE MEMBRANES. BY DR ALEXANDER WOOD.

Dr Wood narrated a case which had occurred lately in his practice. On the 8th of February he received a hasty summons to attend a patient whose accouchment was not expected for a month, but who was suffering from a severe attack of pneumonia. Labour pains had lasted about two hours before he saw her. It was her fifth pregnancy. On making an examination, he discovered a membranous sac protruding from between the labia, which he thought consisted of the membranes. On pressing this up, however, during the interval between the pains, he could not reach the head on account of the interposition of fluid, which could not be removed by pressure. The next pain expelled this membranous body, when it was found to present a pyriform appearance, ending in a long neck, the extremity of which could not be reached by the finger. A double ligature was applied to the neck, which, being severed between the ligatures, the sac was removed entire, with the fluid which it contained.

In size it was capable of holding about six ounces of fluid. It burst in handling it, and discharged a clear limpid fluid. The membranes were found at the next examination presenting as usual, and the case terminated naturally, about an hour after the expulsion of the sac.

Both mother and child did well, although the latter was small and weakly.

ON THE LIGATURE OF THE CORD AFTER DELIVERY. BY DR ALEXANDER WOOD.

Dr Wood commenced by observing that had he not found from a conversation which took place at the last meeting of the Society, that few, if any, of the members present were aware that the practice of putting only one ligature on the cord was recommended by many authors, and practised in many parts of the Island, he would not have troubled the Society with the present communication. The truth was, that, having been led a considerable time ago to try whether allowing the blood to escape from the placenta, would not hasten the expulsion of that body, he had become convinced that it did, and determining to lay the results before the Society; he for the first time had his attention called to the prevalence of the practice elsewhere.

Dr Wood showed, by quotations from the earlier writers on midwifery (as Johnson, 1769; Whyte, 1773; Aiken, 1784; Baudelocque, 1781), that the practice of a single ligature was the prevalent one in their day. It is true, that Baudelocque states that "the custom of meeting two ligatures on the cord seems to be as ancient as the art itself," at the same time he most unequivocally condemns it. The practice recommended by these authors seems to have been abandoned by the authority of Denman who recommends the double ligature, in which he is followed by all succeeding authors of any note, with the exception of Dewees and Rigby.

Dr Wood contended that the emptying of the placenta tended, by reducing its bulk, to effect its separation from the uterine walls, and thus to hasten its expulsion. He stated that the quantity of blood which usually escaped varied from three to five ounces; that he had met with one case, where nine ounces were discharged. As the more recent writers on midwifery, understanding better the nature of the connection between the uterus and placenta, recommended a

double ligature only for the sake of cleanliness, Dr Wood suggested that the placing the sewed end of the cord in a teacup, or wrapping it in a napkin, was all that was necessary for this purpose.

The only possible source of danger was in those very rare cases where twins were present with a single, or united placenta. The existence of a second child was, however, generally ascertained previously to the severance of the cord, which would enable the practitioner, if he chose, to avoid all risks by applying a double ligature in twin cases.

REPORTS ON CHOLERA IN SCOTLAND, 1848-9.

DALKEITH AND ITS NEIGHBOURHOOD.—(*From Dr Thomson*).—The first case of the disease appeared here on the 27th of October last. It was without doubt an imported one. The subject of it had reason to be frequently in Edinburgh, and had been, previous to his seizure, in one of its infected districts. He was of intemperate habits, seldom took a regular meal, and altogether a most likely individual to fall a victim to the disease. The attack proved rapidly fatal. None of the attendants who were continually in close contact with the patient —and that in a small apartment—took the disease, though some of them appeared much predisposed from over fatigue.

No other case occurred in the town until the first of December, in the course of which month, there were eight cases according to the official return, but ten according to my own knowledge. After a temporary cessation, a man was brought into the town in the collapsed stage of cholera, from the parish of Dalhousie, to which place he was reported to have proceeded from an infected district in Edinburgh. He was conveyed to our hospital here, where he shortly died. As after the first, no case followed this last imported one. Of the eight cases that were returned during December, 6 occurred in a short, narrow, and ill-ventilated wynd. The other two that were not returned, occurred there also. Had I been asked where cholera was most likely to begin in Dalkeith, I should certainly have said in this wynd, and moreover, I should have pointed out the very close in which cholera did make its first appearance. The second case was a woman who attended the first house, next, her husband who lived in the opposite close, and then a girl in the same with the first. All of these died. Altogether, there were 6 cases that originated in this small spot. I feel convinced that more cases would have occurred, had not the most destitute been removed to a house of refuge. Some of them were suffering from bowel complaint at the time of their removal.

At Newton, a distance of a mile and a half or two miles from Dalkeith, cholera has been prevailing for some time past. With one or two exceptions, the cases have all occurred amongst colliers, most of whose houses are very damp, and, otherwise, in a miserably bad sanitary state. I attended the second case of cholera that occurred in this district, which proved fatal in the stage of consecutive fever. Upon inquiry, I found that the patient had been predisposed from intemperance—had not been near the first case, but that he had been visiting his relations in the neighbourhood at Newton Farm, where I was attending several farm-servants and their families on account of severe diarrhœa. I believe that the origin of cholera at Newton, cannot be satisfactorily attributed to, nor do I think that its progress has hitherto given much evidence of, contagion. The disease did not break out suddenly as at Loanhead. For nearly a fortnight before the first case occurred, bowel complaints were very *general;* so much was this the case, that the people themselves felt convinced that there was some common and external cause affecting all. The influence, affecting all so simultaneously, seemed to become more and more powerful, until at last, *bona fide* symptoms of cholera manifested themselves. At Newton Farm, where there were so many cases of diarrhœa, only one became *choleraic.* This is to be at-

tributed to the better condition of the people as compared with the colliers. Amongst the latter, the same thing was observed. The most predisposed and vulnerable had cholera, while the others of stronger constitution had only diarrhœa. Though the distance between Newton and Dalkeith is so short, and notwithstanding the frequent intercourse between the two parishes, not one case of cholera has occurred here since the appearance of the epidemic at Newton.

Kilwinning, Ayrshire—(*From Dr Craig*).—Previous to Wednesday, the 3d of January last, I had not seen or heard of any case of cholera in this town and neighbourhood; but on that day visited, along with Dr Service, a lad seventeen years of age, at the Eglinton iron-works, having well-marked symptoms of cholera. Dr Service also stated that on the Sunday previous he had been called to a girl about five years of age, who lived a few doors from this patient, and found her in a somewhat similar state, but not so well marked, and she had rallied considerably on Monday morning, and the pulse having returned, he thought her out of danger. This girl, however, became worse gradually, and died on the Thursday following with symptoms of oppressed brain. She had been brought from Carnbroe two days before Dr Service saw her, and at the time she left, her friends stated that there was no case of cholera there; but I observed from the newspapers that on the same night she was seized, there were thirteen cases of cholera at Carnbroe, most of which terminated fatally. Whether the friends' statement, in regard to there being no case of cholera when the girl left be correct, I have not been able to ascertain.

The next case we were called to was on Thursday the 5th, the wife of a miner, and next day her husband was seized. Both of these persons I afterwards ascertained had been in company with the person first mentioned while affected with the disease. I ought to have mentioned that he had bowel complaint for two days before we saw him, and had been vomiting and purging in the pit amongst his neighbour workmen, and was assisted home by them in a very exhausted state. These three cases having all rallied from the state of collapse, the impression became general that it was not cholera at all, and no little hostility was displayed against the doctors for saying so. On Saturday I found, however, that bowel complaints were very general, but no application was made for medical advice, the people being determined to treat themselves. On Sunday evening, however, the calls for medicine became frequent, and soon after midnight, I had several requests to go to the iron-works. I could see that things were in a bad state, for grief, alarm, and despair were painted in every countenance. On going down I found that Dr Service had been there before me. Several died in a few hours after we saw them. As early as possible I reported the state of matters to the parochial board, who dispatched me to Dalry for additional medical aid. I brought Mr Hunter along with me, and together we went through every house belonging to the works, numbering from eighty to ninety. We found about forty affected with the disease, twenty-seven of whom were completely collapsed. What I wish especially to note is, that by far the greater number of these were miners, who wrought in the same pit with the two men who were attacked the previous week. To follow the progress of the malady at the iron-works would be foreign to my present purpose, further than to observe in reference to a statement made by Dr Grainger in proof of the non-contagious nature of the disease, that not a single visitor to the Tooting Asylum took it—that the visitors to the ironworks enjoyed no such immunity. I had occasion to engage a woman as sick nurse for a destitute family; she went there on Sunday evening, took the disease on Wednesday, and died on Thursday. A man belonging to Kilwinning went out to look after his daughter who was ill; a week after he came home, he was seized with the malady and died. A woman came from Kilmarnock to attend some of her friends, who were ill; she also took the disease. Two boys came from Barrhead, in quest of work, but when they came the disease had broke out, and

they could not get work, and were afraid to go into any house; they crept under a furnace near to some of the houses where the disease was; in the morning one of them was found seized with cholera.

From the violence of the attack at the works, the inhabitants were struck with terror—every one that could get into a house any where else fled from it. One man, with his wife and mother-in-law, fled to the Kilbirnie coal miners' houses, near Dalry, and, immediately on his arrival, he took cholera and died, and in less than a week the disease broke out there. Another man fled to Stevenston, and took the disease—in a few days it broke out there. Another went to Hurlford, near Kilmarnock, and took the malady, and immediately it broke out there. Are we then to look on these occurrences in the relation of cause and effect, or view them merely as coincidences?

I will now relate some facts in regard to its introduction into the town, which fell more immediately under my own eye. For more than two weeks after its appearance at the iron-works, we had not a single case here, although these are little more than a quarter of a mile in distance from this place. The first case which I attended was that of a miner who wrought at the iron-works—rather a severe case; but he ultimately recovered. His wife took the disease slightly; and his father-in-law, an old man, who visited him four days afterwards, took the disease and died. The next case which I shall select as an example, from amongst many, exhibits very strong evidence of contagion. It was that of the grave-digger—he lived in an isolated part of the town, and had no communication with the disease, except burying those who died at the iron-works. He had assisted at about thirty funerals when he was seized. He recovered, and, as soon as he was able, went out among his relations. His father, an old man, died of cholera—so did his sister-in-law—her husband (being his, the grave-digger's, brother); and two of her children had the disease in a mild form. Two of the grave-digger's sisters, both married women, also took it; the one recovered, and the other is at present apparently dying from the consecutive fever. The husband of the latter, and a son of the former, also took the disease, but in a milder form. All these persons live a considerable distance from each other, where there had been no cases of cholera previously, but all of them had had communication with the grave-digger, or with one another, while labouring under the complaint before they took it themselves. I could give many examples equally indicative of the contagious nature of the disease, and, on comparing notes with the other medical men here, I find that they have all been led to the same conclusion by similar examples. One other fact which I observed was, that persons who washed the clothes or bed-clothes of those who were affected were particularly liable to take the disease. Four cases of this kind occur to me at this moment, and all of these proved fatal. The Eglinton iron-works are situated on a dry and well-aired spot; the houses are large and well-aired, and all self-contained. Some of them, from the habits of the occupants, were kept in a very dirty state, but not more so than they were for months before the malady broke out, when there was no disease of any kind amongst them. The deaths were more numerous in the ill-kept dwellings which were in this state; but this, in my opinion, was sufficiently accounted for by personal, independent of local, circumstances.

In the town it has been equally prevalent and fatal, in the newest, the best aired, and isolated houses, as in the lowest hovels; and in the neighbourhood of the worst nuisances, and in second and third stories, as on ground-floors.

COALSNAUGHTON.—(*From J. B. Thomson, L.R.C.S., Edinburgh*).—Coalsnaughton is a mining village in the parish of Tillicoultry, county of Clackmannan. The village is generally the most healthy part of the parish, having an airy and elevated position on the south bank of the Devon. The soil around clayey and retentive—the water, as in most coal districts, is scarce and bad—the locality is filthy—the houses are damp, and the people poor. The population of Coalsnaughton is about 800, a few being employed in the woollen factories,

but the majority engaged in mining operations. When cholera appeared here, the inhabitants were suffering from want of work, and the privations induced by a strike among the miners.

The number of cholera cases during this visitation was as follows :—Eighteen dead ; thirty-three recovered ; fifty-one in all. Of these nineteen were males, and thirty-two females ; five were pregnant and four nursing ; of the adults seized several were intemperate, but this could not be charged against the majority of the sufferers, many of whom were young persons—twelve being under ten years of age. The epidemic was short-lived, beginning on the 31st January, reaching its height of severity on the 15th February, and disappearing (with a solitary exception) on the 25th February. During the prevalence of Asiatic cholera here, upwards of 100 persons laboured under diarrhœa, and were generally relieved by an opium pill, and improved diet. In the great proportion of the decided cases of cholera the premonitory symptom of diarrhœa took place several days before serious symptoms supervened.

As to the contagious or non-contagious nature of the disease, we offer the following facts :—

The *importation* of the disease could not be traced to any contagious influence. The first case was in a very respectable family, where the only suspicion of its transmission rested upon a lodger, who, about the same time had been in the vicinity of Stirling, an unaffected locality. The lodger himself continued well. The same thing happened in the house of the first cholera patient in the adjacent village of Alva. Can this explain the mode of importation from place to place ?

The *propagation* of cholera in Coalsnaughton could not be well explained by contagion. Within two or three days from its first appearance, cases happened in different localities, where no communication existed betwixt the patients or their families. These early cases—one at the east, one at the west, and a third in the centre of the village—became the *foci* from which the cases seemed to radiate. Indeed, the cases may be classed as occurring in these three localities, a large portion of the village being exempt. The number of persons who waited on and had nearest intercourse with the sick, were not victims to the disease in such proportion as others who merely lived in these infected localities, and did not hold immediate intercourse with the sick. The epidemic seemed to spread to all, with few exceptions, in these infected localities. In many instances only one of a family suffered, and only one or two of the nurses and attendants. In one crowded house where the corpse of a cholera patient was, in spite of the parochial authorities, kept three days, no others were affected. Several persons lay alongside of children for the sake of heat all the time of the disease, and are still well—two women under cholera continued to suckle as long as the secretion of milk continued, and as soon as it returned, and the infants escaped. Only in one remarkable instance were a number of the connexions seized, but this might not be from contagion, for it ought to be noticed that these were all living in infected localities at the time of the attack. Upon the whole, the cases of cholera here did not countenance the idea of the disease being either *imported* or *propagated* by contagion.

Selkirk.—(*From a Correspondent.*)—There has not been a new case of cholera here for nearly two months. The entire number of cases was fifty, of which thirteen died. Upwards of 150 individuals suffered during the same period from choleric diarrhœa. The small mortality was due mainly to the mild form of the disease during the last fortnight of its prevalence, and in part, probably also, to the success of the treatment—(see p. 491)—which was in many cases very marked. In reference to the origin of the disease, I observed before—(p. 564)—that among the cases occurring in *Dunsdale* contagion could not be satisfactorily made out. The evidences of its operation were more obvious among those cases which occurred in the elevated part of the town, and which is distant from Dunsdale about half a mile. The first fatal case there

was a woman, who, although she lived in the high part of Selkirk, worked during the day in Dunsdale, where she may be supposed to have got the disease. The next case in the high locality, also fatal, was a woman who lived in the same house with the last, and had waited upon her. The nurse, who was paid by the parochial board to attend the first case, was the next victim. She lived at some distance. The woman who had washed the clothes of the first case after the burial, then suffered. Although living in the elevated part of the town, her house was a considerable distance from the dwellings of the previous cases. She had a severe attack, but recovered. Lastly, the husband of the nurse above referred to, had cholera, and recovered. Of the other six individuals living in the high part of Selkirk who suffered from cholera, several were operatives in the manufactories of Dunsdale, where they passed the greater part of the day.

It is worth remarking that the operatives and poor of Selkirk were very grateful for the attention and kindness shown them during the prevalence of cholera. They had a meeting among themselves, at which thanks were most cordially voted to the chief magistrate, medical gentlemen, and others. A handsome sum was subscribed for presentation to the inspector of poor, whose attention well merited this mark of their gratitude.

KIRKLISTON, LINLITHGOWSHIRE.—(*From Dr Fleming*).—There were seven cases—five fatal. The first case was an Irishwoman, who became sick a few hours after her arrival from Roxburgh. At the time of her residence there cholera prevailed in the neighbouring town of Kelso. She passed through Edinburgh on her way to Kirkliston, but denied having seen any affected person there, or previously. This woman died. Her husband left Kirliston for Edinburgh on the day of her funeral, and was seized there the same afternoon. He was taken to the Cholera Hospital, in Surgeon Square, where he recovered. The second victim in Kirkliston was a man who lived next door but one to the first case, with whom he must have been at least in indirect communication. He denied having seen her while ill. The sister of the doctor who attended these two cases was the third victim ; and the fourth was Dr Braid himself. Although the sister died before her brother, who alone had been in immediate communication with the sick,—it is deserving of note, that he had been suffering from premonitory diarrhœa for two days previous to her seizure, during which period the disease may be supposed to have passed to her by direct contagion. The fifth case, a woman, lived in the house adjoining the dwelling of the second case. She recovered. The nurse who attended Dr Braid and his sister, and who lived on the side of the village most distant from the previous cases, made the sixth case. She died. Lastly, the brother of the second case had a sharp attack, but made a good recovery. During the month of February 1849, when these cases occurred, there were few or no bowel complaints in Kirkliston. Diarrhœa had prevailed there epidemically in the months of October and November 1848.

DALMELLINGTON, AYRSHIRE.—(*From Dr Cruickshank*).—John Rafferty, a labourer, said to be of temperate habits, was seized on the 24th January and died on the 25th. He came from Kilmarnock a short time before. No one here seems to have known him, and the family he lodged with took fright, and left this part of the country the day after he died.

The next case was that of a field-labourer, Ann Wilson. She had come shortly before to lodge in the same house with the former patient. She did not assist or go near him during his illness; but, on the 27th and 28th, she remained at home from her work, and assisted to wash his clothes and the bedding. Up to this time she was perfectly well ; but, about ten o'clock on the night of the 28th, she was seized with symptoms of cholera, and died on the 29th.

These two cases are the only ones that have occurred here. There have been a great number of cases of diarrhœa here for the last three months, which the inhabitants would have to be cholera; none of them were fatal. I may

mention that I am a non-contagionist, although the cases related rather point otherwise.

New Lanark.—(*From Dr R. Logan*).—We have had no cases of cholera in its epidemic form in this place or in the neighbouring town of Lanark: there have been isolated cases *imported* in three instances, which proved fatal, in the neighbouring villages, without any extension of the disease.

Carluke.—(*From Dr Selkirk*).—This district escaped the ravages of the late prevailing epidemic cholera. Dysentery, diarrhœa, and anomalous intestinal "pains," however, prevailed to an extent and intensity unprecedented in the locality.

All the cases treated in this district were imported, with one exception.

Case I.—A young man, employed at the St Rollox Station, Glasgow, was seized on an afternoon in the latter part of December 1848, and arrived at the residence of his friends here three hours after seizure; he recovered. Free intercourse by friends and acquaintances was permitted: the disease was not propagated.

Case II.—Brought from Holytown or Carnbroe district, and died from secondary affection of the brain; none of the friends became affected.

Case III.—The exceptional case occurring sporadically, above alluded to; this patient recovered: no other seizure took place.

Case IV.—Arrived from Airdrie—died in the algide stage; no case followed.

Case V.—(Communicated by Dr France, jun., Lesmahago).—A female, while visiting her sick relations at Holytown or Coatbridge, was seized with cholera, and brought to Draffan, Lesmahago; she recovered: the disease progressed no farther.

Case VI.—(Communicated).—A female from parish of Stonehouse was seized while in attendance upon sick relatives at Cranbroe—returned and died of the affection: the disease was not transmitted to any other person.

[From the small space at our disposal this month for Medical News, and from the valuable reports with which we have been lately favoured from Glasgow and elsewhere, we are obliged to defer the conclusion and summary of these Reports on Cholera till July.—Edits. M. J.]

PRESENTATION OF A TESTIMONIAL TO DR FORBES.

(*From the Medical Gazette.*)

The committee appointed at the general meeting, by the subscribers to the Forbes Testimonial, to expend the sum collected, in the purchase of a piece of plate, presented this testimonial to Dr Forbes on the evening of the 3d of May. The committee invited the subscribers in London and its vicinity to meet them on the occasion, and nearly one hundred professional gentlemen obeyed the summons. The testimonial consists of a very handsome piece of plate, a candelabrum and epergne, beautifully designed and executed by Messrs Smith, of Duke Street, Lincoln's-Inn Fields. The stem is supported by three figures, representing Industry, Health, and Hope, corresponding to Dr Forbes' motto, "labore, robore, spe."

Mr Stanley, president of the Royal College of Surgeons, presented the testimonial to Dr Forbes. After a short allusion to the circumstances in which the subscription originated, Mr Stanley proceeded to read the following address, framed by the committee in accordance with the resolution passed at the general meeting:—

"Sir,—On the occasion of your relinquishing the editorship of the British and Foreign Medical Review, it was felt by many of your friends and the admirers of that journal, that a proper opportunity had arrived for expressing to you their sense of the deep service you had rendered to medical science, by the spirit and method in which, for twelve years, you conducted that important and

influential publication. It would be out of place to enter here into any statement of the mode in which that journal was conducted, or into any estimate of the great influence it from the first excited on English medical literature—an influence, we may add, which still continues, and which is not likely soon to pass away. Suffice it to say, that you brought to the task of editorship intellectual and moral qualities of so high an order, that your review was at once placed in the foremost rank among the journals of periodical criticism, both medical and general. It was impossible that such a journal should cease to appear without some acknowledgment on the part of those who had so largely benefitted by it. It would have been an act of injustice if no record of regret had been entered to mark the close of its useful and honourable career. A subscription was therefore opened, to present you with a testimonial, in expression of the sense of your professional brethren on the occasion; and the rapidity with which the lists were filled, testifies to the esteem with which you are universally regarded.

"We may be permitted, also, to add, that we do not present this testimonial only to the late editor of the British and Foreign Medical Review, but also to the man whose excellent moral character is no less remarkable than his wisdom and sagacity; to the physician who has played an important part, and filled many important positions, without ever forgetting that life is a sacred trust, which we are bound to keep pure and unsullied; to the physician who is, if any one is, a pattern to the younger members of our profession of truthfulness, manliness, and sincerity.

"We will not occupy your time further than to give expression to the earnest prayer of this assembly, that many years may be permitted to you to enjoy the honour which your professional brethren now offer you."

Mr Stanley then said—In the sentiments expressed in this address, Dr Forbes, I most heartily concur, and I am happy that it has fallen to my lot to discharge the pleasing duty of presenting it to you. It is a most gratifying circumstance to me individually to find that the merits of one whom I esteem so highly have drawn forth this public expression of gratitude and regard; and, on public grounds, I cannot but rejoice that the services which have been so well and so manfully rendered have met with spontaneous and immediate recognition.

Dr Forbes replied: I have, sir, on many occasions regretted that I did not possess the happy art of giving appropriate words to feelings which demanded utterance, and never have I regretted it more than on this occasion. But I console myself with the thought that it is not for making speeches, for throwing easily vivid thoughts into eloquent words, that so many of the most eminent of my professional brethren have assembled here this evening to confer on me this great honour, but for a strenuous endeavour to discharge a duty which I had assumed, and to fulfil the tacit guarantee into which I entered when I became the editor of a journal of periodical criticism. And on this ground I do not refuse the honour you offer me; I do not deny that when I established the British and Foreign Medical Review (although I did not altogether exclude personal and private considerations) I did so mainly on public grounds, because I thought such a journal was wanted, and would do good service to the profession. But, sir, I cannot permit myself to assume all the merit you have attributed to me in the too flattering address you have now read. The merit of the journal which I lately conducted, of its literary pretensions, and of its philosophical acumen, is not due to me, but to the band of admirable contributors whom I was enabled to assemble around me. In their names I will not refuse the terms in which you have been pleased to speak of that journal. I may have appeared as an occasional contributor, and more frequently as the reviser or corrector of the criticisms of others, but the chief part of my duty was to see that the works of authors went into competent and impartial hands, and were criticised by men who, as far as I could judge, were the most likely to give a true and unbiassed account of their contents.

And, sir, I believe that, as far as is possible, the criticisms in the British and Foreign Medical Review were able, were honest, were unstained by feelings of petty rivalry, and untarnished by the bias of personal enmity. It was, at least, my intention to make them not only sound in philosophy, but also just as to the authors' merits. That my endeavour was, at any rate, partially successful, I have the high gratification of feeling this evening, sir. That magnificent piece of plate, valuable as a mere mass of metal, valuable as a work of art, has yet to me a value of a different kind, which is not to be measured by gold, nor supported by the most perfect of human productions. The editorship of the British and Foreign Medical Review was to me no ungrateful task; it was not performed with pain and repining; on the contrary, it was a pleasure to me, a gratification of all my faculties, a true labour of love. But had it been otherwise,—had it been, indeed, a hard task performed with sighs, and urged by painful necessity,—I should have been repaid for it now. Now I should have received my reward, and have taken the vantage ground from fortune. For a man may be justly proud when he finds his labours thus rewarded, by the approbation of many of the most eminent of the profession, not only in this country, but also in America—that mighty state which is indissolubly connected with Great Britain by so many ties of common interest. That testimonial, sir, shall be to me the dearest of my possessions; whatever may happen to me, from it I will not be separated; and it shall be cherished by my children when I can receive no more pleasure from it.

I feel, gentlemen, that I have most imperfectly expressed my grateful feelings towards you. You will understand what it is not easy to put into words. Permit me, sir, to acknowledge the kind terms in which you individually have spoken of me, and which, from you, are words indeed of worth.

Dr Forbes was loudly applauded by the audience, and shortly after the conclusion of his speech, the meeting separated.

MEDICAL RELIEF OF THE POLES.

It appears, from a statement made by Lord Eglintoun in the House of Lords on the 20th April, that in the estimates of the present year, there is entered a charge of £8,700 for the subsistence of the Poles, and for the expenses of medical attendance upon them. It was further stated that there were medical attendants provided for these refugees at London and Portsmouth, together with medicines when required. The noble Lord significantly observed that he declined alluding to the diseases for which that medical attendance and assistance were required; but he moved for certain returns which he intimated would satisfy their Lordships, that a very improper use was made of the money annually granted by the nation. Lord Stanley put the case more plainly in stating that the charge was, that the assistance given to these industrious revolutionists, did not consist so much of money to relieve their distress, as of medical assistance given to disease, not arising from distress, but from vice and profligacy.

Considering the mode in which a sick English pauper, with a wife and family, is dealt with, and the stinted medical attendance which is provided for his relief, it appears to us that the inquiry made by Lord Eglintoun was of a very proper nature. The financial part of the return has, however, alone been conceded by Government. The medical statistics have been refused without any good or sufficient reason, since the names of the recipients were not demanded. That there must be something in the charge, is clear from the peremptory refusal of the medical returns; and it is only a reasonable inference that the nation pays to a greater extent than it is desirable to make public, for the relief of these mendicants from the results of their own misconduct.—*Med. Gaz.*

BOOKS RECEIVED.

Addison (Wm.) on Consumption and Scrofula. London. Churchill. 1849. 8vo. Pp. 320.

Wiglesworth—The Dependence of Animal Motion on the Law of Gravity. Parts I. and II. London. Bailliere. 1849. 8vo. Pp. 210.

Mulder—Chemistry of Animal and Vegetable Physiology. Part IV. (Plates.) Edinburgh. Blackwood. 8vo. 1849.

Report of the Crichton Royal Asylum for Lunatics, 1848; and Report on the State of the Crichton Institution during the prevalence of Cholera.

Bernard—Du Suc Pancreatique. Paris. 1849. (Reprint).

Robin—Memoire sur l'Existence, &c. (Reprint.) Paris. 1849.

Scot—Madras Report on the Epidemic Cholera. (Abridged from the original Report in 1824.) 8vo. Edinburgh and London. Blackwood. 1849.

Brown—Questions and Observations in Naval Hygiene. London. 12mo. Churchill. 1849. Pp. 64.

Hare on Curvature of the Spine. Third Edition. London. Churchill. 1849. 8vo. Pp. 245.

A Demonstration of the Necessity of Revelation, drawn from a Phrenological Explication of the Mind. Edinburgh. Maclachlan and Stewart. 1849. 12mo. Pp. 68.

First and Second Reports by the Royal College of Physicians, on Measures for the Improvement of the Health of Towns. Edinburgh. 1849. 8vo. Pp. 19.

Lectures on Medical Missions. Edinburgh. Sutherland and Knox. 12mo. 1849. Pp. 320.

Rynd on Strictures and some other Diseases of the Urinary Organs. London. Longman. 1849. 8vo. Pp. 195.

Solly on the Brain, its Structure, Physiology, and Diseases. Second Edition, with numerous wood engravings. London. Longman. 1847. 8vo. Pp. 688.

Smith (Tyler) on Parturition and the Principles and Practices of Obstetrics. London. Churchill. 1849. 12mo. Pp. 395.

Proceedings of the Westminster Medical Society. Session, 1848-49. No. 1.

Cormack on Fever.—Battersby on Pica. (Reprints.)

Chadwick—On the Use of Alcoholic Liquors in Health and Disease. 12m9. London. 1840. Pp. 122.

Bryson on the Epidemic Fevers of Sierra Leone. 12mo. London. 1849. Pp. 174.

Meigs (Chas. D.)—Obstetrics, the Science and the Art. 8vo. Philadelphia. 1849. Pp. 685. (Woodcuts.)

Mitchell on the Uterine Speculum. 8vo. Dublin. Fannin. 1849. (Plates.) Pp. 83.

Balfour—Manual of Botany. 12mo. Glasgow. 1849. (Numerous woodcuts.) Pp. 641.

Pharmaceutical Journal. Vols. 1 to 7.

Kane (Sir R.) Elements of Chemistry. 2d edition. 8vo. Dublin. 1849.

Shaw—Medical Remembrancer. 3d Edition. London. 1849. 12mo. Pp. 108.

Wilson (E.)—Healthy Skin; a Treatise on the Management of the Skin and Hair. &c. 3d edition. 8vo. London. 1849. Pp. 238.

Eighth and Ninth Annual Reports of the Registrar-General of Births, Deaths, and Marriages in England. London. 1849. 8vo. Pp. 366 and 250.

Collins—Life and Writings of the late Joseph Clarke, Esq., M.D., &c. London. 1849. Longman. Pp. 88.

Arnott—On the Treatment of Headach by Benumbing Cold. 8vo. Brighton. 1849. Pp. 54.

Gilbert on the Extraction of Teeth, &c. 8vo. London. 1849. Pp. 66.

Robertson's Guide to the Buxton Waters. 12mo. London. 1849. Pp. 32.

Report of Pennsylvania Hospital for the Insane.

Bennet (James Henry)—On Inflammation of the Uterus and its Appendages. 8vo. London. 1849. Pp. 527.

Bowman's Lectures on the parts concerned in the operations on the Eye, and on the Structure of the Retina. London. Longman. 1849. 8vo. Pp. 143.

Critchett on the Causes and Treatment of Ulcers of the Lower Extremity. London. Churchill. 1849. 8vo. Pp. 121.

Hugman on Morbus Coxarius. London. Highley. 1849. 8vo. Pp. 81.

NOTICE TO SUBSCRIBERS.

In order that the volumes of the MONTHLY JOURNAL may coincide with those of the RETROSPECT, beginning in January and ending in December, it has been determined that the present volume shall close in December 1849, instead of in the present number. The usual Index, together with that of the RETROSPECT, will accordingly be given in December.

Plate III

Fig. 1.

Position of Twins. (from Smellie)

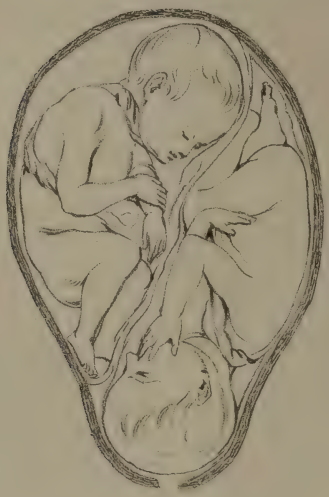
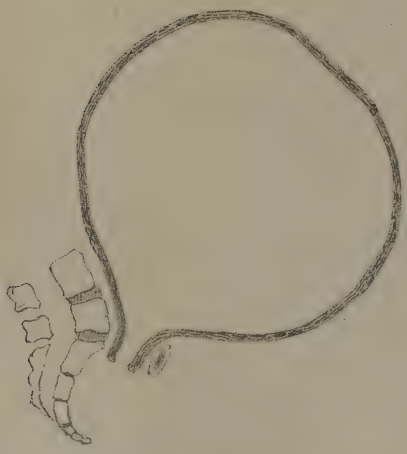


Fig. 2.

Hydrocephalus as affecting Position.

Fig. 3.



Form of Uterus in Cæsarean Operation.

Fig. 4.

Form of Uterus in Breech Presentation (from Chailly)

Fr Schenck lithogr Edinr

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*The Attitude and Positions, Natural and Preternatural, of the Fœtus in Utero, acts of the Reflex or Excito-Motory System.* Part III. By J. Y. Simpson, M.D., F.R.S.E., Professor of Midwifery in the University of Edinburgh.—(*Concluded from* p. 655.)

Section IV.—Malpresentations of the Fœtus; their Causes and Modes of Production.

Hitherto we have spoken only of the usual and common position of the fœtus with the head downwards, and presenting over the os uteri. But other parts of the fœtus than its head sometimes present over the uterine orifice in the latter months of utero-gestation, and during labour. When any other region than the head of the fœtus presents, the resulting "malpresentation," or "preternatural presentation" (as it is generally termed), is one of two kinds, viz., either the presentation of some part of the pelvic end of the ovoid mass of the fœtus;—or the presentation of the lateral surface of the trunk, but particularly the shoulder or arm. In other words, the full grown infant, at the time of labour, is practically found to present over the os uteri in one of three different modes, namely—1, with its head (or cephalic extremity); 2, with the nates, knees, feet (or pelvic extremity); or, 3, it may be situated transversely across the uterine cavity, and hence present a side, shoulder, or arm at the uterine orifice. In a previous part I have already given the following table, with the view of showing the relative proportion in which these three leading orders or genera of presentations of the fœtus are observed to occur in practice, as deducible from the extensive reports on the subject, published by Lachapelle, Boivin, Clarke and Collins.

Table of the Relative Number of Cases in which the Presentation of the Child was found Cephalic, Pelvic, and Transverse.

Reporter.	Total No. of Cases.	No. of Head Presentations.	No. of Pelvic Presentations.	Number of Transverse Presentations.
Lachapelle,	37,126	35,550	1390	186
Boivin,	20,517	19,810	611	96
Clarke,	10,387	10,094	245	48
Collins,	16,654	16,102	504	48
Total,	84,684	81,556	2750	378
Proportions.		96 in 100	1 in 31	1 in 224

In the preceding sections we have seen the difficulties with which all theories of the natural and common attitude of the fœtus with the head lowest, have heretofore been confessedly beset. And it can easily be conceived, that if the rationale of the cause of the natural position of the fœtus has been a matter of doubt and uncertainty, the explanation of any deviations of the fœtus from that natural position has necessarily been involved in still greater dubiety and difficulty.

Indeed, though most systematic writers in midwifery have theorised more or less fully upon the subject of the natural position of the fœtus, and its supposed cause or causes—few of them have, in any degree, entered into the consideration of the cause or causes of its malpositions, or malpresentations; and of these few, none, as far as I am aware, have discussed the question at any length. They have vaguely referred to the subject rather than considered it. Those who believed that the child's head was turned downwards to the os uteri by a kind of somerset in the latter periods of pregnancy, or at the beginning of labour, have, when they mention the subject at all, usually contented themselves by simply attributing its malpositions to some derangement in this supposed mechanism.[1]

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1 Thus Roederer, after speaking of the physical gravitation of the head downwards, adds—"Several causes, however, may prevent its descent, or derange its direction. I place in this class defect of the liquor amnii, obliquity of the uterus, tumours in the uterus, premature efforts, and external violence."—*Roederer. L'Art des Accouchemens*, p. 41.

"The cause," observes Ould, "why the feet sometimes present, is certainly from the body being in a more than ordinary erect posture in the womb, at the commencement of labour, whereby the compression of the parts designed for that purpose, thrusts the body forward in a direct line, and consequently the feet against the orifice of the womb."—*Ould. Treatise of Midwifery.* Preface, p. 15.

The following is the only passage in his Memoir in which I find Professor Dubois referring to the probable cause or explanation of the malpositions or malpresentations of the fœtus:—"If," he remarks, "the ordinary relations of the fœtus to the uterus in the later periods of gestation, are the result of its own efforts, and consequently of a spontaneous determination, this ought to have an internal cause, and internal sensation, which provokes it. What, then

Other authors have mentioned one or more individual causes, as probably capable of producing malpositions of the fœtus, but without attempting any generalisation of the mechanism and operation of these causes.[1]

And some of our latest and best systematic authors on midwifery have declared, as their opinion, that all past suggestions on the subject have failed in affording any sufficient explanation of the pheno-

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is the nature of this cause,—of this sensation? The abnormal situation of the infant, in which the pelvic extremity corresponds to the small extremity of the ovum, might be uncomfortable or painful even for the fœtus; and the spontaneous movements by which it would change its position, might be classed among those produced by a state of suffering;—or else each of the extremities of the trunk of the fœtus fitting better to the form of the ovum, when the pelvis of the fœtus lies superiorly, and the head inferiorly, might it be the easiness of this position which determines the fœtus to seek it, and return to it."—*Mémoires de l'Acad. Roy. de Méd.*, tom. ii., p. 284.

[1] For example, in speaking of this subject, Dr Denman observes, "It seems doubtful whether we ought not to exclude accidents as the common causes of these presentations, and search for the real cause in some more intricate circumstances; such as the manner after which the ovum may pass out of the ovarium into the uterus; some peculiarity in the form of the cavity of the uterus, abdomen, or pelvis; in the quantity of the waters of the ovum at some certain time of pregnancy; in the circumvolution of the funis round the haunches or lower part of the back of the child; or perhaps in the insertion of the funis into the abdomen of the child, which is not in all cases confined to one precise part, but admits of considerable variety."—*Introduction to Midwifery*, p. 467.

"We know little," remarks Dr Clark, "of the cause of preternatural labour; perhaps it depends upon a peculiarity of form, either in the uterus or pelvis. It is said to arise from accidents; but preternatural births are most likely the effects of peculiarity of shape in the parts."—*London Practice of Midwifery*, p. 238.

"It is not easy," says Spence, "to ascertain the cause of the different situations of children in the womb, as in some women it may be owing to too great an inactivity during pregnancy, in others to external violence or over-exertion of strength. Again, the want of a sufficient quantity of the liquor amnii, or the child being entangled by the umbilical cord, may occasion this."—*System of Midwifery*, p. 220.

"The causes," observes M. Chailly, "of presentations of the trunk, cannot be very well appreciated. But it is generally supposed that small size and mobility of the fœtus, and obliquity of the womb, may occasion them."—*Treatise on Midwifery*, p. 443. Transl. of Dr Bedford.

"The smallness and mobility of the fœtus," M. Cazeaux states, "obliquity of the uterus, and of the passages of the pelvis, deformities of the pelvis, are generally considered as predisposing causes."—*Traité des Accouchemens*, p. 363.

Some authors, in expressing their opinions on the probable causes of the malpositions of the fœtus, admit one or more of these individual causes, but reject others. Thus, Dr Rigby, when discussing this point, observes—

"The question naturally suggests itself: by what means is the long diameter of the child in so large a majority of cases kept parallel with that of the uterus? This depends in a great measure upon the form and size of the uterus. Where the uterus is not unduly distended with liquor amnii, and where it preserves its natural oval figure, it is scarcely possible that the child should present in any other way than with its cephalic or pelvic extremity foremost. There can be no doubt that the first early contractions of the uterus, in the commencement of labour, have a great effect in regulating the position of the

mena. Thus, in speaking of pelvic presentations, Dr Tucker states, "no satisfactory reason has as yet been given in explanation for their occurrence."[1] When treating of transverse presentations, Dr Churchill, in the same spirit, remarks, "I think all the explanations as yet offered are insufficient."[2] Dr Ramsbotham conceives that there are not "any evident causes to which we can assign them."[3] "To explain why the head of the child does not invariably present in labour is," observes Dr Lee,[4] "very difficult or impossible."

The present state of obstetric knowledge, then, with regard to the production of malpresentations of the fœtus, is confessedly very defective. All the doctrines that have been previously proposed with regard to the causation of the natural position of the fœtus have been imperfect in this important respect, that they have failed to afford any clue to the solution of the mode of causation of its occasional preternatural positions. But the new explanation, which I have offered, regarding the production of the normal position of the child, derives additional strength and confirmation from its affording at the same time a perfect rationale of the mode or modes of production of its anormal presentations and positions.

In the observations which I have offered in the preceding section of the present communication, I have attempted to establish the following, among other propositions, relative to the common or natural position of the fœtus:—

1. The usual position of the fœtus, with the head lowest, and presenting over the os uteri, is not assumed till about the sixth month of intra-uterine life, and becomes more frequent and more certain from that time onwards to the full term of utero-gestation.

2. Both the assumption and maintenance of this position, are vital and not physical acts, for they are found to be dependent on the existence and continuance of vitality in the child; concurring with its life, but being lost by its death.

3. In human physiology we do not know or recognise any vital

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child. * * We may state that the causes of arm or shoulder presentations are of two kinds, viz., where the uterus has been distended by an unusual quantity of liquor amnii, or where, from a faulty condition of the early pains of labour, its form has been altered, and with it, the position of the child. * * Shortness of the umbilical cord, or its being twisted round the child, insertion of the placenta to one side of the uterus, faulty form or inclination of the pelvis, obliquity of the uterus, as above-mentioned, violent exertions or concussions of the body, plurality of children; of all these we do not believe that there is one which can exert the slightest influence in determining the position of the child."—*Dr Rigby. System of Midwifery*, p. 168.

[1] Principles and Practice of Midwifery (1848), p. 212.

[2] Theory and Practice of Midwifery, p. 258.

[3] Obstetric Medicine and Surgery, p. 333.

[4] Lectures on Midwifery, p. 327.

power or action, except muscular action, capable of producing motions calculated to alter or regulate the position, either of the whole body, or of any of its parts; and further, the motory muscular actions of the fœtus are not spontaneous or voluntary, but reflex or excito-motory in their nature, causation, and effects.

4. The position of the fœtus, with the head placed over the os uteri, is that position in which the physical shape of the normal and fully developed fœtus is best adapted to the physical shape of the normal and fully developed cavity of the uterus.

5. This adaptive position of the contained body to the containing cavity is the aggregate result of reflex or excito-motory movements on the part of the fœtus, by which it keeps its cutaneous surface withdrawn as far as possible from the causes of irritation that may act upon it as excitants, or that happen to restrain its freedom of position or motion.

In proceeding to discuss the production of the malpresentations and malpositions of the fœtus, I shall endeavour to show that its preternatural presentations originate in the derangement of one or other of these normal and necessary conditions; and that they are referable to the following series of causes, viz.: to

First.—Prematurity of the labour; parturition occurring before the natural position of the fœtus is established.

Secondly.—Death of the child in utero; or in other words, the loss of the adaptive vital reflex actions of the fœtus.

Thirdly.—Causes altering the normal shape of the fœtus or contained body, or causes altering the normal shape of the uterus or containing body, and thus forcing the fœtus to assume, in its reflex movements, an unusual position in order to adapt itself to the unusual circumstances in which it happens to be placed.

And lastly.—Preternatural presentations are occasionally the result of causes physically displacing either the whole fœtus or its presenting part, during the latter periods of utero-gestation, or at the commencement of labour.

Let us consider in succession and detail the special causes of malpresentation of the fœtus, referable to each of these general heads.

1.—*Prematurity of Parturition.*

We have seen that the common or normal position of the fœtus with the head placed lowest, and presenting over the os uteri, is not in general assumed till the sixth month; and that from this period onward to the full time, this special position is taken and kept with a certainty and frequency which gradually increase in proportion to the advancement of the pregnancy. The data published in the table given at p. 643, show, in evidence of this law, that, while the percentage of head presentations among children born at the end of

the sixth month is only about 52 in 100, or nearly 1 in 2, this percentage increases to 68 in 100 during the seventh month; to 76 in 100 during the eighth and ninth months; and at last, at the full term of utero-gestation, the proportion of head to other presentations rises as high as 96 or 97 in every 100 births.

It is hence evident that if, from any causes, parturition happens to come on prematurely, the child is much more liable to present preternaturally, than if pregnancy had gone on to the full time; or, in other words, the prematurity of the labour is, in this sense, a cause of the malpresentation of the fœtus.

The following table shows the variety of natural and preternatural presentations of the child met with by different observers in various series of cases of premature labour. The returns of Drs Collins,[1] M'Clintock, and Hardy,[2] refer to the Dublin Hospital, and those of Professor Dubois[3] refer to the Maternity Hospital of Paris. The returns of Hoffman,[4] Hamilton,[5] and Ramsbotham,[6] are practically interesting in one respect, namely, that they are the results obtained on the same point in a large number of cases, in which the induction of premature labour had been artificially performed.

Table of the Presentations of the Fœtus in 1087 *Premature Labours, supervening spontaneously, or induced artificially.*

Reporter.	Total Number of Cases.	Number of Head Presentations.	Number of Pelvic Presentations.	Number of Transverse Presentations.
Collins . . .	498	393	102	3
Hardy and M'Clintock .	108	70	29	9
Dubois . . .	240	147	82	11
Hoffman . . .	120	45	56	19
Hamilton .	59	54	4	1
Ramsbotham .	62	53	6	3
Total	1087	762	279	46
Proportions among premature labours . . .		70 in 100	1 in 4	1 in 23
Proportions among common labours at full time .		96 in 100	1 in 31	1 in 224

The last line in the preceding table, and in several of those tables that follow, gives the relative number of cephalic, pelvic, and trans-

[1] Practical Treatise, p. 461, &c., and Dublin Quarterly Journal for 1836, p. 198.
[2] On Midwifery and Puerperal Diseases, p. 4.
[3] Memoires de l'Academic Royale de Medicine, Tom. ii. p. 287.
[4] British and Foreign Medico-Chirurgical Review, No. IV. 1848, p. 551.
[5] Practical Observations in Midwifery, p. 289.
[6] Obstetric Medicine and Surgery, p. 299.

verse presentations seen among the large series of 84,000 labours reported in the hospital returns of Lachapelle, Boivin, Clarke, and Collins, and which I have already arranged in a tabular form in a previous page (see table, p. 864).—The comparison of the two last lines in the present table shows, 1. That while presentations of the head occur in 96 per cent. in common obstetric practice at the full time, the same presentations occur only in 70 per cent. among premature labours; 2. That pelvic presentations are nearly eight times more frequent among premature labours than among labours at the full time; and 3. That transverse presentations are nearly ten times more frequent among premature labours than among labours at the end of the usual term of pregnancy.

2.—*Death of the Child.*

The child not unfrequently dies in utero, and before labour begins. In cases in which the death of the fœtus is induced from any cause, during its intra-uterine life, the child, when labour at last supervenes, is apt to be found presenting preternaturally. The explanation of this circumstance has been already given in a previous section. The maintenance as well as the assumption of the usual position of the fœtus with the head downwards and over the os uteri, is an excito-motory, and consequently a vital act; and hence, when the vitality of the fœtus is lost, its position, as a result of that vitality, is liable to be lost also. In other words, the death of the child thus becomes a cause of its malpresentation at the time of birth.

Among the Dublin hospital returns published by Drs Collins, Hardy and M'Clintock, the presentations are noted in 669 cases in which the child was expelled in a "*putrid*" state, and where, consequently, it may be justly assumed that death had occurred sometime before the supervention of labour. The following table shows the relative number of different presentations among these 669 putrid children.

Table of the Presentations of the Fœtus in 669 Cases in which the Child had died in Utero.

Reporter.	Total Number of Cases.	Number of Head Presentations.	Number of Pelvic Presentations.	Number of Transverse Presentations.
Collins . . .	527	438	83	6
Hardy and M'Clintock .	142	115	21	6
Total	669	553	104	12
Proportions among putrid children		82 in 100	1 in 6	1 in 55
Proportions among common labours at full time .		96 in 100	1 in 31	1 in 224

As far, then, as these data go, the difference between the liability to cephalic, pelvic, and transverse presentations between children who have died before labour, and those born alive, may be expressed as follows:—1. Head presentations are 16 per cent. less frequent among dead than among living infants; 2. Pelvic presentations are five times more frequent among dead than among living children; and 3. Transverse presentations are four times more frequent among the former than the latter.

Under the first of the preceding heads we have traced the malpresentation of the fœtus to the occurrence of labour before the reflex movement of the fœtus had sufficiently fixed and established its normal position with the head lowest. Under the second head we have seen the absence or loss of the fœtal reflex movements from the intra-uterine death of the child, leading to the same result. We have now to speak of a series of other causes leading to preternatural presentations, by directly changing the relative forms, either of the fœtus or of the uterine cavity, and thus indirectly forcing the contained fœtus to place itself, by its reflex motions, within the containing uterine cavity in some anormal position, in order, so far, to adapt itself to its normal circumstances, that it may avoid and avert, as much as possible, the irritation of those external excitants, that we have seen capable of acting upon its cutaneous surface and excito-motory system. Under this third head, and as referable to it, we shall consider the influence, in the production of preternatural presentations, of diseases of the fœtus, its states of malformation and monstrosity, the existence of twins, of hydramnios, of spasmodic contractions, and organic disease in the uterine parietes, of placenta-prævia, of deformity of the pelvis, and of original anomalous configuration of the uterus.

3.—*Intra-uterine Diseases of the Fœtus altering its form.*

Various affections of the fœtus in utero terminate in alteration to a greater or less degree of the form and shape of the infant. Ascites, for instance, spina bifida, hydrocephalus (Plate III., Fig. 2) &c., have this effect. Of these intra-uterine diseases the most common is hydrocephalus; and, taking it as the most frequent and noted pathological example, I shall limit to it the proofs that I adduce of the effects of diseases altering the form of the fœtus, having the effect of altering also its position or presentation.

Among sixty-nine cases of intra-uterine hydrocephalus, previously referred to as collated by Dr Thomas Keith,[1] in fifty-nine the cephalic, and in ten the pelvic extremity of the infant presented. One of the sixty-nine cases was a transverse representation.

[1] Thesis on Congenital Hydrocephalus, as a cause of Difficulty and Danger in Parturition. Edinburgh, 1848.

Table of Proportions of different Presentations of the Fœtus in sixty-nine cases of Intra-uterine Hydrocephalus, and in 84,000 *Cases of Common Labour.*

Conditions.	No. of Head Presentations.	No. of Pelvic Presentations.	No. of Transverse Presentations.
Hydrocephalus Cases,	59 in 69	1 in 7	1 in 69
Common Cases, . .	96 in 100	1 in 31	1 in 224

Other diseases of the fœtus and its appendages may perhaps act as causes of its malpresentation—such as inflammatory adhesions of the surface of the fœtus to the surface of the amnion; morbid adhesions and fixation of the umbilical cord upon the body or limbs of the fœtus, &c.; but I am not aware of any recorded series of facts sufficient to prove or disprove this supposition. If such causes act in producing malpresentations, they will probably be found to lead to this effect, by restraining, within fixed limits, the adaptive reflex movements of the fœtus, and by thus preventing these movements from placing the fœtus in its usual position within the uterine cavity.

4.—*Malformations and Monstrosities altering the Form of the Fœtus.*

In the recorded histories of cases of malformation of the fœtus, the details of the birth and presentation of the child are very rarely given. The teratological and anatomical facts connected with such malformed infants, have hitherto principally engaged the attention of medical men, and their obstetric history has been almost entirely neglected. But the data that we do possess, prove sufficiently, in relation to such malformations and monstrosities, that the resulting alteration which they produce in the shape of the fœtus, is, as we would *a priori* expect, a frequent cause of their preternatural presentation in utero and at the time of birth.

In cases of abdominal and thoracic eventration of the fœtus (the "Monstres Cesolomiens," in the classification of St Hilaire), the malformed infant usually presents, according to Klein, not by the head, but by the abdomen, the mass of the displaced viscera lying in front.[1]

From Fried, Harhold, and Klein, Burdach cites cases of imperfect development of the lower extremities in which the fœtus presented, not by the head, but by the malformed parts.[2]

In his work on Teratology, Isidore St Hilaire states it as a general and remarkable fact (*fait remarquable*), "that the presentation by the head is much less common among fœtuses affected with

[1] Deutsches Archiv für Physiologie, tom. iii. (1817) p. 39, &c.
[2] Traité de Physiologie, vol. iv., p. 223.

monstrosities and malformations, than among normal fœtuses. I have (he adds) collected cases, published and unpublished, in sufficient number, to leave no doubt in relation to the truth of this result; but I only add, with dubiety, and as an opinion requiring the confirmation of future observers, that the part which most frequently, and as it were by preference, presents, is the region affected with the malformation."[1]

An immense number of cases of double monstrosity have been placed upon record; but the presentations of the children at birth, are stated in very few instances. The following table contains the results of the presentation in fifteen such cases, including, in fact, all which a cursory examination into this point in teratology has enabled me to ascertain.

Table of the Presentations in Fifteen Cases of Double Monstrosity.

Reporter.	Nature of Presentation.	Reference.
Huron . . .	Feet.	Monthly Journal of Med. Sc. 1847-48, p. 67.
Derien . . .	Feet.	Gazette Médicale de Paris, No. 23. 1848.
Bromilon . .	Head.	Edinburgh Med. & Surg. Journal, vol. lv. p. 435.
Lyell . . .	Feet.	Monthly Journal of Med. Sc. August. 1848.
Moreau . . .	Feet.	Traité des Accouch. tom. ii. p. 309.
Duverney . .	Feet.	Mémoires de l'Acad. Roy. de Médic. tom. i. p. 357.
Peu . . .	Head.	Do. do. do. p. 358.
Evrat . . .	Head.	Do. do. do. do.
Brez d'Angers	Feet.	Do. do. do. p. 359.
Regnoli . . .	Feet.	Do. do. do. do.
Ratel . . .	Head.	Do. do. do. p. 360.
Molas . . .	Head.	Do. do. do. p. 363.
St Hilaire . .	Feet.	Des Anomalies, &c. &c. tom iii. p. 114.
Letouze . . .	Head.	Archives Générales de Médicine. Dec. 1848.
Askham . .	Feet.	Lancet, vol. ii. 1848, p. 235.

It may be observed that out of these fifteen cases, the presentation was footling in nine, and cephalic in six. The instances of footling presentation were consequently extremely prevalent, and in fact preponderated over the other. In none did the arm present.

5.—*Twins.*

When two or more children are contained simultaneously within the uterine cavity, the form of the individual cavity or loculament occupied by each child, varies more or less from the regular ovoid form of the expanded uterus. The cavities which contain the fœtuses in multiparous pregnancy are more or less relatively different in shape from the cavity which contains the fœtus in uniparous pregnancy. A glance at the sketch of the form and contents of the

[1] Histoire des Anomalies, &c., vol. iii., p. 570.

uterus in plural pregnancy, given by Smellie, and copied in Plate III. (Fig. 1) sufficiently confirms this remark. And as the reflex irritations and movements of the fœtus force it to adapt itself to the form of the cavity containing it, malpositions and malpresentations among twin children are more common than among single births. The following table in proof of this statement is constructed from the returns of presentations among twin children, as observed in the Dublin [1] and Edinburgh [2] Lying-in hospitals, and among the patients of the London Maternity Charity.[3]

Table of the Presentations of the Fœtus in 808 *Labours with Twin Children.*

Report.	Total Number of Cases.	Number of Head Presentations.	Number of Pelvic Presentations.	Number of Transverse Presentations.
Clarke . . .	126	73	53	...
Collins . . .	449	309	133	7
Hardy and M'Clintock .	190	122	62	6
Ramsbotham .	772	532	221	19
Simpson . . .	30	23	7	...
Reid	48	25	22	1
Total	1615	1084	498	33
Proportions among twin children . .		67 in 100	1 in 3	1 in 49
Proportions among all births .		96 in 100	1 in 31	1 in 224

In pregnancies with triplets, as with twins, there is the same tendency to malpresentations of the child, and from the same cause. Amongst eighteen children born in cases of triplet pregnancy, and detailed in the reports referred to in the foot-note,[4] the following form the modes in which the children presented at birth. Of the eighteen children, thirteen presented by the head, or seventy per cent.; four children presented by the pelvis, and one among the eighteen was a transverse presentation.

6.—*Hydramnios, or Excess of Liquor Amnii.*

Before the sixth month, the quantity of liquor amnii is very much greater, in relation to the size of the fœtus, than at the full time. In

[1] Clarke. Transact. King's and Queen's Coll. of Physic., Ireland. Vol. i, p. 403. Collins. Practical Treatise, p. 314. Hardy and M'Clintock's Report, p. 329.

[2] Monthly Journal of Medical Science, 1848, p. 335.

[3] Ramsbotham's Obstetric Medicine and Surgery, p. 495. Reid, in London Medical Gazette, Nov. 1835.

[4] Collins. Practical Treatise, p. 340; Hardy and M'Clintock's Report, p. 330; Simpson in Monthly Journal of Medical Science, 1848, p. 339; Davies. Ibid, 1841, p. 448.

fact the quantity surrounding the fœtus is up to that period relatively so large, and the uterine cavity so distended with it, that the fœtus can move about in it readily and rapidly. (See Plate II, fig. 4.) This perfect freedom from large room is, as we have already seen, the principal reason why the fœtus does not require to assume any specific position before that time. But when the liquor amnii remains in great quantity up to the full time, or when the fœtus remains small, the permanence of these earlier and temporary conditions produces anormally at the full time the same result which they normally produced at the fifth and sixth months,—the presentation, namely, is often preternatural.

I have no statistical proofs to offer in evidence of this statement, as no collection of cases of hydramnios at the full time, and of its effects upon the presentation, has, as far as I know, been yet made by any author. But the fact itself that malpresentations are often found co-existing with a preternatural collection of liquor amnii, is admitted by most obstetric writers. "The passive motions of the fœtus (observes a late German author)[1] are easily performed when there is an excess of liquor amnii, and we not unfrequently find with it circumvolutions of the cord around the child, and preternatural positions of the fœtus."

7.—*Spasmodic Contraction in the Uterine Parietes.*

In the latter months of utero-gestation, patients often feel and suffer from spasmodic contractions in the uterine parietes. When these spasmodic contractions are severe, limited to particular portions of the uterine walls, and more permanent than usual, they sometimes so alter the normal form of the uterine cavity as to force the fœtus to assume a new and anormal position. "It is (says Dr Rigby[2]) a well-known fact that cross-births, as they have been called, are frequently preceded by severe spasmodic pains in the abdomen, from which the patient suffers for some days or even weeks before labour has commenced; the uterus is more or less the seat of these attacks, which usually come on toward night-time; and, in some instances, it is felt for the time hard and uneven from irregular contraction. It was the circumstance of this symptom having preceded five successive labours of a patient, in all of which the child had presented with the arm or shoulder, which induced Professor Nægele, when attending her in her sixth pregnancy, to endeavour to allay these cramp-like pains, which had begun to show themselves as severely as on former occasions. Having tried opium by itself, and also in combination with ipecacuanha or valerian without effect, he ordered her a starch injection with twelve drops of tinct. opii every night, as long as she continued to suffer from these attacks; the spasms soon ceased, nor did they appear again during the remainder of the preg-

[1] Busch and Moser's Handbuch der Geburtskünde, vol. iii. p. 361. See also the works of Wigand, Carus, Rigby, &c.

[2] System of Midwifery, p. 169.

nancy, and he had the satisfaction of delivering her at the proper time of a living child, which presented in the natural manner."

8.—*Organic Diseases of the Uterine Parietes, &c.*

By far the most common form of morbid growth in the parietes of the body and fundus of the uterus, is the common and well-known fibrous or fleshy tumour. Pregnancy not unfrequently occurs when the uterine walls are the seat of this diseased structure. Fibrous tumours of the uterus are seldom found in connection with pregnancy so large in size, or so placed towards the internal surface of the uterus, as to alter its cavity from its normal form and make the fœtus assume a preternatural position. But occasionally pregnancy does take place even when the tumour encroaches much upon the uterine cavity; and the resulting form of that cavity may be such as to cause a preternatural presentation. In one case, where a large fibrous tumour existed at the fundus uteri, I found the breech of the infant form the presenting part in labour. Preternatural presentations of the infant in connection with large fibrous tumours in the uterine parietes have been recorded by Dr Beatty,[1] Dr Ashwell,[2] &c. But I am not acquainted with any series of data sufficient to show the degree of liability to preternatural presentations resulting from the presence of fibrous tumours, or such other organic diseases in the uterine parietes, or in the ovaries or neighbouring viscera, as may have the effect of altering the shape of the uterine cavity during pregnancy. This defect, however, is capable of being so far supplied by appealing to the next two causes which I have to enumerate, viz., placenta prævia, and great deformity of the brim of the pelvis. Under both these circumstances, the normal form of the uterus is altered by preternatural organic conditions existing in the one (placenta prævia) within the interior of the uterus, and in the other (deformity of the brim) existing without the uterus,—but both leading to the same result of a liability to preternatural presentation by the same cause, viz., a change in the physical shape of the uterine cavity.

9.—*Placenta Prævia.*

The lower end of the ovoid cavity of the uterus at the full term of pregnancy is principally formed after the sixth month, and from the development of the cervix uteri. In placental presentations this lower or narrow end of the uterine ovoid does not acquire its normal size and form in consequence of the implantation of the placental mass upon its interior. Hence, in these cases, the whole normal ovoid figure of the cavity of the uterus is altered in shape, and in adapting through its reflex movements its form to the altered form of the uterus, the fœtus is liable to assume and maintain a preternatural position.

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[1] Dublin Journal of Medical Science, vol. xvii., p. 414.
[2] Treatise on Diseases of Women, p. 338.

The data in the following table afford ample evidence of the great frequency of preternatural presentations of the fœtus in cases of implantation of the placenta over the os and cervix uteri, a fact scarcely, if indeed at all, alluded to by obstetric writers. The data are derived, 1. From the published reports of Madame Lachapelle, and Dr Collins, the only two authors who seem to have stated the presentations of the fœtus in *all* the cases of placenta prævia which they record; and, 2. From a long series of unpublished cases of placenta prævia in the practices of Dr Wilson of Glasgow, and of Dr John and Dr Francis H. Ramsbotham of London, with the manuscript notes of which I have been very kindly favoured.

Table of the Presentations of the Fœtus, in 366 *Cases of Placenta Prævia.*

Reporter.	Total No. of Cases.	No. of Head Presentations.	No. of Pelvic Presentations.	Number of Transverse Presentations.
Lachapelle, . . .	17	14	2	1
Collins, ...	12	8	3	1
Wilson, ...	29	25	4	...
J. Ramsbotham, .	124	103	11	5
F. H. Ramsbotham,	184	155	19	10
Total	366	310	39	17
Proportions in cases of Placenta Prævia		85 in 100	1 in 9	1 in 21
Proportions among common labours at full time, . . .		96 in 100	1 in 31	1 in 224

The comparison of the two last lines in the present table, shows, 1. That, while presentations of the head occur in 96 per cent. in common obstetric practice at the full time, the same presentations occur in only 85 per cent. among cases complicated with placental presentation; 2. That pelvic presentations are more than thrice as frequent among cases of placenta prævia as among ordinary labours at the full time; and 3. That transverse presentations are ten times more frequent in cases of placental presentation than among ordinary labours at the full term of pregnancy.

10.—*Distortion and Contraction of the Brim of the Pelvis.*

Few obstetric authors have adverted to pelvic contraction and distortion, as a cause of preternatural presentations. Yet its occasional operation is certain.

"Mr Barlow,"[1] observes Dr Ramsbotham,[2] "states that he is induced to believe preternatural presentations are far more frequently met with under distortion of the pelvis, than when that organ is well-

[1] Essays on Surgery and Midwifery, p. 348.
Obstetric Medicine and Surgery, p. 299.

formed. This remark (Dr Ramsbotham continues) coincides with my own observation;" and in another part of his work,[1] he further remarks, "Transverse presentations are by no means comparatively more frequent among the poor than those in affluent circumstances; but some women seem to be naturally predisposed to this irregularity. Thus, a patient whom I attended in all her labours, out of five children which she has borne, has been the subject of four transverse presentations; her pelvis is slightly distorted at the brim. And another woman, now dead, who always, under pregnancy, became a patient of the Royal Maternity Charity, in twelve labours suffered seven shoulder presentations. I delivered her myself five times under these difficulties, and my father twice. This person also possessed a contracted pelvis."

When the pelvic contraction is great, and involving the brim, and hence interferes much with the development and shape of the lower end of the uterus, its influence in leading to preternatural presentations of the fœtus is well marked, and capable of statistical proof. The operation which requires to be had recourse to when the pelvis is thus very highly deformed, is the Cæsarean Section. In most of the cases in which this operation is recorded to have been practised, the presentation of the fœtus is not mentioned, and in 44 instances only have I been able to find, on a casual examination, any adequate data relative to the presentation of the child. References to these 44 cases are given in the accompanying foot-note.[2] Among the 44 infants, 30 presented by the head, or about 68 per cent.; 8 of the children, or nearly one in every 5, were pelvic presentations; and 9, or one in about every 7, presented trans-

[1] Obstetric Medicine and Surgery, p. 334.

[2] The following are references to the cases in which the head is mentioned as presenting:—Smellie's Midwifery, vol. iii., p. 423; MMS. Lectures of Professor Young; First letter of Dr Hull of Manchester, three cases; Hull's Translation of Baudelocque's Essay on Cæsarean Section, three cases; Edinburgh Medical and Surgical Journal, vol. xxiv., p. 296, also p. 304; vol. xxx., p. 53; vol. xxxi., p. 443; Medico-Chirurgical Transactions, vol. ix., p. 13; British and Foreign Medical Review, vol. x., p. 572; vol. xi., p. 531; vol. xiii., p. 545; Lancet, June 1840; Northern Journal of Medicine, vol. iii., p. 247; Meigs' Obstetrics, p. 531; American Journal of Medical Science, vol. i., p. 269, two cases; vol. xii., p. 386; Monthly Journal of Medical Science, 1841, p. 218; 1842, p. 425; 1843, p. 160; 1844, p. 358; 1845, p. 323, p. 887; 1846, p. 309; 1847, p. 130. In the succeeding cases, some part of the pelvic extremities is mentioned as presenting:—Hull's Translation of Baudelocque's Essay, two cases; Medico-Chirurgical Transactions, vol. vii., p. 264; Edinburgh Medical and Surgical Journal, vol. viii., p. 11; vol. xvii., p. 106; British and Foreign Medical Review, vol. ii., p. 271, and a second time in the same case; American Journal of Medical Science, No. xxxii. 1835, p. 546; Monthly Journal of Medical Science, 1843, p. 835. The following are references to cases in which some part of the superior extremities of the infant is stated to have presented:—Hull's Translation of Baudelocque's Essay, four different cases; and Edinburgh Medical and Surgical Journal, vol. xxii. p. 248.

versely. Or we may state these results as contrasted with the presentations in cases of common labour in a tabular form, as follows :—

Table of Proportions of Different Presentations of the Fœtus in 44 *Cases of Cæsarean Section, and in* 84,000 *Cases of Common Labour.*

Conditions.	Proportion of Head Presentations.	Proportion of Pelvic Presentations.	Proportion of Transverse Presentations.
In Cæsarean Sections,	68 in 100	1 in 5	1 in 9
In Common Labours,	96 in 100	1 in 31	1 in 224

In speaking of pelvic contraction as a cause of malposition of the infant, Dr Ramsbotham seems to doubt whether it be possible to explain in what manner such a cause could lead to the effect which he attributes to it. "*How* (he observes) a contraction of the pelvic brim can influence the position of the fœtus in utero, it is difficult to explain or even to imagine."[1] But the solution of this apparent difficulty is sufficiently easy upon the principles which I have endeavoured to state in the present essay. A contracted brim, more especially when that contraction is great, directly interferes with the expansion of the lower part of the uterus, forces the uterus, in this way, into a preternatural form, and forces the fœtus to place itself by its reflex actions in preternatural positions, in order to lie adapted to that form. When the pelvis is much deformed by mollities ossium, or rickets, as in cases requiring Cæsarean section, other additional circumstances aid in leading to still greater alterations in the form of the uterus. The trunk is generally also deformed, the ribs approached towards the pelvis, and the abdominal cavity diminished in depth. During its development, the expanding uterus is not only prevented from developing itself downwards, but it is also prevented from developing itself to its usual extent upwards. It grows and expands forwards and laterally, or, in other words, in the directions in which there is least resistance to its increase in volume. Under these restraints and conditions it expands more laterally and antero-posteriorly, and less in the longitudinal direction, than is natural. It tends to assume such a form as is represented in plate III. (Fig. 3.) This globular form, simulating the globular form of the uterus at the fifth month, is perhaps the cause why transverse presentations are so peculiarly common in instances in which the brim of the pelvis is so much deformed as to require Cæsarean Section.

11.—*Irregularity in the Configuration of the Uterus.*

Like every other organ of the body, the uterus occasionally shows, in particular individuals, deviations to a greater or less extent from

[1] Obstetric Medicine and Surgery, p. 229.

its normal standard form. Some of its more marked deviations in shape and development interfere so much with its functional activity as to be a cause of sterility. Other changes found in its configuration do not necessarily prevent conception, but when pregnancy occurs along with them, the mis-shaped uterus, during its growth, continues sometimes to remain so far altered in form and shape from the natural ovoid figure of the expanded organ, that the adaptive reflex movements of the fœtus necessarily place it, in such a cavity, under a preternatural position. Jahn,[1] Meissner,[2] Meckel,[3] and others,[4] have described and collected many cases in which the uterus showed various anomalies in form and configuration under different original deviations of form or development. But without entering into an account of these individual anomalies and their effects, I will content myself with illustrating, by a single case, the mode of action of the cause in question, in the production of malpresentations:—

A patient in her two first labours had the arm of the child presenting. In her third labour the same presentation recurred. On examining the uterus before delivery, Dr Lecluyse found it of an anormal configuration. It was short and depressed in its vertical direction, but large and expanded towards either side. It was ellipsoid instead of pyriform in shape; and had thus gained in lateral width what it had lost in perpendicular height. Its long axis was from side to side; and in placing itself under its reflex movements, so as to adapt the long axis of its ovoid mass to the long axis of the uterus, the fœtus, of necessity, was placed and presented transversely. After the uterus was emptied in this case, the organ still presented the same deformity of shape.[5]

The preceding case refers to an original deviation in shape in a single uterus. In cases of duplicity of the uterus impregnation sometimes takes place. The pregnant side or horn occasionally presents, when distended, such a degree of irregularity of shape, as to force the fœtus into some corresponding irregularity in position. Geiss reports a case of duplicity of the uterus, where the presentation of the fœtus was transverse.[6] In describing such cases, most authors have omitted to state the nature of the presentation.

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[1] Schlegel's Syllog. operum ad Artem Obst., vol. i., p. 257.

[2] Frauenzimmerkrankheiten, Band, i., p. 535, *et seq.*

[3] Pathologischen Anatomie, Band, i., p. 673.

[4] Voigtel's Handbuch der Pathol. Anatomie, vol. iii., p. 1-455, &c. Colombat's Traitè des Maladies des Femmes, vol. i., p. 200. See also the writings of Hunter, Rokitansky, Tiedemann, Cassan, Moreau, &c.

[5] Annales de la Société d'Anvers, 1845, p. 89. London Medical Gazette, vol. xxxv., p. 847. In Plate III., Fig. 4, I have copied from Chailly the figure which he gives of the shape of the uterus under a pelvic presentation. An uterus of such a configuration (with the ovoid reversed—the largest extremity lowest) would, according to the principles laid down in the text, lead to the pelvic instead of the cephalic position of the fœtus.

[6] Rust's Magazin für die gesammte Heilkunde, vol. xx. (1825) p. 8.

Perhaps under the present head of causes I should notice the supposed effects of obliquities and flexions of the uterus in the production of malpresentations. These obliquities and flexions, however, when they exist before pregnancy, generally become spontaneously rectified as utero-gestation advances; and do not thus ultimately interfere with the shape of the uterine cavity, or, through its shape, with the position of the child. But in those rarer cases where the configuration of the uterine cavity remains altered by their influence, particularly in its lower or cervical part, this altered configuration of the containing organ may occasionally affect the corresponding position and mode of presentation of the contained fœtus.

12.—*Mechanical and Casual Displacements of the Fœtus.*

Some authors have considered accidental displacements of the fœtus from falls, carriage exercise, strong emotions, &c., as one of the principal causes of its malpresentations, whilst others have doubted or denied the possible influence of their operation.

Up to the sixth or seventh month of pregnancy the fœtus is in general relatively so small, and the surrounding quantity of liquor amnii so abundant, that the child may be readily displaced from the position which it happens at the time to occupy, by any violent movements or succussions on the part of the mother. When the fœtus enlarges more, and ultimately requires, in consequence of its increased length, to place its long axis parallel with the long axis of the uterus—the possibility of its being accidentally turned completely round in the uterine cavity, is prevented by the physical fact of the long axis of its head and body being longer than the width or short axis of the cavity, in which it would make this supposed evolution. The usual length of the fœtal ovoid at the full time is about twelve inches, and the usual lateral width of the uterus about eight or nine inches, so that the former could not readily turn within the latter. But when the fœtus is smaller, the quantity of liquor amnii considerable, and the shock given to the maternal trunk in any degree great, a complete displacement of the fœtus may no doubt occur, so as to change, for example, a cephalic into a pelvic presentation; and the return from the pelvic to the primitive cephalic position is subsequently prevented by the same physical relations as guarded against its original occurrence. My friend, Mr Cockburn, informs me that a patient of his, the mother of eight children, had seven of the children presenting naturally, and one by the pelvis or feet. When well advanced in pregnancy with the child that ultimately presented preternaturally, she happened to be thrown down with a severe fall upon her back. To Mr Cockburn she declared immediately afterwards her own conviction, that she felt the child turn round at the time of the accident, and that it would be born in a wrong position; as ultimately proved to be the case.

The presenting head of the fœtus sometimes happens to become

mechanically displaced at the commencement of labour, and a primary normal presentation becomes thus changed into a secondary preternatural presentation. This has been principally observed to happen when there was some obstruction at the brim of the pelvis, either in the bones or soft parts; the head when driven obliquely against this obstruction by the first uterine contractions, has slipped aside, and the neck, or rather shoulder, come to present over the os uteri. Denman,[1] Merriman,[2] Burns,[3] &c., have described and directed attention to cases of secondary malpresentation of this kind. Where the quantity of liquor amnii at the time of labour continues great, and the child is small and mobile, it may occasionally, though rarely, at the full time, become repeatedly displaced in different ways under the pressure of the contracting uterus, and the reaction of the opposing maternal structures. Baudelocque relates a case in which "during a labour of thirty-six hours, the child presented successively, and several times over, the head, the feet, the back, the shoulder, or one of its sides; and, at the instant the membranes burst, the belly, the knees, and a loop of the cord."[4]

The various minor modifications of the position of the head and pelvis met with at the commencement of labour, are, for the most part, owing to slight mechanical changes produced in the commencement of labour by the compressions of the uterus upon the presenting parts, during the first pains, and the reaction of the maternal parts upon those parts of the child.

Section V.—Relative Influence of the Specified Causes in the Production of the different Genera of Preternatural Presentations.

There is considerable variety traceable in the power and frequency with which the different causes of preternatural presentations that I have described act in producing these different presentations. They not only produce malpresentations, with different degrees of certainty, but some produce one form of malpresentation more readily

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[1] "Having been called to women in the beginning of labour, and finding, by an examination, that the head of the child presented, I have left them for several hours, till the first changes were naturally made. When I have examined them on my return, I have found the arm of the child presenting, the head being departed out of my reach."—*Denman's Introduction*, p. 495.

[2] Medico-Chirurgical Transaction, Vol. x., p. 62.—In a case of tumour of the soft parts obstructing delivery, the head, after presenting during a labour which was allowed to last for three days, was removed under the action of the pains, and the right shoulder came to present.

[3] "It is a fact well ascertained, that although the head may be felt distinctly in the commencement of labour, yet when the membranes break it may be exchanged for the shoulder or some other part. I have been informed of a case where the shoulder was exchanged for the head, and Joerg seems to have met with the same circumstance."—*Burns' Principles of Midwifery*, 9th Edition, p. 411.

[4] System of Midwifery. Heath's Translation, Vol. i., p. 264.

than another. The comparative effects in this respect of certain of the causes which I have enumerated cannot be ascertained, because we have no statistics of the results to guide us. In reference to others, I have adduced statistical information sufficient to enable us to calculate imperfectly, and in a general way, their relative effects. The following table shows the comparative frequency with which six of the causes of preternatural presentation, which I have above considered, respectively led to the occurrence of preternatural presentations of the fœtus :—

Table of Proportion of Preternatural Presentations under Six specified Causes.

Conditions.	No. of Cases.	No. of Malpresentations.	Proportion of Malpresentations.
Premature labour .	1087	325	1 in 3
Cæsarean section .	44	14	1 in 3
Twin pregnancy .	1615	531	1 in 5
Death of the child .	669	116	1 in 6
Hydrocephalus . .	69	11	1 in 6
Placenta prævia . .	366	56	1 in 7
Common practice .	84,684	3128	1 in 27

But the causes producing preternatural presentations do not always produce the different forms of these malpresentations in the same relative proportion. Some causes predispose more to pelvic than to transverse presentations, and others the reverse. The following table gives the relative frequency of pelvic presentations, under the influence of the six causes enumerated :—

Table of Proportion of Pelvic Presentations under Six specified Causes.

Conditions.	No. of Cases.	No. of Pelvic Presentations.	Proportion of Pelvic Presentations.
Premature labour .	1087	279	1 in 4
Cæsarean section .	44	9	1 in 5
Twin pregnancy . .	1615	498	1 in 3
Death of the child .	669	104	1 in 6
Hydrocephalus . .	69	10	1 in 7
Placenta prævia . .	366	39	1 in 9
Common practice .	84,684	2750	1 in 31

It will be observed, that the order of relative frequency of pelvic presentations under these six specified causes, differs from the order of relative frequency of preternatural presentations in general under the same causes, and as given in the table immediately pre-

ceding. The order of relative frequency of transverse presentations from the same six causes, is very different from either of the above, as the facts in the following table will prove :—

Table of Proportion of Transverse Presentations under Six specified Causes.

Conditions.	No. of Cases.	No. of Transverse Presentations.	Proportion of Transverse Presentations.
Premature labour .	1087	46	1 in 23
Cæsarean section .	44	5	1 in 9
Twin pregnancy . .	1615	33	1 in 49
Death of the child .	669	12	1 in 55
Hydrocephalus . .	69	1	1 in 69
Placenta prævia . .	366	17	1 in 21
Common practice .	84,684	378	1 in 224

The preceding tables demonstrate, that some of the causes which are liable to lead to preternatural presentations, are more liable to produce pelvic than transverse presentations; or the contrary. And if we venture to inquire into the grounds of this difference of effect, we will find it existing, I believe, in the peculiarities of form, and, consequently, of adaptation, which the causes in question produce, either upon the containing uterus or the contained fœtus. For example, let the cause producing the malpresentation be liable to change the form of the uterus, so that this organ be anormally increased in width, or in lateral or anterior development,—and the result will be a tendency to the frequent occurrence of transverse presentations of the fœtus; for the long axis of the ovoid mass of the fœtus becomes placed transversely in order to be adapted to the misshapen uterus, the long axis of which in these supposed conditions, is more lateral or transverse than vertical. We have often this form of the uterus in Cæsarean section, for reasons that I have already mentioned under that head (see also plate III. fig. 3); and in placenta prævia, where the placental mass fills up more or less the cavity of the cervix, and prevents its full development. Under the action of both these causes, the preceding table shows that transverse presentations are especially common. Again, in hydrocephalus of the fœtus, transverse presentations are rare, but pelvic presentations common; because the irregularity of form in the contained body is such, that the child may be adapted to the containing uterus, far more readily in a pelvic than in a transverse position.

SECTION VI.—ON THE RECURRENCE OF MALPRESENTATIONS IN SUCCESSIVE PREGNANCIES IN THE SAME MOTHER; AND ITS CAUSATION.

Various authors mention the aptitude to the recurrence of preternatural presentation in different pregnancies in the same mother; and the fact is one well known and established in obstetric pathology.[1]

Sometime ago I delivered by turning, for presentation of the arm and cord, a patient of Mr Cunningham. It was the sixth child, and the only one born alive. With one exception the presentations in her five previous labours had been all preternatural, and most of them accompanied with prolapsus of the cord. Dr Collins[2] mentions an instance in which as many as nine preternatural presentations occurred in the same mother.

Occasionally the presentation in the same mother though always preternatural, is not always the same presentation. For example, Dr Lee[3] mentions a woman with distorted pelvis, in whom the inferior extremities presented in the first and second labours, and an arm in the fourth; and another who had eight preternatural labours in succession, the arm presenting in the first four or five, and the nates or inferior extremities in all the others. In other instances there is a regular recurrence of exactly the same type of preternatural presentation in several successive labours. A case is recorded by Walther[4] in which the shoulder presented in six successive pregnancies, in consequence of a malformation of the uterus. Madame Renard[5] has recently published an instance, in which a patient had the right shoulder presenting in five successive labours; the pelvis was deformed.—Even more rare forms of complex preternatural presentation sometimes recur regularly in different labours in the same patient. Dr Hamilton was in the habit of mentioning, in his lectures, a case in which presentation of the breech and one foot occurred in six or seven successive labours in a patient of Mr Moir.

The recurrence of the same, or different malpresentations in successive pregnancies in the same mother, has generally been looked upon as a problem more difficult to explain and solve, than the

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[1] "Though preternatural presentations (says Denman) seldom occur when they are dreaded and expected, it is remarkable that some women are peculiarly subject to them; not once only, which might be considered the effect of some accident, but exactly to the same presentation, whether of the superior or inferior extremities, in several successive or alternate labours."—*Introduction to the Practice of Midwifery*, p 467. See also the works of Ramsbotham, Burns, Meigs, &c.

[2] Practical Treatise, p. 40.

[3] Lectures on Midwifery, p. 327.

[4] Journal de Chirurgie, tom. iii. p. 59.

[5] Gazette des Hôpitaux, Avril 1849.

more simple fact of the occasional occurrence of a preternatural presentation of the fœtus. And some authors seem to consider the phenomena as almost beyond the reach of explanation. "I feel wholly unable" (observes Dr Lee[1]) "to explain the cause of preternatural presentation occurring repeatedly in the same individuals, as described by Dr Denman and other systematic authors." "It is" (he elsewhere observes[2]) "very difficult, or impossible, to explain why the head of the child does not invariably present in labour, or assign a cause for the same woman having preternatural presentations in several successive labours, the head of the child being rarely, if ever, in them the presenting part."

The doctrine of the causation of the positions and presentations of the fœtus, which I have attempted to develope in the present memoir, affords a simple key to this obstetric enigma.

Some of the causes producing malpresentation, in the way described, are occasional or accidental only; and are not liable to recur in other pregnancies in the same mother. Hence, under the action of these causes, a malpresentation of the fœtus will occur in one or two labours only in the same patient, and not in all. To this set of instances belong the presence of twins; death, or disease, and malformation of the fœtus; prematurity in the labour; mechanical accidental displacements of the fœtus; and changes in the configuration and form of the uterus from spasms in its parietes, from the cervical or other anormal implantation of the placenta, and perhaps from other conditions which I have not taken time to trace out in the preceding observations, as fœcal accumulations in the colon, compression of the abdomen, and other such causes as may produce unequal pressure on the external surface of the uterus, and thus change the shape of its internal cavity,—causes, some of which are capable of being prevented and treated, and their effects on the presentation of the child thus averted. But other causes of malpresentation of the fœtus are not occasional and accidental, but permanent and constant. They are not limited to one pregnancy, but present in all. They are anatomical conditions, either existing throughout the whole life of the mother, or recurring with each successive labour. For example, the lower part of the ovoid cavity of the uterus may be altered from the normal configuration and shape in each successive pregnancy, by deformity of the brim of the maternal pelvis,—the deformed brim preventing its full development, forcing it always to assume a particular type and form, and this type being one which obliges the fœtus to assume some special or preternatural position, in order to adopt and accommodate itself to the existing special and preternatural form of the uterine cavity. I have already cited from Dr Ramsbotham, Renard, &c., cases of deformity of the brim of the pelvis, leading in this way to a succession of preternatural presentations of the child

[1] Clinical Midwifery, p. 117. [2] Lectures on Midwifery, p. 327.

in the same mother. Again, if the uterus itself is misshapen and malformed, a similar result follows. In each successive pregnancy there is the same constant deviation from the normal ovoid form of the uterine cavity, and as a consequence, the same deviation from the normal adaptive position of the fœtus. In the instances which I have quoted above from Walther and Lecluyse, of the recurrence of the same malpresentation in several successive pregnancies in the same mother, the cause of these malpresentations was an irregularity in the configuration of the uterus. The same organ,—as the mouth, nose, hand, &c., &c., is liable, when irregular in form, to present the same irregularity in several members of the same family. The same no doubt holds good in regard to the uterus also. Dr Keiller has informed me of an instance of preternatural presentations recurring in different pregnancies in twin sisters. It appears to me that this curious fact will find its solution in some peculiarity in the form of the uterus repeated in the two sisters.

If the preceding remarks had not already extended far beyond the limits intended, I would have proceeded to show that the *positions* of the head, &c., of the child, in relation to the circumference and different diameters of the brim of the maternal pelvis, are, like the presentations, regulated in their ultimate analysis by the reflex or excito-motory actions of the fœtus. It is only by reference to the reflex excitations and motions of the fœtus, that we can explain, for example, the rarity of the positions of the head, &c., in the direct diameters of the brim, their frequency in the oblique, and especially in the right oblique diameter,—and the greater relative proportion of occipito-anterior than of occipito-posterior positions of the cranium.

ARTICLE II.—*Note, in reference to the Case of Cystic Oxide Calculus, reported in the preceding Number of the Journal.* By JAMES MILLER, F.R.S.E., Professor of Surgery in the University of Edinburgh, &c. &c.

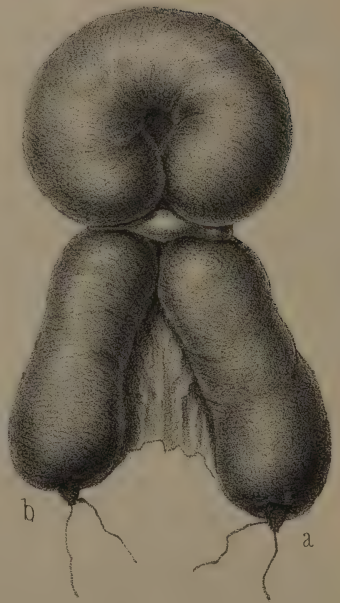
As I had hoped, the publication of this case has directed attention to Mr Liston's collection of urinary calculi, preserved in University College Museum. And, through the kindness of Dr Walshe, I am enabled to complete the narrative, very satisfactorily. The missing link of the chain was—the nature of the stone which Mr Liston had extracted, fifteen years before the second lithotomy.

In the article "Products, Adventitious," contributed by Dr Walshe to the "Cyclopædia of Anatomy and Physiology," there is, at page seventy-nine, an admirable wood-cut, exhibiting the characters of a calculus, marked thus—"Pure Cystin Calculus; long diameter nearly $2\frac{1}{8}$ inches; short ditto, $1\frac{3}{8}$. Univ. Coll. Mus." And on referring to the catalogue, this stone is found to belong to "Charles Irvine, æt. 37; cut at Edinburgh in 1834."

Fig. 1

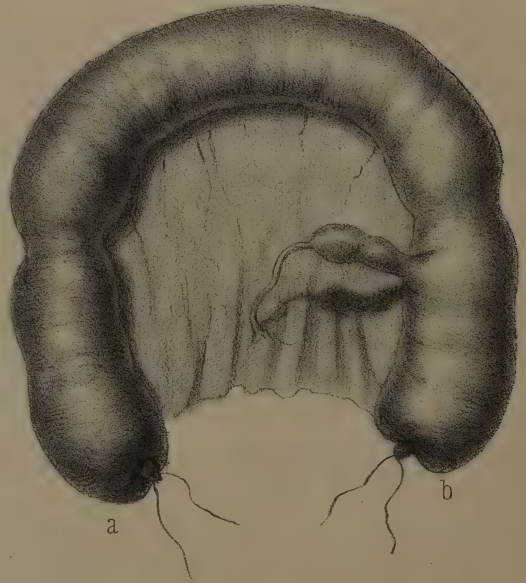
Anterior view of the strangulated intestine & Stricture
a. Gastric extremity
b. Rectal extremity

Fig. 2



Posterior view of the strangulated intestine & Stricture
a Gastric extremity
b Rectal extremity

Fig. 3.



An Appearance of the natural relations of the Diverticulum to the intestine
a. Gastric extremity
b. Rectal extremity

"This identification of your patient's former calculus," says Dr Walshe, "greatly adds to the interest of the case. Such a continued and determined tendency to the precipitation of cystin has not hitherto, so far as I know, been observed—or, perhaps, it would be better to say, recorded."

I may add, that I heard lately of the patient from Dr Maculloch of Dumfries, under whose care he now is. The Doctor reports him to be very well, but often showing strong tendency to *phosphatic* deposits; which are readily corrected by the ordinary means. No doubt, the injury of the spine continues to affect the kidneys unfavourably.

ARTICLE III.—*A Remarkable Case of Strangulation, caused by a Diverticulum.* By WILLIAM PIRRIE, F.R.S.E., Regius Professor of Surgery in Marischal College and University of Aberdeen; one of the Surgeons to the Royal Infirmary; and formerly Lecturer on Anatomy in the University.

DAVID WHITE, seventeen years of age, a fine-looking young man, had always enjoyed excellent health until the 22d of April; on which day, while walking in the street, he was suddenly seized with sickness, vomiting, and violent pain in the abdomen; the pain being constant, but attended with frequent paroxysms of aggravation.

At the commencement of the attack the belly was not tumid, nor was there any tenderness on pressure—on the contrary, the patient had an inclination to compress the belly with his hands, especially during the paroxysms of pain, and to turn himself round in bed; whereas, in the course of five or six hours, the abdomen became tender to the touch, and ultimately so much so that the slightest touch occasioned pain and vomiting, the belly gradually became tumid, and the patient was obliged to preserve his body as motionless as possible in order to prevent the aggravation of pain.

Such is the history of the symptoms, as given by my friend, Dr George Morrison, who attended him from the commencement of the attack.

My colleague (Professor M'Robin) and I saw the patient for the first time twenty-four hours previous to his death. His symptoms, when we saw him, were—distension of the abdomen; constant violent pain, with paroxysms of aggravation like the tormina of ileus; tenderness on pressure; sickness, urgent vomiting of a greenish liquid; no stool since the commencement of the attack; pulse 130, small and feeble; features collapsed, and the countenance expressive of great exhaustion.

These symptoms continued for twenty-four hours, without undergoing any material change, death taking place within sixty hours from the commencement of the attack.

The suddenness with which the symptoms appeared, their history, and the absence of swelling in any of the usual sites of hernia, produced a strong impression on the mind of Dr Morrison, Professor M'Robin, and myself, that the symptoms were caused either by intus-susception, or by internal hernia, or by some internal cause of strangulation; in short, that it was a case of enteritis, occasioned by some internal mechanical cause.

I made a post-mortem examination twenty-four hours after death, in presence of the medical gentlemen above-mentioned. On opening the abdomen, a small quantity of a sero-sanguineous fluid was found in the cavity of the peri-

toneum; the stomach, and a great part of the small intestine, were greatly distended with flatus; the distended portion of the intestine was much inflamed, and there were slight adhesions of lymph in some parts. The lower third of the ileum and the whole of the great intestine were collapsed, and of a perfectly healthy appearance. On displacing some of the convolutions of the small intestine, a portion of the ilium, twelve inches in length, was found greatly distended, strangulated, and in a state approaching to gangrene; the part of the intestine to the gastric extremity of the strangulated portion being violently distended and inflamed, and that to the rectal extremity being comparatively collapsed, and of a perfectly healthy appearance; the strangulation being effected by a diverticulum of the ilium, having very peculiar relations and connections.

No. 1 of the accompanying drawings gives a front view of the strangulated intestine and stricture. No. 2 gives a posterior view. No. 3 presents an appearance of the natural relations of the *diverticulum*, obtained after emptying the intestine, and withdrawing it from underneath the diverticulum by which it was strangulated. (See Plate).

The diverticulum, at its entrance into the intestine, was furnished with a semilunar valve, formed by a doubling of the mucous membrane; but the diverticulum communicated with the intestine, and contained intestinal matters. It was an inch and a-half in length, and terminated in a slightly-dilated cul-de-sac; from the extremity of which a membranous band was sent of, one extremity of which was evidently continuous with the serous coat of the diverticulum, and the other as evidently not merely attached to, but becoming continuous with, the anterior lamella of the mesentery. Through the aperture formed by the diverticulum, mesentery, and the portion of the intestine from which the diverticulum is continued, twelve inches of intestine had passed at the commencement of the attack, and became strangulated.

The above cannot but be regarded as a very curious and extremely unusual case—not that it is rendered so by the mere existence of a diverticulum, which is itself a rare formation, but the presence of a diverticulum being the occasion of strangulation.

So far as I am aware, there is only on record one case bearing a striking resemblance to the above. I allude to a preparation in the museum of St Bartholomew's Hospital, of which I subjoin the description as extracted from the published catalogue of that valuable collection:—

"Portion of small Intestine, from which a Diverticulum is continued.—The extremity of the diverticulum is adherent to the contiguous part of the mesentery, so as to form a circular aperture or ring. Through this aperture a portion of intestine, twelve inches long, passed, and became strangulated. The patient, a lad subject to constipation, died four days after signs of strangulation of the intestine."—*Catalogue of the Museum of St Bartholomew's Hospital.*

It will, however, be seen that, in one point at least, the cases differ: in that of St Bartholomew's Museum the fundus of the diverticulum is adherent to the contiguous portion of the mesentery—whereas, in the case I have described above, the fundus of the diverticulum is connected with the mesentery by the serous coat becoming contracted into a membranous band, and expanding into the anterior lamella of the mesentery. In the catalogue of the museum of the Royal College of Surgeons in Ireland, there is a description of a preparation in which a diverticulum, four or five inches long,

caused strangulation of several feet of intestine; but the relations of the diverticulum, as well as the mode in which it effected strangulation, were very different from the cases described above.

ARTICLE IV.—*Case of Inversio Uteri induced by Polypus, in which Extirpation was Successfully performed with the Knife.* By C. H. HIGGINS, M.D., F.R.C.S.L., Corresponding Member of the Edinburgh Obstetric Society.

[*Read before the Edinburgh Obstetric Society, April* 11, 1849.]

Juliana Virgin, æt. 17, was received into the Taunton and Somerset county hospital, on the 20th of April 1843, for a supposed cancerous affection of the os and cervix uteri. The patient, an unusually dark brunette, of bilious temperament, was unmarried, and had never borne a child or suffered miscarriage. She had of late been occupied as a maid-servant, prior to which she had lived at home with her mother. She represents her general health to have been excellent until the present series of ailments commenced,—an account confirmed by the mother.

She states that six months ago she experienced a deviation from health, accompanied by the appearance of the catamenia for the first time. That the discharge lasted three or four days, and upon its cessation she recovered her wonted health. That she continued free from all sense of illness until about a month before christmas 1842, when the menstrual flow re-appeared, but on this occasion in a violent degree, and persisted for nearly a month, in consequence of which she applied at the hospital and became an out-patient under my excellent colleague, Dr R. Burridge. From Dr Burridge I learn that she attended regularly for about three months, that her symptoms were those of menorrhagia, accompanied with unusually great pain and tenderness above the pubic region. That she gradually improved, and an interval was again established between the monthly periods, but that during these intervals the menorrhagia gave place to a fœtid leucorrhœal discharge. Her general health and appearance, however, were much restored, and she ceased to attend at the hospital for six weeks before her admission into it, in April 1843. By her own account she continued in this improved condition, capable of attending to the duties of her situation, until about three weeks since. At this time, however (early part of April 1843), she experienced a most severe menstrual period, attended with very violent and continuous bearing down pains in the natural situation of the womb, and in the back; which, from her description, clearly resembled those that characterise the expulsive efforts of the uterus during the latter stage of labour. These, according to her, tremendous pains, persisted for some hours, and then suddenly ceased, and she immediately discovered a large substance filling up the vagina, and projecting beyond the labia. She suffered greatly from dragging pain, and in the course of a day or two a considerable fœtid sanguineous discharge came on, and she became too ill and depressed for her menial duties. She was now seen by the usual medical attendant of the family—under whose care she remained for some days, and, at his suggestion, she was at length sent into the hospital, and placed under my care.

I saw the patient, for the first time, on the day of her reception, and found her with a countenance expressive of great depression and suffering. The pulse very low, but quiet and regular. The usual excretions were said to be performed naturally, and her appetite was tolerably good. She chiefly complained of unceasing pain of a *dragging* character at the lower part of the abdomen and back, and consequent complete loss of rest. Upon examination, in the presence of two or three of my colleagues, there was found, occupying the vagina, and protruding beyond the external genital fissure, an irregularly shaped

mass, about the size of an adult human heart, and consisting of two distinct, but connected portions. A firm fleshy substance of pyramidal form, covered at its base by a nodular fungoid-looking insensible growth, of a spongy consistence—somewhat resembling the cauliflower excrescence of Dr Joseph Clarke.

The growth, here and there, presented a broken down and ulcerated appearance, giving out a copious fœtid and bloody discharge. Upon more closely investigating the nature of the connection which existed between these two apparently dissimilar portions of the tumour, a firm tough pedicle, half an inch in diameter, and somewhat less than this in length, was perceived to arise from the centre of the former body, and to lose itself in the other mass; of which mass, the greater portion merely overlay its more solid neighbour, as if it had been flattened and pressed out upon it.

The opinion formed of the case at this stage of the examination, was that a double fibrous polypus had been generated in the cavity of the uterus, and extruded by a sort of slow labour, at a menstrual period when the womb would be more disposed to take on such an excited action; and that the more distant half of the tumour had become decomposed and broken down by exposure to the air and friction. In order, however, to arrive at a more precise diagnosis of the case, I proposed to remove the looser and broken portion at once, as it was clear that the true nature of the ailment could only be determined by a more intimate examination of the solid part of the mass, which was at present obscured to a considerable extent by the other. It was easily lifted up and scraped away with the handle of a scalpel, until the pedicle was reached. This being of a firm fibrous structure, required to be divided. The whole being in this way got rid of, the now denuded fleshy body was seen to be an uniform reddish, pyramidal substance, somewhat flattened from before backwards, about the size of a turkey's egg, firm to the touch, very sensitive, and having its narrow end high up in the vagina. The surface was roughish, and bathed in a fœtid bloody secretion, with here and there a slight abrasion, within the area of which might be seen traces of fibres, resembling muscular fibre.[1] Upon making a vaginal examination I discovered, instead of a dilated os and cervix uteri, as was anticipated, at about three or four inches up the passage, a complete *cul de sac*, and immediately below this, a prominent ridge or thickening, encircling the apex of the tumour. These unlooked-for signs, taken in conjunction with the roughness of the surface, presenting so marked a contrast to the usually smooth and almost polished appearance of a polypus, and the absence of every thing like a uterine mass above the pubis, together with the history of the case, satisfied me that we had to deal with some thing more serious than was at first conjectured. That, however unlikely *a priori*, the case was in fact one of those rare instances in which a polypus connected by a short pedicle with the internal fundus of the uterus had been suddenly expelled, and, dragging down with itself the organ, had caused it to undergo complete inversion; and that the broken-down mass, which I had removed in the early part of my examination, was indeed the polypus—the original cause of all this mischief; and which, as far as one could judge from its present condition, was of about the same size as the displaced uterus. Some doubts, however, of the correctness of my opinion being expressed by those around me, from the improbability of such an occurrence, as inversion from polypus in a person of the youth and character of my patient, she being a virgin, as well as from the extreme difficulty of accurately distinguishing between inversion and polypus, I proposed to search for the originally *internal* orifices of the Fallopian tubes, which, if my judgment of the case were correct, should be found on the surface of the tumour. These I was fortunate enough to hit upon, and having inserted into each of them a common bristle to the depth of two or three inches, I succeeded in gaining over my less hasty colleagues to my own view of the case.

During these various and long-continued manipulations, from which the pa-

[1] These were rendered very distinct when viewed through a common inch lens.

tient appeared to suffer very little, about six or seven ounces of blood were lost, but chiefly during the removal of the polypoid mass.

Having satisfied myself of the nature of the case, as well as of the impossibility of restoring the uterus to its normal position, I determined, as my patient had escaped the first and most usual dangers of such an accident, viz. nervous depression or exhaustion from hemorrhage, to interfere with the case no farther at this time, but to be guided as to my future proceedings by such circumstances as should arise. The patient was directed to preserve a recumbent position, to support the uterus against the pubis, and to use an appropriate lotion on the part.

For three or four days the patient remained in a tolerably comfortable state, but unable to rest without opiates. The discharge was constant and copious, in spite of various applications, and was evidently exhausting her strength. On the 27th, that is, a week from the date of the above examination, she displayed some restlessness with increased exhaustion, and her appetite was observed to be much impaired ; but the secretions were natural. She complained of unceasing pain in the back and lower part of the abdomen. Two days later, the exposed fundus uteri presented an angry and unfavourable appearance, extending upward towards the vagina. By the third of May, the inflamed appearance of the fundus had decidedly increased both as to intensity and extent, and the surface presented several excoriated spots, which were attributed to the passage of the urine over it, and to its friction against the labia and thighs. The discharge was less fœtid, owing to the use of a weak solution of chloride of lime ; but it was excessive, indeed it was found impossible to keep the bed dry. The condition of the patient was now daily becoming more and more unsatisfactory and hazardous. In spite of perfect quiet and rest, nourishing diet, tonics, astringents, wine, &c., &c., her strength was fearfully lessened ; the unhealthy appearances on the fundus rapidly extending, and the excoriations increasing ; and I began to apprehend an unfavourable termination to the case. Indeed, the prospect before my unhappy patient was now deplorable. The reduction of the uterus I had already found utterly impracticable. To leave it in its present situation I felt must speedily render her condition extremely distressing, if it did not expose her to certain and rapid death. In this state of the matter, I could figure to myself no plan for saving my patient, but that of *completely extirpating the womb.* Taking into consideration the youth and previous health of the poor girl—that the organ was free from malignant degeneration—that the uterus was an organ not immediately necessary to life—that, in its present abnormal condition and situation, its functions were for ever destroyed ; indeed—that it was, and must ever prove, only a burdensome mass, if not the source of an ultimately fatal hemorrhage, and the patient earnestly imploring me to relieve her from the miserable plight she was in, I made up my mind to extirpate the womb, as the only alternative in this distressing case. I, therefore, at once requested a consultation, agreeably to the rules of the Institution, and stated my impressions and fears, and the plan I proposed to adopt. These being acceded to, I determined to proceed with the operation with as little delay as possible; and I performed it on Friday the 5th of May 1843, in the way detailed below, in the presence of all my colleagues, medical and surgical, and about a dozen professional friends from the town and neighbourhood.

The patient, having been previously prepared by a small dose of oil over night, and an enema early on the following morning, was brought into the theatre, and placed in the position adopted in the operation for lithotomy (but without being tied). The labia being held aside by two of my colleagues, and traction downwards and outwards made on the uterus, I passed, by means of a double canula, or rather a modification of this instrument, the two parts of which could be separated or brought together at will, a tape band half an inch wide, as high up on the inverted portion of the vagina as possible—both as a precaution against hemorrhage, and also to give me a command over the divided vagina afterwards ; and having tightened this band somewhat, I excised,

with a common scalpel, guarded to within an inch of its extremity,[1] the entire uterus, close to the tape band, and through the portion of the vagina immediately beyond the cervix. The excision was easily accomplished, and could not have occupied many seconds, and there was scarcely any hemorrhage. A double-bladed speculum being now introduced into the vagina, I passed three silk sutures through the entire substance of this part, at some little distance from its cut edge, on two opposite sides, and brought it into accurate apposition, as I should have done in an ordinary excised wound, so as to procure, if possible, adhesion by "the first intention," an anticipation in which I was not ultimately disappointed. I now ventured to slacken the tape band slightly, but a flow of blood instantly taking place, I retightened it, and at once arrested it. Having satisfied myself that the band was not applied so tightly as to endanger sloughing, in fact, that it was acting simply as a tourniquet, I left it there, fastening the projecting end of the double canula against one of the thighs; and having filled up the vagina with a sponge moistened in cold water, the patient was replaced in bed. She bore the operation well, but subsequently complained sadly of the pain and distress it had cost her.[2] When visited soon after the operation she was extremely depressed, and appeared to suffer greatly from abdominal and lumbar pains—for which she was ordered a full dose of laudanum in some wine, and perfect quiet enjoined.

When I again saw my patient, at five P. M., four hours afterwards, she had enjoyed some sleep, but looked uneasy and very languid, and complained of thirst. The pulse was 100, regular, but very low; the skin was warm. She referred to constant pain in the abdomen, but bore pressure above the pubis pretty well. The catheter having drawn off half a pint of urine, which was clear and limpid, she expressed herself more comfortable. I now quietly loosened the tape band, but a disposition to bleeding still manifesting itself, I again tightened it, but scarcely so much so as before. A fresh piece of sponge was introduced into the vagina. Warm fomentations to the abdomen were directed, and she was ordered an anodyne.

At 8 P. M.—There was a false alarm of hemorrhage. The vagina, however, as a precautionary measure, was washed out with a solution of alum, and a new piece of sponge, soaked in this, was left in the passage. The patient appeared very restless and uneasy, as if alarmed. At 11 P. M. no hemorrhage had occurred. I found the patient lying on her back with both legs fairly extended; the countenance less anxious, and altogether more comfortable; the pulse steady, soft, and about 100. She complained still of pain in the abdomen of a griping character—bore pressure well—had passed some urine in a napkin—had not felt any nausea—but spoke of constant thirst. She was directed to have saline effervescing draughts occasionally, and ten grains of *pil. Conii. Co.* immediately.

May 6th.—My intelligent pupil, Mr John Prowse, who sat up with the patient, reported that she had passed a comfortable night, sleeping a good deal, and soundly. At half past 11 A. M. I found my patient lying on her side, with legs extended; her pulse 120, regular, soft, and very low—tongue slightly coated—countenance less anxious. She again drew attention to the pain in the abdomen and back, but did not shrink from pressure freely exerted. Urine had been passed several times during the night and morning. No action of bowels, which had been freely moved on the previous day by the oil and enema; still somewhat thirsty; all appearance of restlessness gone, but the de-

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[1] I would recommend a knife bent an inch from its point to an angle of 120°, and only cutting by this portion.

[2] It is impossible to reflect upon the pain and suffering endured by my unfortunate patient without many a feeling of regret, that the anæsthetic properties of chloroform were not sooner discovered. At the same time, we must all pay a tribute of grateful admiration to the humanity and genius of Professor Simpson, whose happiness it is to have originated so great a boon.

pression still very marked. The saline effervescing medicines were ordered to be continued with three grains of sesqui-carbonate of ammonia in each dose. The tape band slightly loosed—no appearance of bleeding.

The evening report of this day was equally good. The opiate was repeated.

7th.—The report states, that the patient had slept well all night, and there had been no appearance of hemorrhage. When visited at 12 noon, she was lying on her back with legs quite extended—free from pain in the abdomen; but there was still some uneasiness in the loins. The pulse was 110, and somewhat fuller; tongue slightly coated, but moist; skin natural; countenance placid; exhaustion not so marked; no vomiting or nausea since the operation; still complained of thirst and an indifference to food. The tape, double canula, and sponge were entirely removed; not the least appearance of hemorrhage. The parts external and internal cleansed with warm water. The sutures in the vagina were divided, as the wound was found to be healing nicely, with only a very small quantity of thin discharge issuing from it. The patient's linen was this day changed, and she was moved to a fresh bed. Bore the moving exceedingly well, and seemed grateful for the change. The medicines were continued, and some light pudding directed for dinner, as well as an egg. At 8 P. M. the report was, that the patient had slept comfortably during the day. She was free from pain; the pulse was still quick, but improved in tone; less thirst; the countenance was somewhat flushed; urine had been passed frequently, but no action of the bowels had occurred. She was ordered to take three grains of calomel and five of Dover's powder, and oil in the morning.

8th.—The report of this day marks a decided improvement. The pulse was 104, and fuller. The bowels had been twice gently moved, and plenty of urine passed. There was some evidence of returning appetite. The medicines were all stopped, and a more generous diet directed.

On the 9th, the improvement was still greater. The pulse reduced to 84, and stronger. The patient expressed herself quite free from pain and all uneasiness; and relished an egg and some beef tea.

11th.—Much improved in all respects. The appetite becoming natural. From this date the patient continued steadily, and without a drawback, to gain strength; and at the end of two months left the hospital, quite restored to health, and free from discharge. The vagina appeared about three inches long, and was closed by a complete cul de sac—a small ridge or line of cicatrix running through its middle. The wound of the vagina had completely healed at the end of a week.

I have had frequent opportunities of seeing and examining my poor patient since her removal from the hospital. On the 19th of February 1844, she came to see me, having walked a distance of three miles in about an hour; she was not at all fatigued, and she intended walking back again. She stated that she was, and had been, quite well, and should not, from her own feelings, know that anything unusual had occurred to her, except that she did not menstruate. She was very cheerful, and grateful for her recovery. On the 29th of July I again saw her; she was looking exceedingly well and cheerful. She told me she had never experienced the slightest discomfort, except periodical pains in the loins once a month, which lasted two or three days; but she had never had any sort of discharge, which could be regarded as vicarious menstruation. I have frequently seen her during the last three years. The last time I did so was at a friend's house, where she was living in the capacity of nurse-maid. This was in May or June 1848, five years from the date of the operation; and she was quite well, and had never ailed in the slightest degree. She stated that she had not experienced the periodical lumbar pains so often of late.

The extirpated uterus measured in length about three inches, and in breadth, at the fundus, two and a half inches. Assisted by my friend, Mr Standert, of Taunton, I made a clean section into the cavity, and exposed the glistening peritoneal coat and portions of the broad and round ligaments, and Fallopian tubes, with the bristles contained. The preparation

was seen by a great many medical men, among whom I may particularly mention Dr Clutterbuck, of London, and Richard Stafford, Esq., of the Marylebone Infirmary.

When I originally contemplated laying the above remarkable case before the profession, I proposed to myself to make it the basis of a monograph on the subject of uterine polypus, as a cause of inversio uteri—the uterus not being pregnant—distinguishing *this*, by some disputed, origin of the malady (Newnham, Capuron), from those infinitely more numerous instances in which the accident takes place in connection with labour,—such as from "*unequal contraction* of the uterus after delivery;" (Radford) "spontaneous inversion after delivery from atony;" (Newnham, Radford, Waller,) inversion from violence in extracting the placenta (Manning, Denman, Newnham, Windsor, &c.)—but the examination of the whole subject has been undertaken by one so much more able than I am to do justice to it, viz., by Dr Green Crosse of Norwich, in the New Series of the Transactions of the Provincial, Medical, and Surgical Association, that I shall content myself with such reflections as naturally spring out of my own case.

The first circumstance to which I would draw attention in commenting upon this case is the extreme youth of the patient. Malgaigne, in his "Traité des Polypes Uterines," Paris, 1833, has given a table of 51 persons who laboured under polypus of the womb. In this we find the age of twenty-six noted as the earliest period at which it had been observed. This table also shows that a vast majority of his collected cases occurred between the ages of thirty and fifty, viz., 36 of them; strongly confirming the general opinion that polypus uteri is a disease of middle life. I have not succeeded in finding any record of such an occurrence at so early an age as that of my patient.

That a polypus generated in the interior of the womb may result in the inversion of this organ, can admit of no reasonable doubt; for such an occurrence, in a minor degree at least, has now been verified and recorded by numerous observers, and frequently met with on dissection. My talented friend and quondam fellow-student, Dr H. Oldham, of London, has related the particulars of such a case, in the 2d volume of the "Guy's Hospital Reports," new series, page 109. His patient was a virgin between fifty and sixty years of age. She had suffered, for sometime, from hemorrhage, and subsequently from an offensive discharge. She died, worn out from discharge and bleedings. As she would not permit a local examination, the nature of her case was only disclosed on post-mortem investigation, when a polypus was discovered, closely connected to the right horn of the uterus, which part it had completely inverted. Dr Crosse, in his essay, alludes to a somewhat similar case, of which a preparation has been preserved in the Museum of St Bartholemew's Hospital. Dr Denman also relates a case, in which the inversion had proceeded further than in the

former instances. Of the *complete* inversion of the womb from polypus, in addition to the instance which fell under my own notice, I have met with the record of three cases—one given by Von Sauden, and quoted at length by Dr Crosse; a second, given by the late Dr D. D. Davis, in his "Principles of Obstetric Medicine," vol. 1st; and a third, by Dr Fleetwood Churchill, in his admirable work "on the Principal Diseases of Females," second edition, page 317, which he saw in the Jervis Street Hospital, Dublin, under the joint care of Dr Montgomery and Mr Lynch. Dr Churchill refers to a similar case, related by Dr Browne, in the "Dublin Journal;" but as I have not the opportunity of consulting this periodical, I must content myself with referring to it on Dr C.'s authority. I believe, that a due comparison of these cases with mine, will show that mine was not the least remarkable example of the malady, either as to its history, or the subsequent consequences to the subject of it.

The diagnosis of inversio uteri has been pronounced a difficult matter, where it is partial or slight in degree, and even when complete, it is liable to be confounded with polypus. I would venture to suggest, that the signs usually given as diagnostic of inversion, are incomplete and unsatisfactory without the plan that I adopted, of searching for the orifices of the Fallopian tubes, and inserting bristles[1] into them, a proceeding to which I was led in consequence of doubts being expressed as to the nature of my case, after I had ascertained the existence of all the usual marks of the accident. Where it is successful, the proceeding I suggest, must unquestionably remove all doubts on the subject. At the same time, I would caution the reader from regarding a want of success in his search for the Fallopian orifices as a negative proof of the non-existence of inversion; but I am disposed to regard the proceeding as not very difficult, or requiring any great amount of dexterity.

It has been questioned by some, whether extirpation of the womb is a justifiable proceeding; but, I believe, the objectors to this, no doubt, most serious operation, have been induced to pronounce their veto, from an almost exclusive examination of the proceeding in connection with malignant degeneration of the organ. In this class of cases, it doubtless appears a questionable undertaking, not only from the almost invariable want of success which has been recorded, but also from the growing impression in the profession, that the tendency to malignant disease, when once it has been pronounced, is rarely to be eradicated from the system. When the subject of extirpation of the womb for inversion is examined *apart* from such malignant fault in the organ or the system,

[1] I am disposed to object to the use of a probe for this purpose, even of a punctum probe, as not unlikely to force a passage for itself, an accident which could not occur with a bristle.

there appears to me no reasonable objection to the operation, formidable as it assuredly is. The numerous instances of the successful issue of such cases, to be found in various treatises and journals, justify this opinion; and, as Dr Churchill says, (op. cit., page 332)—"There is abundance of evidence to show that life may be preserved after the loss of the womb." "Rousset relates a case, where the uterus was destroyed by gangrene, and the patient recovered; and Rousset, Primrose, Radford, and Cooke, have given cases in which the uterus appears to have sloughed off without compromising the patient's life."—*Idem.*

In the "Edinburgh Medical and Surgical Journal" for July 1842, page 262, there is recorded a case, which shows, in a remarkable degree, the impunity with which the uterus may be removed. A woman of feeble constitution had a good delivery, and the placenta also came away easily. The midwife who attended her, having pushed her hand into the vagina felt a tumour, which she mistook for a second child. She accordingly pulled with such force at this, which happened to be the uterus itself, that she tore it from its attachment, and then, by means of a knife, separated it from the vagina, and removed it entire. In thirty days the woman recovered.

Dr Churchill gives the following list of persons by whom the operation of extirpation was successfully performed:—Ambrose Paré, Petit, Carpi, Sclevoght, Vater, Laumonier, Bouchet, Boudel, Dessault, Hunter of Dumbarton, Chevalier, Johnson, Hamilton, Clarke of Dublin, Windsor, Davis, Hull, Blundell, Moss, Lasserre, Williams, Newnham.

The propriety of the operation under certain circumstances, supported as it is, by so many successful instances, does not, then, admit of doubt; for, as Dr Burns says (Midwifery), "We are warranted to prefer extirpation of the uterus to certain death." The whole question, then, turns upon the probable issue of a case of inversion. It is generally admitted that the reduction of an inverted uterus is very difficult, unless it is connected with parturition, and the reduction is attempted immediately after the accident; and that the difficulty increases with every hour, until after a very few hours it becomes hopeless; the difficulty depending somewhat upon the degree to which the inversion has proceeded. Supposing, then, an irreducible case of inversion, like the one which has given occasion to these remarks, what is the probable termination of such a case? Dr Davis says, that "most frequently this displacement of the womb, when not speedily fatal, is attended by exhausting hemorrhages, both periodical and occasional, as well as by other forms of morbid profluvia;" and Dr Churchill gives it as his experience, that death, at no distant date, is the usual termination of the malady, "from syncope, dropsical effusions, and hectic." Moreover, the inverted organ has occasionally been strangulated, and separated by sloughing or gangrene, with great danger to the sufferer. (Radford, Capuron, Cooke.)

On the other hand, it is unquestionably true that a few women have been the subject of such an accident, who, having passed over the fearful nervous depression, or the great hemorrhage which frequently immediately succeeds such an occurrence, have for years escaped, in a surprising degree, its ordinary consequences. "Dr Denman mentions the case of a patient who consulted him for an inverted uterus twenty years before her death, and Delmotte another in which inversion was complete thirty years before. (Churchill.) These, however, I conceive, must be regarded as the exceptions to the general rule of such cases. In the instance which fell under my notice, the discharges and constitutional disturbance were so evidently leading to a fatal issue, that I did not hesitate to adopt the alternative of extirpation, feeling, with Dr Burns, that I was justified in preferring this to the certain death of the patient. And I trust, to argue from results only, that I may appeal to the happy termination of the case as an additional proof in favour of this proceeding, and to say at least "non ego frustra." Having decided upon the propriety of the operation, it was a question with me whether I should adopt the ordinary plan of removal by ligature, or deviate so far as to employ excision of the organ previously strangulated by a silk ligature, as recommended by Dr Davis and Mr Windsor, for at first I did not contemplate any deviation from the prescribed methods. But reflecting on the probable sources of danger in the operation of extirpation, it appeared to me that *hemorrhage* was the only one to be really apprehended, and I began to debate how far this might be prevented without subjecting my patient, already worn down, by pain and discharge, to all the usual distressing and depressing symptoms attendant upon the application of a ligature, with the apparition of peritonitis in the back-ground. Believing that I could accomplish this object by means of a broad tape band in lieu of the ordinary ligature, so applied as to act as a mere tourniquet, and which could be readily slackened, or even altogether withdrawn, when the risk of hemorrhage had ceased, and which, from its flattened surface and width, would not lead to the strangulation of the part around which it was placed, I was induced to depart, in the way already detailed, from the operative methods hitherto employed. In addition to the comparative immunity from pain and constitutional fever, in my case, I would suggest whether the more rapid and satisfactory recovery of the patient were not in a great degree promoted by my tourniquet ligature keeping the divided parts generally not only in close apposition, but in bringing and maintaining similar textures on the opposite sides of the wound against each other, and by permitting the employment of sutures to preserve their close apposition undisturbed. How far I was justified in my reasoning, or in subjecting my patient to a proceeding unsanctioned by precedent, or in my hope of inducing union of the divided vagina by the first intention, by means of sutures, I must

leave to the decision of others, earnestly but humbly suggesting a candid consideration of my operation, and a fair comparison of the *progress* of my case, *after the extirpation*, with that of those in which removal of the wound has been accomplished by means of a strangulating ligature.[1]

That the only safeguard against post-partum hemorrhage is an equable and uniform contraction of the uterus after the expulsion of its contents, I suppose is a position that cannot be denied; nevertheless, we must not lose sight of the fact that occasionally and under various circumstances, we do not meet with this equability and regularity of contraction, and yet no hemorrhage succeeds. It will be sufficient to instance cases of severe after-pains, where we have alternate contractions and relaxations of the organ, persisting sometimes for days, without any consequent bleedings. The following brief outline of an unusual case, however, more strongly displays this apparent anomaly, where there was a total absence of uterine contraction for a considerable time after delivery, without supervening or accompanying hemorrhage.

On the 8th July 1843, I was summoned to a Mrs H., æt. twenty-five, a person of leucophlegmatic temperament, who had been in labour for some hours with her first child. She was delivered of a live female infant at nine p.m., by means of the short forceps, the powers of the uterus failing after the head had descended to, and was resting on, the perineum; on which account, also, after a considerable interval, the placenta required to be extracted. The separation of the secundines was easily accomplished, but independent of any expressive efforts on the part of the uterus, which remained passive, and was perceived through the abdominal walls to be quite placid. Suspecting that clots must be distending its cavity, and much alarmed with the apprehension of flooding, I immediately introduced the hand into the womb for the purpose of removing these, but was surprised to find the organ already quite empty, and almost fully dilated, and showing no disposition to contract. Hoping to excite its action, I retained my hand in the cavity for nearly the space of two hours, keeping up, at the same time, firm grasping pressure through the abdominal parietes, with the other hand; but all in vain, the organ remaining entirely passive. At length, perceiving not the slightest tendency to hemorrhage, and worn out by anxiety, long watching, constrained position, and want of refreshment, I ventured to remove my hands, and putting on a firm abdominal bandage with a pad over the uterus, I left the patient, with whom I had now been uninterruptedly fourteen hours. In the course of half an hour I revisited her; no bleeding had manifested itself, and the uterus still remained patent to a great degree, although between two and three hours had elapsed since the separation of the placenta. Contractions, however, were gradually taking place, and the uterus ere long assumed the size and condition usual after delivery.

The labour in this case was remarkable for nothing but its lingering and atonic character. The patient's strength or courage never flagged, and she made a good recovery without a single drawback. She has since born two fine children, her labours being characterised only by the same want of tone as in the first instance.

[1] Vide Newnham's Essay; Windsor in London "Medico-Chirurgical Transactions," vol. x.; Moss in "Lancet," vol. i. for 1836-37.

Article V. — *An Improved Uterine Truss, with Elastic Gutta Percha Pessary.* By Charles Ritchie, M.D., Glasgow.

The uterine truss, when complete, consists of the frame represented in the wood-cut, an abdominal belt, and certain narrow slips of India-rubber, attached to the belt and to the frame, by which the latter is kept in its proper situation in any position and state of the respiration. The frame consists of gutta percha, and of a slender wire of tempered steel, which runs enclosed within the slightly curved vaginal stalk, from the elastic pessary at its uterine end, to the lower part of the upright hypogastric plate in front, and along the centre of each limb of each part of this for a couple of inches, when its ends are made to unite within a narrow cross band.

This frame may require slight modifications, in individual cases, as to the length of the vaginal stalk, the breadth of the pessary, and the breadth also of that part of the upright or hypogastric plate in front, which rests on the vestibule or nymphæ. It is possible, also, that the angle at which the pessary is placed with the vaginal stalk may sometimes require some variations; but I have found several modifications of the instrument suit equally well in the same case. This is to be ascribed to the flexibility of the instrument, by which it accommodates itself readily to existing circumstances.

In constructing this instrument, I limited myself to obtaining the needful support to the uterus consistently with what would not dilate the parts by its size, occasion pain by its hardness, or excite uneasiness by its weight, and I have thus necessarily got into the same general arrangement as to its shape with Dr Simpson in his instrument. It will at once be obvious, however, that the principle on which the present truss supports the uterus is different from that of Dr Simpson's. The pessary part of the frame is a very elastic oblong air-cushion, made of thin highly-polished gutta-percha, and may be made to rise at about an angle of 65 from the vaginal stalk, and, when the instrument is applied, to rest nearly parallel with the plane of the outlet of the pelvis, in apposition with the anterior aspect of the cervix and anterior labium of the uterus, thus pressing this organ, while it also supports it, in a direction backward and upward. The pessary can be attached to the vaginal stalk at a more acute angle, as in that represented in the wood-cut, in which it rises from the slightly-curved vaginal stalk at an angle of about 45; and when *in situ*, and the woman in a standing position, its upper surface is nearly parallel with the plane of the brim, and the orifice of the uterus rests upon its centre.[1]

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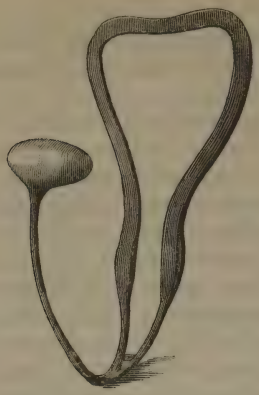
[1] The gutta-percha frames are made by Mr Paterson of this place; and may be had of Mr Hilliard here, or of Messrs Hilliard & Son, Edinburgh. From an over-sight, the accompanying representation is deficient in a narrow transverse plate, which passes across from the one limb of the frame to the other, where



The shape of this pessary is otherwise very convenient, as one extremity of it, oiled, can readily, from its size and form, be passed through the narrowest sphincter, while the configuration of the vaginal stalk and hypogastric plate is such that when the former is fully introduced, even by the least intelligent patient, the pessary must necessarily be in accurate apposition with the uterus.

The small diameter of the vaginal stalk, its elasticity and at the same time its strength, and the lightness and suppleness of the hypogastric plate, have all commended themselves much to my patients.

I have said that the instrument is worn with an abdominal belt, to which it is attached by narrow tapes of India-rubber. The kind of abdominal belt may be allowed to vary, according to the wishes or wants of the patient. Some women, from the weight of the viscera of the abdomen on the uterus, require a regular abdominal supporter, others may do with a band, three or four inches broad, of vulcanised caoutchouc. The smell of this latter is objected to by some, and its excess in elasticity by others, in which event cotton, linen, or flannel may be substituted.

The mode of attachment of the vaginal frame to this belt also differs in different cases. When a perfect abdominal supporter is worn, two slips of vulcanised India-rubber, each about a quarter inch in breadth, are sewed to the belt behind, corresponding in situation to each side of the coccyx. These are brought forward along the corresponding sides of the perineum, and made to cross beneath the transverse band on the front of the lower part of the hypogastric plate, which corresponds with the symphysis of the pubis, and their

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these diverge, and in which the bifurcated ends of the vaginal wire, enclosed in the two sides of the frame, unite. The price taken for them is four or five shillings. Mr Paterson is well known as a scientific anatomical modeller.

ends are then carried round the lower part of the trunk, and secured. Another pair of common or India-rubber tapes may next be attached to the upper corners of the hypogastric plate, and also carried round the body and tied. The lower edge of the abdominal belt covering the hypogastrium should then be brought down upon the front of the upright frame of the instrument in the vagina, and if necessary this edge of the bandage and the corresponding part behind may be kept secure by means of two additional India-rubber tapes passing along the perineum from the one to the other.

When a simple band around the belly is all that is worn, it will be requisite, in addition to the tapes from behind, which are fixed, as in the former case, to the lower and upper angles of the upright hypogastric plate of the instrument, that the upper edge of this be enclosed either in a duplication of the lower edge of the abdominal band, if made of caoutchouc, or in that of a vertical two-inch broad piece of this material, attached by its upper edge to the band around the abdomen.

When either of these methods of affixing the uterine truss is employed, the vaginal portion of it is kept secure and comfortable in any position. The urine is evacuated without inconvenience, as are also the catamenia; and should either the edges of the abdominal supporter or of the perineal bands gall the skin, the evil is obviated at once by covering them with chamois leather.

The only other means of treatment I employ are—attention to prevent a loaded state of the bowels, and a regular use of the cold-water sitting-bath. This latter I employ at first at a temperature of 70°, and for a few minutes only; afterwards, and in mild weather, I recommend the bath to be of a lower temperature, and to be used for half-an-hour.

ARTICLE VI. — *On the Pathological Anatomy of Cholera.* By W. T. GAIRDNER, M.D., Pathologist to the Royal Infirmary of Edinburgh.

[*Communicated to the Medico-Chirurgical Society of Edinburgh, June 6,* 1849.]

THE following statement is founded on the examination of eighty-nine fatal cases of cholera during the late epidemic, in the theatre of the Royal Infirmary of Edinburgh. In the course of my duties as pathologist to that institution, I have had unusual opportunities of contrasting and comparing the appearances in cholera with those presented to my notice in connection with other diseases; and the constant use of this *comparative* method of observation has led me to consider many statements as erroneous, which are generally, and in some cases all but universally, received in relation to this disease. The greatest care has been taken to verify or correct the results obtained by previous observers, more especially where any theory of the disease appeared to be involved; microscopical ana-

lysis and chemical tests have been applied wherever they promised to aid in the investigation; and in regard to such doubtful points as appeared likely to acquire precision by numerical analysis, the results have been preserved in the tabular form.

The bodies opened were mostly from the cholera hospital, being taken at random from those who died under the care of Dr W. Robertson; a few, however, were from the Infirmary. The proportion of females to males was 2 to 1. The average age of the patients was thirty-three, by far the greater number being between twenty and forty. Four of the females were in different stages of pregnancy; but as the greater number of the bodies of females known to be pregnant were given to Mr Goodsir by his special request, this number does not give an accurate idea of the large proportion of pregnant cases. Several of the females had been nursing, as was shown by the distended mammæ, which readily yielded milk on pressure. The great majority of the bodies examined were evidently those of persons previously healthy and vigorous. They presented the external appearances, so well known in cholera, of lividity and collapse, the last being caused by the shrinking of the cellular tissue from the absorption of its watery parts; there was, however, no diminution of the fat, which was usually in very considerable quantity on the abdomen and elsewhere. The muscles were of good colour, and usually in strong tonic contraction from the rigor mortis. The more important facts resulting from the examination of the internal organs are embodied in the following conclusions:—

I. *Previous Diseases of those attacked.*—Cholera appears, during the late epidemic, to have attacked chiefly persons in health, or in the retrograde stages of chronic affections; and to have spared almost entirely those affected with acute or actively progressive disease. Appearances of acute disease were chiefly observed when death took place after more or less distinct reaction, and were evidently the *sequelæ* of the choleric affection. The chronic lesions were exactly similar in kind to those most commonly found in hospitals and dissecting rooms, but bore a decidedly lower proportion than is usual to the whole number of cases examined. Thus the lungs had the traces of old disease in one-sixth of the cases; among which two only (cases of miliary tubercle) indicated progressive disease. The liver presented a chronic lesion in one case; the kidneys in four, of which only two (incipient Bright's disease) were progressive. The uterus and ovaries were frequently more or less abnormal, but the only progressive lesion was ulceration of the cervix, usually quite superficial. The intestines were uniformly exempt from chronic disease. It thus appears that the opinion which has been so extensively prevalent since 1832, that cholera attacks chiefly or exclusively individuals of unsound constitution, or bearing the traces of previous organic disease, is not borne out by the facts of morbid

anatomy; and in particular, that there is no evidence whatever that previous disease of the intestinal canal pre-disposes to cholera.

II. *The Blood* is much less affected in its physical characters than is usually supposed to be the case in cholera. Its coagulation within the vessels takes place much as in other diseases. In the majority of instances, firm clots are found within the heart, more or less completely decolorised; and the serum or non-coagulated portion contains the greater part of the blood-corpuscles. The colour of the blood presents nothing unusual, the epithets "dark" and "venous" being in no degree more applicable to cholera blood after death than to that of every ordinary form of fatal disease. The remarkable viscidity of the serum (or portion of the blood not involved in the clot) so often noticed, was chiefly observed in cases fatal during the collapse or early reaction; and was certainly owing to the removal of the fluid matter by the intestines. The effect of this in modifying the chemical constitution of the blood has been fully shown in Dr Robertson's researches ("Monthly Journal," May, p. 764). The microscopic appearances of the blood presented nothing unusual.

III. Much importance has been attached to *Congestion* in relation to the pathology of cholera. But it is an error to conceive of congestion as an essential or universal condition in this disease; for, although the lungs and right side of the heart are frequently loaded with blood to a considerable extent, the liver, spleen, and kidneys are, in most cases, paler than is usual in other affections. The intestines present every shade of colour from the palest to the deepest. The uterus and Fallopian tubes are generally loaded with blood. The venous system of the brain, and indeed the great veins generally, are also in most instances full. But such appearances are very common in other forms of fatal disease.

IV. A tendency to *Ecchymosis* in various situations was certainly characteristic of cholera to an extent not common in other fatal diseases. These ecchymoses seldom occurred externally, except in one situation, viz., beneath the conjunctivæ of the eyes, which were very commonly more or less bloodshot. Among the internal organs it was more frequent on the intestinal mucous membrane, especially of the colon, than in any other situation; but very frequent also in the form of petechiæ on the posterior surface of the heart, and occasionally in other places, as the cellular tissue surrounding the cervical vessels, and in that around the dura mater of the spinal cord. Perhaps there were other situations in which ecchymoses might have been found on examination; but in the above it was so frequent, that in only six out of twenty-six cases was it not found in one or more of them.

V. The *Glandular Secretions* in cholera are probably more or less diminished in quantity (although there are no very precise observations on this point); and they appear also, from the observations of Dr D. Maclagan[1] on the milk, and of Dr J. W. Begbie and other observers on the urine,[2] to undergo great alterations as to quality. But with the exception of the urinary secretion, which in the collapse appears to be nearly, if not altogether suspended, there is no evidence that any of the more important and constant glandular secretions are suppressed in any stage of this disease. The milk could be readily expressed in streams from the ducts, even after death, in nursing women. All the other glands preserved their natural appearance and structure, and the lymphatic and mesenteric glands contained a secretion which presented the normal microscopic elements.

Next to the suppression of urine, the suppression or retention of the bile has been assumed (on account of its apparent absence in the dejections) to be one of the most characteristic features of Asiatic cholera. The assertion that the bile is suppressed, however, is obviously incorrect; and it is only wonderful that this idea has not met with more positive discouragement by pathological writers. In the late epidemic, in every instance, except two, the gall-bladder contained more or less of bile, which was mostly of good colour and consistence; and, in the greater number of instances, in quantity sufficient to produce considerable distension. One of the exceptional cases was the one formerly referred to, of diseased liver, in which the gall-bladder contained only a thin light-coloured fluid; in the other, abundance of bile was present in the duodenum and stomach. The gall-ducts were most frequently empty, but occasionally contained bile; a probe passed in all cases easily from the duodenum to the gall-bladder, being usually somewhat tightly grasped at the neck of the latter, as is invariably the case, whatever be the cause of death.[3] The duodenum contained, in most cases of collapse, no appearance of bile; but there were, neverthe-

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[1] See Dr Robertson's account of the Practice in the Cholera Hospital, in the "Monthly Journal," Dec. 1848, p. 394.

[2] Dr J. W. Begbie has made a very extended series of observations on the urine voided in the earlier periods of reaction, from which it appears that the urea is generally much diminished, and in some instances entirely absent; and that albumen is almost invariably present in greater or less amount, together with epithelium, for a variable period after the commencement of reaction. The presence of albumen was noticed at a very early period in the Edinburgh epidemic, and has since been extensively observed in the Parisian hospitals. Dr Begbie also informs me that various other modifications of the urinary secretion have been observed by him; in particular, a reaction with nitric acid indicating the presence of bile.

[3] This constriction at the neck of the gall-bladder is probably due to an elastic tissue. It has been frequently mistaken for *spasm* of the gall-ducts, a condition which I believe, *as far as post-mortem examinations are concerned*, to be purely imaginary.

less, several cases in which the characteristic green colour was present in greater or less amount in the contents of the duodenum and stomach, though not in those of the intestines generally.

It appears, therefore, clear, that the non-bilious character of the evacuations cannot be due to non-secretion. It must, therefore, be ascribed to retention of the secreted bile in the gall-bladder. But it may well be doubted whether this retention can be rightly regarded as any part of the pathological process in cholera. The quantity of bile which passes into the duodenum, under ordinary circumstances, in the absence of the natural stimulus of food, is probably very small; and the quantity which appears in the fæces in the normal state, or even under an ordinary attack of diarrhœa, would be quite insufficient to tinge visibly the enormous quantity of fluid thrown off by the intestines in cholera. Further, the examination of the cholera dejections chemically shows that biliary colouring matter can frequently be discovered in them by the nitric acid test, when it is not appreciable otherwise;[1] and it is fully proved by the *post-mortem* appearances, that bile is occasionally found in appreciable quantity in the duodenum during the collapse state (though to a more considerable extent during the reaction). From these circumstances it seems probable that the secretion of the liver is discharged into the intestines in cholera to quite as great an extent as in most diseases in which digestion is totally interrupted, or as in a healthy individual when fasting; and that its ceasing during the collapse to colour sensibly the dejections, is the consequence of its extreme diffusion through the mass of fluids in the intestines.

Repeated microscopic examinations of the liver revealed nothing unusal. The kidneys, however, appeared in many cases to have undergone morbid changes, the cortical substance being pale and turgid, and the tubuli uriniferi gorged with imperfectly developed epithelium, which was mostly loaded to an unusual extent with oleo-albuminous granules. A similar state of the kidneys occurs after scarlatina, and not unfrequently after typhus fever, and some other acute diseases.[2]

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[1] Dr Parkes seems to doubt whether the matter indicated by this reaction is bile; but neither does he admit it to be uric acid, the only substance which has been alleged to have caused confusion. The principal reason stated for his scepticism is that "there is a remarkable retention of this product (bile) in cholera, as in dysentery." It will be at once obvious that this is the very preconception against which I am arguing. See the "London Journal of Medicine," Feb. 1849, p. 143.

[2] I am indebted to my friend, Mr Alexander Borthwick, now in Dumfries, who studied along with me very carefully the condition of the different organs in cholera as compared with the miscellaneous cases which came under our notice, for tabular statements which show quite satisfactorily the much greater comparative prevalence of an excessive quantity of oleo-albuminous exudation in the kidneys of cholera patients, than either in the liver or the heart, which were in this respect below the average. None of these cases, however, with

The secretions of the serous membranes appeared to be diminished in quantity, and everywhere more viscid than natural; an effect probably of the altered constitution of the blood.

VI. *The condition of the Intestinal Canal and its Secretions* demands special notice. The most frequent of all the abnormal conditions of the mucous membrane was the prominence of the intestinal glands, both aggregated and solitary, but especially the latter. This condition, the *psorenterie* of some French writers, was found in about two-thirds of the cases. The great frequency of patches of ecchymosis in the intestines, especially in the cæcum, has been already alluded to; in these cases the intestinal contents were usually more or less tinged with blood, and presented blood corpuscles among their other elements under the microscope. The ecchymosed patches were distinctly circumscribed, in tint varying from claret colour to the deepest purple, approaching black, but in parts not unfrequently greenish or ash-coloured; their surface presented a similar appearance, in all but colour, to the rest of the mucous membrane, and was not perceptibly elevated. In three instances, however, a different appearance was observed in the colon, the mucous membrane presenting a few flattened elevations, each over the extent of about a sixpenny piece, of a greyish or leaden colour. These elevated patches were evidently due to a sub-mucous exudation (probably identical with the diphtheritic exudations described as occurring in cholera by Virchow), and yielded on section a creamy fluid; in this fluid microscopic examination showed no well-marked pus-corpuscles or complete cells, but a number of nuclei, on which acetic acid produced no effect. In one protracted case, in which the great intestine was much ecchymosed, there were traces of dysenteric lymph on several parts of the mucous membrane. In two other instances, there was distinctly exudation of yellowish lymph, with heightened vascularity on the external serous surface of several folds of small intestine.

On the other hand, it was by no means uncommon, especially in early fatal cases, to find the intestines throughout natural in colour and appearance, or even paler than natural; and in many of these cases there was no prominence of the solitary glands.

The intestinal contents resembled closely at first sight the well-known cholera stools. In their most characteristic appearance they were yellowish-white, but frequently acquired, from blood on the one hand, or bile on the other, various shades of orange and greenish colour. On microscopic examination, the intestinal contents invariably showed immense quantities of perfect epithelium, sometimes in coherent masses, peeled from the mucous membrane,

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the exception of those alluded to in a former part of this communication, presented the granulations of Bright, or any other unequivocal marks of a chronic disorganisation.

and preserving the form of the villi and follicles to which it had been attached. The existence of epithelium in the fluids found in the intestines in cholera has been noticed by Boehm (see "Medical Times, June 24, 1848), and other observers, and has been supposed to indicate a complete desquamation of the intestinal epithelium as one of the special pathological conditions of cholera. In reality, however, the appearance proves precisely the reverse; for the presence of epithelium in such large quantities in the fluids found in the intestines after death, is the result of purely mechanical maceration upon a mucous membrane to which, during life, the epithelium remained attached. That this is the case is proved by these facts—1st, That artificial maceration produces a similar result on a healthy mucous membrane; 2d, That the epithelium found in the fluids of each division of the intestines is always that of the particular part where it is found; 3d, That the true cholera stools, *passed during life* contained so little perfect epithelium, that it cannot be considered as anything more than an accidental ingredient.[1]

The examination of the cholera-stools leads to much more important and less confusing results than that of the intestinal fluids after death. These discharges separate by filtration, or on standing, into a colourless, or slightly coloured fluid, of an alkaline reaction and a flaky sediment. The former is usually of a specific gravity from 1005 to 1010, and contains, therefore, a very small proportion of solid matters; these have been shown by Dr Parkes to consist chiefly of salts. Repeated examinations of this fluid have shown during the late epidemic (as Andral showed in the former) that albumen, as tested by heat and nitric acid, is not necessarily present in it. Indeed, the presence of albumen usually coincided with that of a small quantity of blood, which, as before mentioned, is frequently present. The fluid, however, contains *constantly* a small quantity of an organic substance which is precipitated by alcohol, by corrosive sublimate, and (when acidulated), by ferrocyanate of potash; and which presents, in other respects, the chemical reactions of mucus.[2]

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[1] A similar conclusion, in regard to the desquamation of epithelium, has been maintained by Dr Parkes, in an elaborate article on the *Intestinal Discharges in Cholera*, in the "London Journal of Medicine" for February last. Although there are many opinions in Dr Parkes' paper from which I differ, it affords me much gratification to observe, that the principal facts included in the description of the cholera-masses by himself and his colleagues, correspond so nearly with what I have myself observed.

[2] M. Mialhe calls this substance *albuminose*, and considers it as "the ultimate product of the digestion of albuminous substances," (See "L'Union Médicale," 5th April 1849). His pathology of cholera is founded on the alleged presence of albuminose in the blood, which assertion, however, he does not appear to have attempted to establish by experiment. I prefer, therefore, the more familiar term of mucus, which is perfectly applicable to this substance, and

The flakes which form the sediment of the cholera-stools, have likewise the ordinary chemical reactions and physical properties of mucus. They are, however, opaque and turbid, and when submitted to the microscope, show a hyaline finely-striated basis, involving numerous granules, nuclei, and cells. The granules and minuter molecules require no particular description. The nuclei are from 1-250th to 1-180th of a line in diameter, circular or slightly oval, and not affected by acetic acid. The cells are comparatively few in number, mostly globular, seldom exceeding 1-150th of a line in diameter. Some of them are but little affected by acetic acid, and single-nucleated; others present all the appearances of perfect pus cells, being two, three, or even four-nucleated, and having the cell-wall rendered very transparent by acetic acid. The most common appearances, however, were the nuclei without cells above-mentioned.

Any one who is familiar with the pathological phenomena displayed by mucous membranes in a state of irritation, will readily recognise the similarity of the above microscopical appearances to those of ordinary catarrhal discharges from any of the mucous surfaces of the body; the chief peculiarities of the choleric fluid being the enormous exaggeration of the watery and saline matters evacuated, and likewise the smaller tendency to the discharge of albumen, and the development of pus-corpuscles, which are so readily formed and thrown off from a mucous membrane in a state of ordinary irritation.

The effects upon the blood of the removal of the watery and saline constituents are well shown in Dr Robertson's analyses; and it is probable that a considerable amount of the albumen of the blood also passes away by the intestines, in the form of mucus, which is nearly allied to it in composition, and which, as has been shown, forms the organic basis of the choleric evacuations.

VII. The *Nervous System* presented no lesion worthy of remark. The pia-mater of the brain and spinal cord were often considerably injected, especially that of the cord, which, from the position of the body after death, almost always presents this appearance. The spine was, however, only opened in four cases; and in one of these, there were found small calcareous plates upon the arachnoid of the cord—the most frequent, probably, of all its chronic lesions. The sympathetic ganglia, and the pneumo-gastric nerves were repeatedly examined; but nothing unusual, except in a few cases slight ecchymosis, was discovered.

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was used by Andral, to one which is associated with a very doubtful hypothesis. The nearly allied chemical relations of mucus and the protein-compounds are well known.

Part Second.

REVIEWS.

Report on Quarantine. (General Board of Health.) Presented to both Houses of Parliament by command of Her Majesty. London: 1849. 8vo. Pp. 172.

THE immediate occasion of the report before us appears to have been the quarantine which was temporarily imposed by the Privy Council on vessels coming from the Baltic before the late outbreak of cholera, in October last. The Board of Health had been employed in making inquiries into the sanitary state of British vessels in communication with ports where the disease prevailed, with the view of preventing the outbreak and spread of the disease on ship-board in the open sea, which they found to occur more frequently than had been anticipated. In the course of these inquiries they procured evidence of numerous circumstances arising out of the quarantine regulations, which appeared to them to call for immediate attention, as being very detrimental to the health of passengers and crews placed in quarantine, and not essential to the safety of the community. The Board were induced to report these grievances to the Privy Council, in a letter dated the 9th November 1848, and, in addition, to propose the question "whether the inconvenience, expense, suffering, and probable loss of life involved in quarantine, are likely to be compensated by any amount of public benefit which quarantine regulations can be supposed to be capable of effecting." The Board were again required to consider the subject, in replying to questions addressed to them on the 24th of November, by Lord Palmerston; and subsequently received additional information as to the condition of the ships of the mercantile marine, which satisfied them still more strongly "of the necessity of seeking the accomplishment of the object of quarantine by different means than those which this system sanctions and adopts."

The result of these repeated cogitations is the present report, in which the Board propose "the entire discontinuance of the existing quarantine establishments in this country, and the substitution of sanitary regulations" in ships, entirely similar to those to be employed on shore in the prevention of endemic and epidemic diseases.

The report which enunciates this sweeping condemnation of the existing quarantine regulations is strongly impregnated with the peculiar views on the mode or origin and propagation of epidemic diseases which have all along marked the productions of the sanitary reformers, and more particularly the reports of the Metro-

politan Sanitary Commissioners in 1847 and 1848. These views are characterised by a growing distrust of the existence or efficacy of the *specific* causes of epidemic diseases, and a disposition to refer all, or at least the greater number of these diseases, to similar local and general causes—the *local* causes being found in putrefactive effluvia, and other miasmata dependent on deficient sanitary regulations; the *general* being comprised under the somewhat vague term of *epidemic influence*, an expression which has never been sufficiently well-defined by those who have used it, but has generally been employed to express the sum of the meteorological or atmospheric influences which may be conceived to bear on the propagation of epidemic disease; excluding, however, or at least disregarding, the communication of it from the sick to the healthy. Indeed, human intercourse has ever appeared to the advocates of such doctrines as those to which we refer, a mere secondary and insignificant agent, if not actually inert, in the propagation of epidemic diseases; and the tendency of their speculations has accordingly been to discountenance or reject the idea of contagion, in regard to every such disease, when its communication from the sick to the healthy has not been proved, either by the inoculation of a tangible virus, or by a weight of evidence amounting almost to demonstration in favour of atmospheric contagion.

Opinions like the above, though common enough among the so-called non-contagionists of France, have never had any very extensive influence on medical opinion in this country. To the sanitary reformer, however, the view of local miasmata, aided by epidemic influence, as the cause of epidemic disease, offers irresistible attractions. By placing the local circumstances in the light of the *fons et origo mali*, without which the general causes are powerless, and combined with which they are all-powerful, it accords with his favourite idea, that a few simple sanitary precautions will uproot and extinguish pestilence and epidemic disease in every form. Aided by this idea, he becomes the apostle of a new faith, in which the vulgar fears of ages, with all their superstitious, cruel, and selfish results, are at once annihilated; and the plague-struck city deserted by those who ought to succour it—the devoted house with the red cross on the door—the dying left in their misery, with the dead for companions—the crowded lazaretto—the gloomy and anxious quarantine ship—the military *cordon*—vanish at once from human history, together with the monstrous chimæra that caused them—the theory of contagion. Such aspirations are plainly indicated, if not directly stated, in the present report; and several of the supporters of these views, in the daily and weekly press, have told us, that the belief in contagion, like that in astrology or witchcraft, is fast dying out among all sensible men. We might easily prove to such persons that telluric, atmospheric, and celestial influences were the favourite theories of astrological writers on these subjects, and that the views of the contagionists were gene-

rally opposed by them, but this would be beside our present purpose.

We would not willingly check one philanthropic aspiration, or obstruct one real reform; nor do we believe that the opinions which we, in common with the great body of medical men in this country, hold on the subject of the propagation of epidemic disease would, rightly considered, have any such effect. But at all events, we are bound, in the simple interest of truth, to proclaim, that the general medical opinion of this country has no share in the enunciation of the vague and inconsistent views of the Board on this subject; that the great majority of medical men regard the propagation of such views as dangerous in the extreme, and doubly rash and indefensible when prefixed to a measure of proposed practical reform by a board essentially non-medical, and whose single and temporary medical authority is in no way responsible to any medical tribunal, or indeed to any public body, save to the Board itself.

The Board seem to be very well aware of the discrepancy between their own opinions as stated in this report, and those which carry the weight of medical authority: and, accordingly, they throw themselves for support upon popular and commercial prejudice, in terms well fitted to rouse the jealousy of medical men.

"It is remarkable that the change of opinion which has taken place on this subject (contagion) in Great Britain, has advanced with greater decision and rapidity among non-professional persons engaged in commercial and public life than among medical men. *It is not a technical question, but one of evidence, on which a person capable of observation is as competent to judge as any physician.* Such a person, indeed, may have the advantage of having nothing to unlearn. The Spanish Cortes in 1822, by a considerable majority, in direct opposition to the unanimous opinion of all the physicians who were members of their own body, being nine in number, as well as to the general belief of the physicians of Spain, rejected the project of a code of sanitary laws, founded on contagion, which had been prepared by three successive committees or commissions of public health. It is also observable that the committees of the House of Commons in our own country have generally indicated a more advanced state of opinion on this subject than the majority of the physicians whom they have had under examination."—P. 20.

It would be difficult to over-estimate the fallacy and self-sufficiency of these statements. They indicate a state of feeling, on the part of the Board, which we are sorry to see so strongly demonstrated, because we fear it is calculated most seriously to impair their efficiency, not only in quarantine reforms, but in many other matters in regard to which we wish them well. The passage we have marked in italics, displays a misapprehension of the relative value of public and medical opinion, which it is impossible for us to pass over. Disputed questions, whether medical or otherwise, are generally questions of *evidence*, and it is perfectly well understood and admitted on all hands that where the evidence can be brought fully, and divested of technicalities, before a non-professional jury, *free from partiality, and bound to consider*

it in a complete form, a good judgment may be obtained. But we deny that the question of contagion is not a technical question, and we deny still more strongly, that the classes of our population connected with commerce and public life, form such a jury. The question of contagion is so intensely technical, that very few, even of the best informed medical men, can approach it without a feeling of mystification and difficulty, arising from the confused use of terms; and the very last French medical commission which investigated the subject of quarantine, could only get rid of the difficulties arising from this cause among its members, by excluding the word contagion altogether from its report. We shall presently see whether the General Board have dealt with the technicalities we allude to in any more satisfactory manner. While we take the liberty, therefore, of doubting whether even a limited number of public men, fairly impannelled, and obliged to hear all the evidence, would readily attain to a secure and just conviction on many of these subjects, we certainly cannot admit that the opinion of the commercial classes and of public men, bound, as they are, by no obligation to investigate the whole subject, and swayed by all their special interests to decide it in conformity with the view which imposes no restriction on human intercourse, should have had any weight with the Board, in opposition to the opinions of medical men, who can have no possible interest in deceiving them. It is true that the grievances of these classes merited the greatest consideration; but we contend, that it was the duty of the Board to have ascertained the opinion of the medical profession on the medical questions involved, and to have proposed reforms in conformity with that opinion, instead of seizing the occasion to indoctrinate medical men and the public into their own views upon a subject which is believed by themselves to have (see p. 18) "nothing whatever to do with quarantine"—the special object of this report.

We propose, in the sequel of this article, to submit to our readers a very few remarks on the opinions of the Board with reference chiefly to three subjects: 1st, Epidemic Diseases; 2d, Epidemic Influence; 3d, Contagion (including therein Infection, which the Board are especially anxious to distinguish from it). In regard to the first of these subjects, we find the following remarkable passage:—

"Epidemic diseases were formerly universally considered to be essentially different in their nature, each being thought to depend on its own specific contagion; and the correctness of this view seemed to be confirmed by the great apparent difference between typhus, scarlatina, influenza, plague, yellow fever, and cholera; but whether each of these diseases depends on a peculiar and specific cause, or whether they all derive their origin from one common agent, essentially the same in nature, but modified by peculiarities of climate and other circumstances, and which, under varying conditions, gives rise to various forms or types of disease, each having definite characters and running a particular course,—whichever of these views be adopted, it is agreed by the most eminent investigators that there is a general resemblance between these various forms of disease, and that they have the following characters in common:—They are all fevers; they are all dependent on certain atmospheric

conditions; they all obey similar laws of diffusion; they all infest the same sort of localities; they all attack chiefly the same classes, and, for the most part, persons of the like ages; and their intensity is increased or diminished by the same sanitary and social conditions."—P. 5.

The examination of this paragraph is of considerable importance; for, from what immediately follows, and, indeed, from the whole report, it is evident that the whole arguments of the Board are based upon the idea, that whatever can be proved of the above six diseases, *or of any one of them*, may be considered as true of every epidemic or pestilential disease. For, say the Board, "the consideration of these *common properties of pestilence, under whatever name it may occur*, has led to the general conclusion, that the true safeguards against *pestilential diseases*" (not the above six epidemics alone), "are not quarantine regulations," &c.—P. 5.

We pass over in silence the flimsy hypothesis which is here merely hinted at, though not asserted, and which attempts to refer all these diseases to an identical cause. But we cannot admit the "characters in common" here assumed of all, to apply to almost any two of these diseases. That they are all epidemic diseases, we indeed admit, as also that they are *possibly* (with a reservation in regard to cholera) fevers; nor have we much difficulty in believing that, like most other diseases, they are dependent (more or less) on *certain* atmospheric conditions; but as regards all the other points of alleged concurrence, we maintain that no more unfortunate selection could have been made; while the identity of their *laws of diffusion*, the main point in the Board's argument, is a mere *petitio principii*, unworthy of a schoolboy. What! Influenza, the disease especially of the richer and middle classes;—and typhus, the disease almost exclusively of the poor! Scarlatina (why not measles and hooping-cough also?), the diseases of childhood and youth;—and typhus, that of adult and old age! Cholera and yellow fever, diseases, primarily, of tropical climates;—typhus and plague, with rare exceptions, of the temperate zone! Cholera and influenza travelling westward;—plague, northward;—yellow fever, eastward and westward;—typhus, in all directions indifferently within its limited range! Influenza, striking down half the population of a district in a single night;—typhus or scarlet fever, creeping insidiously from person to person, from house to house, from town to town! Truly these are most discordant elements out of which to form the "common characters" of the Board.

The board argue that influenza cannot be kept out by quarantine; that influenza and the other diseases named are similar in their laws of diffusion; therefore, that all epidemic diseases are beyond the scope of quarantine. Now, influenza is a disease to which quarantine never has been applied; and we know, therefore, no reason why the argument in question should have been made to rest on influenza as an illustration, except that the trouble of proof was thus conveniently saved. But the argument no more affects

the question of quarantine as applied to plague or typhus fever, than if it had been drawn from hooping-cough, dysentery, or scurvy, which are all of them epidemic diseases.

We shall not ask the board why they have not chosen to include in their list these and other diseases of the same class, which in some respects, no doubt, "obey similar laws of diffusion" with the six named; but we cannot but express our surprise that with all the facility displayed in finding out common characters amid such diverse diseases, they should not have thought proper to give a place to smallpox, beside its near neighbour in the nosology, scarlatina. We should have been inclined to place smallpox at the very head of their list;—it is a most deadly pestilence, the very type of an epidemic fever, ranging through all climates, all ages, all ranks; and yet controlled and limited by proper ventilation and the other sanitary regulations alluded to by the Board, to a very marked extent. Yet smallpox is undoubtedly contagious and infectious. Will the Board admit the argument from smallpox to cholera, in respect of contagion, as it calls upon us to allow the argument from influenza to plague in respect of quarantine?

But we have done with this specimen of inconsistency and loose analogical reasoning, and pass to the consideration of *epidemic influence.*

We have already had to complain of the vagueness of this expression. We can ourselves attach no idea to epidemic influence, except by conceiving it to be the influence, or aggregate of influences, which cause any disease to become epidemic. Of the nature of these influences, and of the reasons why they should act more at one time than another, we confess ourselves to know little or nothing, though we doubt not their existence. But the Board pretend to greater knowledge. The epidemic influence of continental writers becomes, in their hands, a magnificent creation. It is a peculiar state of the *atmosphere*, "may exist over thousands of square miles, and yet affect only particular localities." How is it known to *exist* in the localities it does not *affect?* If the theory of an atmosphere requires the supposition of this unlimited prevalence and limited action, it is an obvious objection to the atmosphere as the immediate cause of the disease. An epidemic atmosphere which exists, but does not produce any appreciable effect, may be conceived extended over the whole globe, as easily as over a country. Of course it is quite clear that such an atmosphere could not be kept out by quarantine; but will any reasonable man say that this is any reason why *the disease*, which this atmosphere only fosters and permits to spread, should not be kept out?

What are the characters of the epidemic atmosphere? We are told that the outbreak of influenza in London, in November 1847, was preceded by storms of wind and rain, sudden alterations of temperature, fog, and absence of electrical phenomena. Cholera in

St Petersburg in June 1848, was accompanied by high winds, rain, thunder, and changes of temperature. Plague is constantly accompanied by great moisture. In our own country, Mr Wagstaff found that when the atmosphere was warm and moist, he "could always foretel that there would be an increase of malarian disease *of some sort*," in *close*, *undrained* and *crowded* places.

This, with some remarks on the periodicity of epidemics, constitutes nearly all the evidence that the Board have to adduce on the epidemic atmosphere. On the other hand we have to state, that we have examined the meteorological returns for London during the late cholera, without discovering any very marked coincidence between the phases of the disease, and the atmospheric conditions; that great differences were noticed between the atmospheric conditions in Dumfries during the epidemics of cholera in 1832 and 1848 (see Dr Barker's Report, p. 940); that in Edinburgh, the weather was fine and dry during a considerable portion of the last visitation; and varied extremely during that of 1832, which lasted from January to November. Further, during 1847-8, typhus fever established itself in Edinburgh during upwards of a year, and was observed to follow a tolerably regular course of increase and decline, amid all the variations of weather for which the northern metropolis is famed.

We advert to these facts, the first which present themselves to our recollection, not because we by any means disbelieve in the *possibility* of some atmospheric conditions favouring the diffusion of cholera, but merely to show that nothing like an epidemic atmosphere has yet been demonstrated. The admission of the Board of Health, is to much the same purpose, though we cannot see in it any reason for talking of an epidemic *atmosphere* so confidently, in preference to an epidemic *influence;* and still less reason for putting forward the doctrine of epidemic influence, as bearing on the quarantine question.

"We cannot tell, because meteorological science, as connected with the propagation and spread of disease, is as yet in its infancy. We have, indeed, some knowledge of the influence of two of the more obvious conditions, namely, those of heat and moisture; but of the action of the subtler agents, such as electricity and magnetism, the present state of science affords us little information. Still there are unequivocal indications (?) that there is a relation between the conditions of the atmosphere and the outbreak and progress of epidemic disease, *though we are as yet ignorant of the nature of that relation;* but, whatever it may be, we may be quite sure that it is beyond the influence of any such agency as that of quarantine."—P. 10.

The end of the Board's inquiries into epidemic atmosphere, or influence, therefore, is nothing more or less than complete ignorance. Yet they have no hesitation in calling it (p. 6), "the primary and essential condition" of the diffusion of epidemic disease, which quarantine "confessedly cannot reach."

But there is one circumstance in reference to the epidemic influence, which the Board have entirely omitted to mention, and which

appears to us the best ascertained of all the conditions regarding it. We do not wish here to make general statements, as the Board have done, in regard to *all* epidemic diseases; but as regards cholera, plague, and typhus, there is no room for doubt that these epidemics, in their transit from country to country, pursue a course accurately corresponding with the great lines of human intercourse. Thus the cholera, from its focus in Bengal in 1818, spread to every part of India, with an entire disregard of its alleged north-westerly direction, and of every other geographical law, but pursued everywhere the great roads and lines of transit, with a constancy which is perfectly wonderful.[1] From India it spread, not westward alone, but to Ceylon, Burmah, the Philippine Islands, and China; it also reached Bourbon, at the same time that it was slowly pursuing its way towards Europe by the land-route of Persia, often against the course of the wind. In no case, as Dr Graves has remarked, has it *ever exceeded the rate of human communication;* in no case has it ever traversed the ocean at a rate exceeding that of ships. Further, on landing in a new country, it has *invariably* attacked first the great sea-ports in constant communication with infected districts.

We have no space to enter at length into this subject, or the same thing might be proved of plague. Laying aside, as open to possible objection, the argument from the extension of that disease up the navigable course of the Danube; it may yet be maintained, without fear of contradiction, that the great *foci*, whence plague has been diffused in Europe, have been the Mediterranean *seaports* in direct communication with Egypt. .Even supposing, therefore, that the propagation of these diseases from one individual to another were utterly incapable of proof, the transmission of the "epidemic influence" from mass to mass of human beings, along the lines of human intercourse, remains a fact so singular and indisputable, that we think the Board were bound to have given it prominence as a character of that influence, instead of putting forward a number of obscure and variable atmospheric conditions, only to mask the admission of their utter ignorance of the subject. As to typhus fever, we know no well informed medical man, with the exception of the medical advisers of the Board, who has ventured to dispute that this disease was, in 1847, directly *imported* from Ireland into Scotland, England, Canada, and the United States, by the immense numbers of emigrants who swarmed into Liverpool, Glasgow, New York, Montreal, and thence into various other parts of these coun-

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[1] All doubt on this subject will be removed from the mind of the reader, by a glance at the map prefixed to Mr Scot's Madras Report, in the abridged edition published during the present year, and the perusal of the excellent and instructive narrative contained in the same volume, which we regret not to have sooner found an opportunity of recommending to our readers. The whole of this subject, as regards cholera, has been admirably and clearly treated by Dr Graves. See his "Practice of Medicine," Dr Neligan's edition; Vol. i. p. 383.

tries. But on this subject we have some remarks to make further on.

In the few words we have to say upon the views of contagion advocated by the Board, it is not our intention to enter into the discussion of the contagiousness of cholera and plague, to which subjects considerable space is devoted. We shall have other opportunities of doing justice to these important topics; and at present, we have sufficient occupation in noticing the views of the Board on the general question of contagion, and its influence over the spread of epidemics. That the Board attach extreme importance to their own views upon this subject is not to be doubted, seeing that they have devoted forty pages of the present report to its consideration, although they distinctly repudiate the idea of its having anything to do with quarantine. That they are dissatisfied with the opinions current among medical men and the public upon this subject, may be gathered from the repeated denunciations of them as mischievous and requiring correction. But we very much doubt whether they have stated their own views so as to secure comprehension, and we are sure that they have entirely misapprehended the opinions common in the present day among those who call themselves contagionists. Let us take the following passages by way of illustration :—

"We apprehend few will question our former statement that the progress of the opinion of observers in Europe during the last half-century has been steadily towards a material modification, if not an entire abandonment, of the doctrine of contagion with reference to the majority of epidemic diseases, taking the word contagion in its strict sense, that is, the communicability of disease exclusively by contact: direct, that is, with the body or breath of an infected person; or indirect, with something which an infected person has touched."—Pp. 18, 19.

"There has been much confusion of terms in respect to the use of the words contagion and non-contagion. We have had instances of professional men who avowed their belief of the contagiousness of typhus, and stated that they had experienced it in their own persons. When asked for the evidence on which the belief was founded, they have usually related some circumstances such as those above described, showing, not the *contagiousness*, but the *infectiousness* of the disease. Contagion is a term applicable to a different set of circumstances. According to the hypothesis of contagion, no matter how pure the air, no matter what the condition of the fever ward, if the physician only feels the pulse of the patient, or touches him with the sleeve of his coat, though he may not catch the disease himself, he may communicate it by a shake of the hand to the next friend he meets; or that friend, without catching it himself, he may give it to another; or if the physician wash and fumigate his hand, but neglect the cuff of his coat, he may still convey the deadly poison to every patient whose pulse he feels during the day. If this were so, the track of a general practitioner who attended one patient labouring under a specific epidemic disease would be marked by the seizure of the rest of his patients; if it were true of cholera and typhus, some members of this Board must have fallen by these diseases, who from morning until night received inspectors that came from places where these epidemics were rife; and if any disease of common occurrence really possessed such powers of communication and diffusion, it is difficult to conceive how it is that the human race has not been long since extinguished."—Pp. 46, 47.

We need not point out to any medical or well-informed reader the monstrous caricature, or rather misrepresentation, contained in the latter of these two passages. If the Board can find a single medical man in this country, who believes in the properties of contagion as here set down, they will have made a discovery more remarkable than any they have as yet given to the world. We doubt if any contagionist writer, from Jacopo da Forli to Dr Copland, ever drew such a picture, or ever conceived that contagion might multiply itself, through shaking of hands or feeling of pulses, to the utter extinction of the human race. We need not, surely, tell the Board that no contagionist—at all events of recent times — has overlooked the power of the atmosphere to diffuse or weaken the contagious influence, or has contended that it may be communicated to all persons or under all circumstances. The frequent failure of direct inoculation, with so concentrated a virus as that of small-pox or cow-pox, is sufficient to show the influence of modifying circumstances. But the Board has chosen its illustrations of contagion rather from the distempered visions of ignorant old women, than from the writings and conversation of medical men and men of the world.

We apprehend, that whatever may have been the scholastic distinctions drawn as to the different kinds of contagion, practical writers have generally concurred in applying this term to the *communication of disease from the sick to the healthy*, simply and without reference to the *method or channel* of communication. This is, indeed, the great practical question, which ought never to be lost sight of in hair-splitting differences between infection and contagion. It would be easy to show that the earlier believers in a contagious aerial virus, included this always under the term contagion; and it was not till a later age, that those who were anxious to throw doubts upon the intangible, while forced to admit the tangible virus, transferred to the latter the name of infection, which originally referred only to the corruption of the air from any source. But this is unnecessary, for as the habitual use of the word contagion in this country is in the sense we have assigned to it, we shall not adopt the over-refined and unpractical distinction of the Board of Health.

Taking the word in this sense, we submit that the question of contagion resolves itself into that on which almost all really practical deductions rest;—viz. in a certain disease (epidemic or not) are those placed in proximity with the sick, *ipso facto*, more liable to attack than those similarly circumstanced in other respects, but placed apart from the sick? In the conduct of this inquiry, which is one of more pure induction than perhaps any other medical question, we hold that collateral theories, such as those of epidemic influence and local miasm, and of the identity or analogies of various epidemic diseases, have frequently been allowed to exercise a most undue influence. *The question of the contagiousness of a disease*

is not affected by the existence or non-existence of other modes of origin or propagation. It is a simple and independent question, which must be decided by pure observation of the facts, as to each disease on which suspicion rests; if decided in the affirmative, it does not necessarily exclude any of the other theories we have alluded to, which must all of them stand or fall by their own evidence. It is clear, that vague analogies should only be admitted in this case to bias the mind when the method of observation fails to yield the requisite results.

Had the Board adopted this simple method of inquiry, they might perhaps have taken a view of their duties, which would have resulted in the publication of a body of facts capable of easy application to the subject before them, instead of a treatise on some obscure doctrines of general pathology, not too easily dealt with even by the hardest heads. Had they taken this course, we should not have been under the necessity of objecting to their proceedings as we have done, for they would then have discussed the questions laid before them in a practical spirit, and not have obtruded their medical and erroneous opinions upon the public. Finally, they would have avoided the inconsistency of declaiming against contagion, and then admitting it as probably extending over a far wider range of diseases than medical men in general have claimed for it. Such an admission, we think, will be found in the following passage, notwithstanding the cautious negative of Dr Sutherland.

"It is not unreasonable, however, to suppose that a single infected person, and much more a large body of infected persons localising themselves in the midst of a population already predisposed to disease and actually under an epidemic influence, may act upon that population zymotically, that is, as the leaven that sets in action the fermenting mass. 'It is difficult,' says Dr Sutherland, 'to obtain satisfactory experimental evidence, but I have an opinion, founded on the result of a great number of observations, that—1. A healthy person, sleeping for a single night in a locality where cholera has localised itself, may return to a healthy locality and take cholera and die, and no one else suffer? 2. If there be a slight epidemic influence present, perhaps one or two persons in the house may suffer. 3. If the influence be still stronger, a few cases may happen in the neighbourhood, and that without any communication with the infected house; and should the epidemic influence increase in intensity, these dropping cases may be fresh centres from which the disease may spread with rapidity. I look on the poison which propagates cholera in an epidemic atmosphere as being truly *zymotic* but not *contagious*. *Why may it not be the same with other pestilences?*'"—P. 55.

Similar admissions are made as regards typhus fever, but far more strongly.

"These rooms are generally filthy, and always ill-ventilated; and their pent-up air is rendered noisome often by the long retention in them of the evacuations of the patients, as well as by the poisonous exhalations from the lungs and skin, which, having no exit, become highly concentrated. A medical man enters such a room; remains in it during the time which is necessary to exa-

mine and prescribe for the sick, and to give instructions to the nurses and attendants; in the anxiety of his professional duty he forgets the precautions due to himself; he breathes this contaminated air in all states of health, perhaps when weakened by indisposition or exhausted by fatigue; at last he is seized with the disease, or, as it is commonly termed, he catches the fever. In like manner the Catholic priest, as too frequently happened during the prevalence of the recent epidemic typhus at Liverpool, who receives the dying confessions of the sick, whose ear is commonly close to the mouth of the confessing person, and whose visit is often protracted considerably longer than that of the medical practitioner, suffers proportionately in a greater degree even than the latter. But if these offices were performed in the open air, where the poisonous exhalations are immediately diluted, and by that dilution destroyed, no injurious effect would happen. In point of fact, in various fever hospitals, where due attention is paid to ventilation, and where care is taken to prevent the wards from being overcrowded, such offices are daily performed by nurses, medical students, physicians, and priests; and many years sometimes pass away without the occurrence of a single case of fever among such officers. The same is true with regard to the relatives of the sick who come to visit them in these wards, and who sit upon their beds, touch their bodies, and handle their clothes without sustaining injury."—Pp. 45, 46.

No one denies the position so anxiously contended for by the Board, that the most important means of neutralising contagious or epidemic influences is by encouraging free ventilation. No contagionist of the present day denies that a moderate degree of dilution will render the strongest contagious virus inert, as happens indeed with all other poisons; and the extent of the necessary dilution is certainly understood much better now than at any previous period. Dr Haygarth's principle of treating fever and smallpox, with proper precautions, in ordinary wards, was introduced some years ago into Edinburgh Infirmary by the staunch contagionists who formed its medical staff, as a measure of protection for the nurses and attendants from the injurious effects of strictly *fever* wards; and although the plan has been found to fulfil its object without serious danger to the general patients, this circumstance is viewed as an apt illustration of the laws of contagion, instead of being supposed to disprove contagion itself.

We contend, however, that after the admissions in the passages above quoted, it is mere folly and trifling with language on the part of the Board to deny the contagious properties of any pestilential disease. As these diseases, according to the Board, "all obey similar laws of diffusion," it is clear that they are *all* contagious, in the sense in which that term is commonly accepted. The importation of the Pali plague into India, and of typhus fever into the towns and villages of Britain and America, ought to require, in their eyes, the very smallest possible amount of proof, if this zymotic hypothesis be admitted as general. But the reader of the above passage in regard to typhus, will be surprised to learn that there is another, in pp. 84, 85, in which considerable doubt is thrown on the importation of this disease into our large towns from Ireland, on grounds so absurd, that we really do not know what to say of them. On carefully considering this passage,

however, we find it so equivocally worded, as to leave us quite in the dark as to whether the Board means after all to deny or affirm the importation. We think it unnecessary, therefore, to trouble our readers by giving them this passage at length; and we shall only say that, in our opinion, the importation of the disease by the Irish, and its extensive *zymotic* or contagious diffusion among our own poor in England and Scotland, are facts as certainly demonstrated, and of as terrible an importance in a sanitary point of view, as any in medical science.

Our readers are now in a position to judge whether the Board have succeeded in overthrowing contagion, establishing epidemic influence, and proving, as regards quarantine, "that the fundamental principle upon which it is based is fallacious, and that the only means of preventing the origin and spread of epidemic diseases is by sanitary measures." For our own parts, although we consider quarantine reform indispensable, because the practical working of quarantine has been inefficient and cruel, besides being, in many instances, really a violation of its own principles, we yet believe the main principles upon which it was based to be unquestionably true. In any alterations which are proposed, therefore, in our quarantine system, it is our duty to keep in view those principles, as far as may be consistent with humanity and sound policy; and we doubt not that a far more extensive and wise application of them than was ever possible under the existing quarantine system, with its numerous loop-holes and evasions, may be carried out, without maintaining the present intolerable restrictions on commercial enterprise.

The necessity of discussing, in the first place, the opinions of the Board of Health on the laws of the diffusion of epidemics, obliges us to reserve, for a future occasion, the question of quarantine reform. We may, however, so far anticipate what we shall have to say on that subject, as to assure the Board, that the medical profession in this country, as a whole, is by no means disposed to view with superstitious alarm, alterations which are really demanded by the progress of civilisation and the ever-increasing commercial relations of this country. Medical men, or at least the more enlightened among them, claim to be persons, not merely "*capable* of observation," but persons who *have* observed, and who, moreover, have calmly considered this subject in all its bearings, and with all its evidence before them. Although they cannot pretend to the inestimable advantage of having "nothing to unlearn," they trust that they have learned some things not unworthy of consideration from the accumulated and recorded experience of four centuries; and we are certain that the General Board of Health would have acted in a manner more calculated to advance a policy, which we acknowledge to be, in many of its tendencies and even details, enlightened and humane, had they availed themselves of the knowledge and experience of the medical body in general, instead of exciting the jealousy and distrust of that body,

by throwing themselves into the hands of those whose opinions are considered by the majority of medical men to be at once crude, inconsistent, and dangerous. We fear that much really important influence will be lost, and much violent opposition, both liberal and illiberal, roused by this unfortunate and rash step. We shall, however, give to the reforms of the Board our most careful consideration, conscious that erroneous views, either hastily adopted or illiberally retained, are especially injurious, and especially unjustifiable, in that profession which has ever been, and still continues to be, regarded by the public as its proper advisers in questions of the kind treated of in this report.

Practical Pharmacy: the Arrangements, Apparatus, and Manipulations of the Pharmaceutical Shop and Laboratory. By FRANCIS MOHR, Ph. D., and THEOPHILUS REDWOOD. Illustrated by 400 Engravings on wood. London, 1849. Pp. 390.

MR REDWOOD has done good service to the art of which he is a successful cultivator, in presenting to the British public a translation of Mohr's "*Handbuch der Pharmaceutische Technologie*," a work exactly such as has long been imperatively called for by the state of pharmacy in this country. We are in the habit of pluming ourselves on our great superiority over every other country in the practical arts; but it is impossible to conceal from ourselves that there is at least one in which we have no pre-eminence to boast of, for on almost all that relates to the practice of pharmacy, the British druggist is far behind the German "Apotheker," and the French "Pharmacien."[1] We are well aware that there are many exceptions to this statement; and that in most of our larger towns there are men of education, character, and intelligence who practise the business of druggists, and are in every way equal to even the best of other countries; but, unfortunately, these are the exceptions which prove the rule, and serve only by contrast to point out more distinctly its truth. Hitherto the business of the druggist has been looked upon much more in the light of a mere trade, than in that of a liberal profession, as both custom and law have made it in other nations of Europe. This state of matters, and its disadvantages, have, however, at length forced themselves upon the attention of the more intelligent of our druggists, and led to the formation of the Pharmaceutical Society of London, which, with the aid of its very ably conducted "Journal," has, by its active exertions in promoting a regular system of scientific education for the druggist, already done much to raise his status, and to supply a superior class

[1] In the strictly commercial department of the drug business, we believe that Britain is superior to all other nations.

of men to occupy what is in reality a very responsible position, and one demanding no ordinary amount of intelligence and attention. Things being in this transition state, Mr Redwood has, we repeat, done both a good and a well-timed service to the druggist, in affording him the means of acquiring a knowledge of the methods and practices of the German apothecary, by translating the work before us. It is true that their methods are in many respects very different from those practised in this country; but Mr Redwood has both foreseen and provided for this difficulty, by supplying us with, not exactly a translation, but rather what a German editor would call a "*Bearbeitung*" of Mohr's work, in which those portions which may be considered as purely German have been excluded, and replaced by other matter more in accordance with the practices of our own druggists. Indeed, if we have a fault to find with a work in which the duties of editor have been so well and so carefully performed, it is that perhaps a little too much has been done in this way, and that it might possibly have been more advantageous to our own druggists had some of the excluded matter been permitted to remain, or, at least, had some general account been given of the management of the business of druggist in Germany. This struck us forcibly in reading the very first section of the work, where we have an extended description of a druggist's shop according to the English system, with very minute details regarding ventilation, &c., but without any reference being made to the German "*Apothek*," although the latter presents many points worthy of imitation, especially as regards the preservation of poisonous drugs, and the precautions to be attended to in dispensing them, so as to avoid those unhappy accidents which too frequently occur in this country, from their being confounded with innoxious substances. This, however, is a point on which difference of opinion may reasonably be expected to exist, and does not in the least detract from the merit due to Mr Redwood for the very admirable manner in which he has performed the duties of editor. Indeed, a very considerable proportion of the volume is exclusively the work of Mr Redwood, and the amount of labour which it must have entailed upon him, is such, that a very little addition to it might have sufficed for the production of an entirely original work.

A treatise on such a subject as practical pharmacy being made up essentially of minute details, does not admit of a general critical analysis. The one before us is comprised in three great divisions. The first treats of the general arrangements of the laboratory, the best method of fitting it up, and the arrangement of furnaces, boilers, &c.; the second of the special arrangements and manipulatory processes, illustrated by excellent woodcuts of the instruments and apparatus employed in each operation; and the concluding section gives an account of the dispensing of drugs in all their different medicinal forms. In each of these sections we have eminently practical details explained with great clearness and precision.

We give an outline of the section on the preparation of extracts and tinctures in illustration of the mode in which each separate subject is discussed.

The section referred to is commenced by treating of the three different modes of making watery extracts, by boiling, digestion at a moderate temperature, and maceration in the cold. Having shown that in most cases the process by infusion yields extracts better in quality and greater in quantity than those obtained by decoction, the authors proceed to consider what is the best method of proceeding in the preparation of extracts by infusion. As the fluid menstruum becomes charged with the soluble matter of the substances operated upon, it is desirable to secure its rapid removal, which may be done by the press, by displacement with high pressure or with low pressure. These processes are minutely described, and their respective advantages duly weighed. After this follows the process of evaporation, and the comparative advantages of the open fire, the water bath, steam bath, spontaneous evaporation, and evaporation in vacuo. And the authors insist particularly on the precautions requisite for the successful performance of this part of the process. They animadvert, and with justice, on a practice which it appears is not uncommon in the preparation of the extracts of henbane, hyoscyamus, aconite, and other green extracts, of separating the chlorophyle, and again adding it after the evaporation is completed, to give the extract a green colour, which is by many considered a test of its good quality. They observe :—

"This practice may serve for the deception of those to whom the extract is sold, but it can answer no good purpose. The chlorophyle itself possesses no medicinal activity, and its rejection altogether has been proposed by some pharmaceutists, with a view to the improvement of the extracts. If, however, it be retained, as is directed in our pharmacopœias, it should be subjected to all the influences to which the other parts of the extract are exposed, so that it may serve as an indicator of the care and judgment which have been exercised during the process of evaporation; for as this substance is very easily decomposed, and its green colour destroyed, by those agencies which cause the decomposition of other more active constituents of the extract, its unaltered condition, if it have remained present during the whole process, will afford good grounds for assuming that the extract in other respects is uninjured."—p. 71.

Extraction with alcohol and ether, and the preparation of tinctures, next fall under consideration, and here Mr Redwood has discussed at great length the important question of the comparative advantages of the processes of maceration and percolation in the preparation of tinctures. Our readers are aware that the authors of the Edinburgh Pharmacopœia were the first to recommend the adoption of the method by displacement; and that they have given, in the last edition of that work, directions for applying it in the preparation of most of the tinctures. We subjoin Mr Redwood's concluding summary of the comparative advantages of the two methods.

"In reviewing the several advantages and disadvantages which attach respectively to the two processes which are recognised in the British Pharmacopœias, namely, *the process of maceration* and *the process of displacement*, it would appear, on the one hand, that the objections to the process of maceration apply more strongly to its adoption on the small, than on the large scale; and, on the other hand, that the process of displacement is less applicable to operations on the large, than on the small scale.

"When tinctures are made in large quantities, percolation is never likely to supersede maceration, on account of any practical advantages it may possess. If the prescribed directions be duly attended to, the process of maceration is unexceptionable. This process is more simple than the other; the mode of operating is more uniform,—it is, in fact, always the same; it requires less of skill and dexterity in conducting it; it requires less constant attention during its progress, which, in operating on large quantities, is a consideration; and, finally, the apparatus required is less complicated.

"When only small quantities of tincture are made at a time, and kept in stock, the adoption of the process of displacement will often be found convenient and advantageous. It offers the means of making a tincture in two or three hours, which by the other process would require as many weeks. The process being thus completed in so short a time, for the quantity contemplated might be made at one operation, it would not be so likely as the other to experience neglect during the performance of it, or a deviation from the prescribed instructions; the product would therefore be more uniform. Finally, in many cases, the tincture and spirit may be removed from the dregs more completely, in operating on small quantities by this process, than by the other.

"Even in those establishments where tinctures are generally made by maceration, the process of displacement will be found useful as a resort in cases of emergency, and for the preparation of some particular tinctures."—p. 95.

In these remarks there is a great want of that precision which has been described as generally characterising the work. Mr R. should have indicated more exactly what he means by the phrases "small" and "large quantities," and should have had some more satisfactory reason than any he has offered, for stating that the process he recommends as the best in small, should not be, as we might expect, equally advantageous in large operations.

In the laboratory of Messrs Duncan and Flockhart of this city, all the tinctures, excepting those of kino, catechu, opium, and one or two other drugs, have been prepared by percolation during the last nine years. The quantity of tincture made at one operation varies according to the medicine from two to eight gallons. They have found this method not only more convenient, economical, and expeditious, but a better product is obtained, and the exhaustion of the drug is obviously more complete than when maceration is employed. The solid residue left after the preparation of the tinctures of ginger and rhubarb, is altogether void of taste or smell. The skill and experience required to regulate the degree of comminution to which the substances are subjected, and the firmness with which the powders are packed in the cylinder are readily attained by a little attention and practice.

In the preparation of infusions, the process by displacement with cold water has, in their establishment, almost entirely superseded the older methods, and it is found to have the same advantages in ope-

rating on all quantities, from one to a hundred gallons. The passage of the liquid through the solid matters is aided, when necessary, by withdrawing the air from the space between the percolating frame and the bottom of the vessel, by means of the air-pump. The employment of percolation in the preparation of soap-maker's ley and in brewing affords additional illustration of its successful application on the large scale.

The last great division of the work is almost entirely from the pen of Mr Redwood. It treats of the dispensing of medicines as practised in this country with an accuracy and minuteness which are not to be found in any other treatise. We extract the following remarks on the covering of pills on account of their practical importance.

"Several substances are used for covering pills, such as *magnesia, starch, liquorice powder, lycopodium, gold and silver leaf, gelatine*, and a mixture of *gum and sugar*. The application of these substances to the surfaces of pills is intended to prevent their sticking to each other or to the box, and also to prevent their being tasted during the act of deglutition.

"*Magnesia* is very commonly used for covering pills. As a light absorbent powder it answers the required purpose very well, yet there are some cases in which its use is not free from objection. Thus, for instance, if calomel pills be covered with magnesia, decomposition will, after some time, occur, the mercury being reduced, or oxide formed together with muriate of magnesia. Calomel pills that are kept ready made should never, therefore, be covered with magnesia; powdered starch might be used in this case.

"*Liquorice-powder* is sometimes employed in preference to magnesia for covering pills, its sweet taste being considered advantageous in masking that of the other ingredients of the pills. There is, however, a very serious objection to the indiscriminate use of this powder, which arises from the fact that, with some persons, it occasions an irritation of the fauces, which deprives them of the power of swallowing pills which are thus covered.

"*Lycopodium* is but little used for covering pills in this country. It is extensively employed on the continent, and it forms, undoubtedly, the most suitable powder for the purpose. It is a light powder, the particles of which readily adhere to the moist surfaces of pills, without becoming themselves moist. It is also free from taste, and has no tendency to cause or to undergo decomposition. When lycopodium is used it should be applied to the pills on the machine or in the finisher, and none of the powder, excepting that which adheres to the surfaces of the pills, should be put into the box. Pills thus prepared have a much cleaner and more finished appearance than those to which a quantity of unattached powder is added, as is generally the case when magnesia or liquorice-powder is used.

"The application of *gold or silver-leaf* to the surface of pills is a very ancient method of covering them. But of all the methods adopted this is the most objectionable. Gilded pills have often been found to pass through the entire alimentary canal without undergoing any alteration, being completely protected by their metallic covering.

"*The covering of pills with gelatine* is the most elegant and efficient method of fulfilling the objects contemplated in the processes now under notice. A pill, when thus covered, has a clean, shining surface, which is dry, hard, and not at all sticky. No powder is, therefore, required in addition to the gelatine. The ingredients of the pill, being enclosed in a gelatinous case, are preserved from the action of the air, and, to a certain extent, are prevented from undergoing volatilization; moreover, the pill itself may be swallowed without perceiving taste or smell."—p. 355.

The method of covering pills with gelatine is then described and illustrated by woodcuts.

Mr Redwood deserves the best thanks of the profession for the very excellent style in which he has executed his task; and our readers will find much in the work which will repay perusal, even though belonging to the ranks of the pure physician or surgeon. The country practitioner, who is obliged to dispense his own medicines, will find it a most valuable assistant.

Essai sur l'Emploi Thérapeutique des Ferrugineux; par. M. le Dr Jacques, Bruxelles. 1848. 4to pp. 114. (*On the Therapeutic Uses of Iron and its Preparations*).

Essay on the Use of Alcoholic Liquors in Health and Disease. By John Chadwick, M.D. London 1849. 8vo. Pp. 123.

We regret that we cannot speak favourably of either of these essays. Neither one nor other has any pretensions to original inquiry, without which very few indeed are the monographs in therapeutics which merit publication.

The essay of M. Jacques was written in competition for a prize offered in 1844 by the Belgian Royal Academy of Medicine. The number of competitors is not mentioned, but "le prix n'a pas été décerné; une medaille d'or a été accordée à M. Jacques, à titre d'encouragement." It is curious that the fact of having written an essay, which, in the absence of more able competitors, has been declared unworthy of the prize, should give the author so much satisfaction, and still more mysterious that the Belgian Academy should declare an essay unworthy of their prize, and at the same time honour it with a place in their "Mémoires." The essay is a long, and upon the whole fairly executed, compilation from the current medical journals and works in materia medica; it is one, however, through which we have waded with great perseverance without discovering a single new fact or opinion, on which to repose for a moment our wearied attention. It is possible that the author is a young man, and truly merits the *encouragement* he has received, but the academy have placed him in a false position by the publication of his memoir.

The subject of Mr Chadwick's essay was announced for prize competition in the beginning of 1848, and the essays were to have been delivered on the 31st of December of the same year. In December, however, it was publicly advertised that the time should be extended to October 1849. Mr C. having his essay ready for December, is naturally, and we think justly, offended at the postponement; but we do not sympathise with him in his impatience for authorship. Even if his essay had lain "useless in his desk" for nine months, he could have occupied the time profitably in its

improvement. It is a very ordinary production, made up in great part of extracts from other writers, and might have been entombed in Mr C.'s desk without any loss to the public.

We hope that the liberality of the philanthropic donor of the prize will be rewarded in the calling forth of an able and original memoir on the highly important subject selected for competition; and we look forward with interest to the publication of the successful essay, when we trust to be furnished with materials for a full consideration of the use of alcoholic liquors in health and disease. The candidates have been wisely kept in ignorance of the donor's sentiments respecting temperance societies, as unfortunately the strong party feelings exhibited in the discussion of this question tends to obscure the truth.

We take this opportunity of pointing out a common error in the selection of subjects for prize essays, the object of which, in our opinion, should be to foster original inquiry. These are generally of too comprehensive a character, often embracing matters which, for their complete discussion, would occupy volumes. Either the competitor, in his anxiety to be as comprehensive as his subject, is reduced to a mere compiler, in which capacity that time is consumed which the donor had vainly hoped would have been devoted to original investigations; or the original inquirer is altogether deterred from entering the lists from a consciousness of his inability to elucidate so wide a theme, or from the conviction that the time required for this purpose would be longer than he could with propriety devote to it. It appears to us that the subjects of prize-essays ought to be of such a nature as to admit of their complete investigation within a limited time, and thus make the competition one more of talent than, as it too often is, of mere industry.

For example, in place of the subject which forms the title of the first of the two books above-noticed, it would have been better had the labours of the candidates been confined to any one of the following questions:—What changes do iron and its preparations undergo in the stomach and intestines? In what form do they enter the circulation, and what changes do they undergo there, and in their passage into the secretions? Is there an absolute increase of iron in the blood during a chalybeate course? Have the preparations of iron a direct emmenagogue action? Is iron ever contra-indicated in chlorosis, and in what cases? Has iron any action on the healthy spleen? and so on a variety of interesting questions may be suggested; some easy, others difficult, but the definitive solution of any one of which would be an important step in medical science, and would redound to the author's fame more than a hundred such essays as that of M. Jacques, which comprehends (as required by the Belgian Academy) the consideration of iron, and the ferruginous compounds, in all their relations,—historical, physiological, therapeutical, and pharmaceutical.

Lectures on the parts concerned in the Operations on the Eye, and on the Structure of the Retina and Vitreous Humor. By WILLIAM BOWMAN, Professor of Physiology, &c., in King's College, London; Assistant-Surgeon to the King's College Hospital, &c. &c. Pp. 143. Longman: London. 1849.

MR BOWMAN is well known, more especially as a leader in the investigation of the physiological anatomy of the human body, and we are here presented with the results of his labours in a new field for microscopical observation, which cannot fail to increase his well merited reputation.

These lectures were delivered at the Royal London Ophthalmic Hospital, Moorfields, in June 1847. They consist chiefly of observations on points in the anatomy of the eye, regarding which previous anatomists have differed, or where the ordinary means of investigation could furnish no further information. The observations of chief interest are those on the structure of the cornea, a new elastic layer in front of which he has discovered, not previously noticed, although it is thicker than the elastic layer behind; the ciliary ligament and greyish zone around it, which he finally proves to be muscular in its nature; the suspensory ligament of the lens, by which the ciliary muscle can advance the crystalline for the vision of near objects; the fibres of the iris; and the lens and its capsule; in addition to the vitreous humour and retina, on the latter of which, more especially, much interesting information is given.

Also, the pathological application of his observations is pointed out, in connection with which an account of the microscopical examination of various diseased conditions is given, and an appendix of select and illustrative cases added. By such observations he has opened up a way by which we are likely to acquire a more intimate knowledge of certain pathological conditions of this organ, as well as better to understand the indications for their treatment; and in which those only can follow in whom are combined, as in Mr Bowman, the anatomist and physiologist with the pathologist and ophthalmic surgeon.

We seldom meet with so much new matter in so small a compass and so unpretending a form, but we think that in such circumstances the writer should be more careful in mentioning the limits of his originality, where it begins and where it ends; as different readers will be very apt to fall into opposite extremes in estimating the original merit of the work before us; and the mention of these limits would have in no way involved a departure from the modesty which characterises it.

There is so much new matter in these lectures, that we have resolved to present our readers with an abstract and brief comment upon them in the "Retrospect," to which, therefore, we now refer;

but we cannot conclude this notice without again expressing our high opinion of the author. He appears to be accomplished alike as an anatomist and microscopical observer. As a writer, he is plain, concise, and unassuming. He appears to possess the rare faculty of describing things as they exist, without being biassed by preconception, or by the theories of others; and we are therefore accustomed to hold his opinions in respect, and to receive his observations as trustworthy and accurate.

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVIII.

MEETING X.—*April* 18, 1849.—DR BEGBIE, V.P., in the Chair.

FIBROUS GLANDULAR TUMOURS.

Dr Mackenzie exhibited three fibrous tumours which Mr Syme had recently removed by operation. One of the tumours was of great size,—indeed, the largest which Mr Syme had ever removed from the mamma without sacrificing the gland. The second, a round fibrous mass of about two inches in diameter, had also been taken from the female mamma. As this patient's mother and sister had died of cancer, the daughter's case would probably, in the olden time, have been regarded as of unfavourable prognosis. The third tumour was from the sub-maxillary region. Mr Syme had removed all three within the last two days, and a fourth of a similar kind from the sub-maxillary region; he had likewise seen one requiring removal from the parotid. He remarked that these fibrous growths were most frequent in the neighbourhood of the conglomerate glands. They were sometimes of slow growth,—the large one from the mamma had existed for twenty years; and the smaller one for eight years.

DISSECTIONS OF STUMPS AFTER AMPUTATION.

Mr Spence produced several dissections of the stumps left after amputation of the leg, thigh, and fore-arm. He first called the attention of the society to the mass of muscular tissue which overlapped the bones in some of his preparations. The leg belonging to one of these stumps had been amputated by the flap operation, twelve years before the patient's death, and the stump was still well-formed, and covered with muscle. He was inclined to doubt, whether the amount of atrophy was so considerable in the muscles of stumps after amputation, as was generally believed.

It had been stated by Mr Hargreave, that after amputations performed during early life, the bone included in the stump continues to grow in length, and two of the bones (femora) now before the Society, seemed to have acquired some such increase. In the upper part of a femur, from the University Museum, there was likewise an apparent increase in length, but this was probably due, not to any fresh deposit of osseous tissue, but to the altered obliquity of the neck and shaft of the bone, consequent upon the weight of the patient's body having been, after the amputation, allowed to rest principally upon the other leg. Two of Mr Spence's preparations, exhibited an elongated point of bone protruding from the surface, which had been divided by the saw, the end of the bone was *conical*, not rounded off as after amputations performed on the adult.

Mr Spence believed, that neuromata were always and unavoidably formed when nerves were divided, that they were present, by whatever method amputation was performed, and however deeply the nerves were covered. When a nerve was cut across, the neurilemma retracted, the nervous fibrillæ projected, and ultimately became firmly connected with lymph effused about the point of section. Some fibrillæ were spread out upon the surface of the lymph, others were imbedded and interwoven in its substance. The tumours thus formed were not productive of pain unless exposed to pressure between the bone and skin, or so thinly covered as to be affected by atmospheric influence, and change of temperature. The size of these neuromatous tumours was not dependent upon their being more or less thickly covered, and in some of Mr S.'s dissections, very large ones were exposed, deeply seated in the stumps. An extended investigation, by means of similar dissections, might lead to more determinate views as to the plans of operating best adapted for securing a well-formed stump.

INTESTINAL CONCRETIONS OF EXTRAORDINARY SIZE, IN A YOUNG FEMALE.

(*In a letter from Dr Ritchie, Physician to the Royal Infirmary, Glasgow. Communicated by Dr Cumming.*)

"The mass which I sent you the other day, which you will observe consists wholly of long human hair, was extracted after death from the stomach of a girl aged twenty-one, admitted into the Royal Infirmary under my care, on the 4th October, 1848. She came in with great meteoric distension of the abdomen, feculent vomiting, and the other usual symptoms of obstructed bowel, and the menses were stated to have been always irregular. On the 5th, when I saw her first, I perceived some hardness to the left of the umbilicus, but the presence of the tumour in question was concealed by the great tympanitic swelling of the bowels till the 12th, two days previous to which the constipation had been overcome, and the general distension removed. The tumour, which lay in the epigastrium, and encroached a little on both hypochondria, and which also presented a rounded convex exterior, and a well-defined margin, simulated the appearance of the liver so strongly, that it was mistaken for that organ, till it was seen that, on grasping it through the parietes, and pulling it downward, a considerable hollow space could be produced between its upper edge and the ensiform cartilage.

"Immediately on the removal of the ileus, which happened under the steady use of half a drachm of magnesia every four hours, an enema of tepid water three or four times daily, warm fomentations to the abdomen, and a grain of opium every six, and then every eight hours, the pulse began to rise, severe diarrhœa came on, with aphthous exudations on the throat, and she died on the 24th, or twenty days from admission. The notes of her inspection are as follows :—

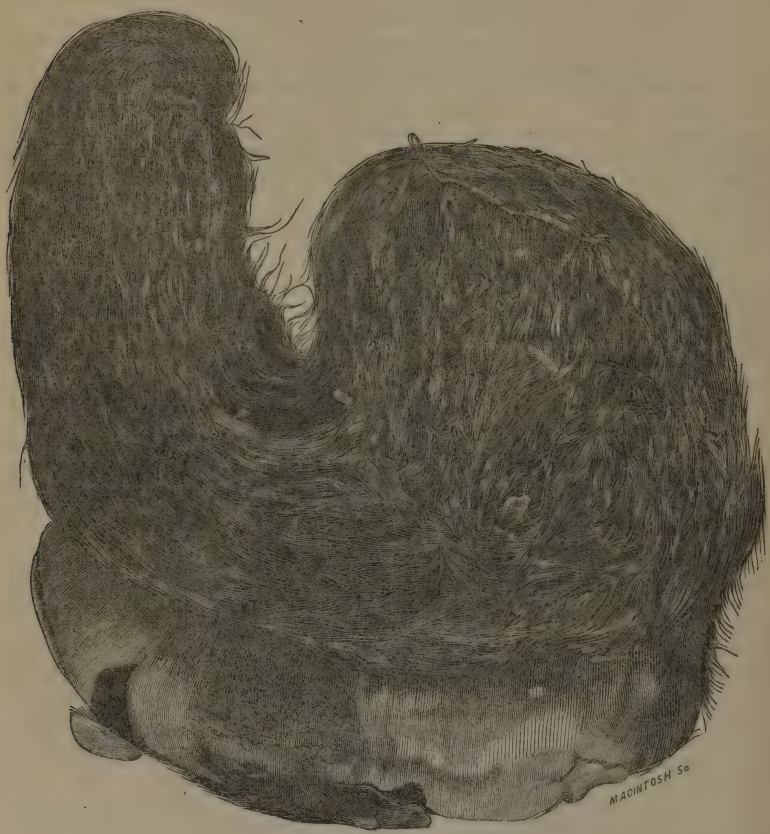
"On opening the abdomen, the tumour formerly described was found to be situated in the stomach, on opening which, a mass of the precise shape of that organ was extracted. It weighed, as removed from the body, twenty-one ounces, and, except at two parts, corresponding to the line of the greater curvature of the stomach, where for about an inch and a half at each part, there was an incrustation of a dirty or ash-coloured pasty substance,—it consisted exclusively of an accumulation of coils of moist female hair. The portions of stomach which had been in apposition with the patches of incrustation on the hair, were ulcerated nearly to the peritoneal coat. The mucous lining was here and there highly injected, and throughout it numerous elevated patches, consisting of groups of follicular orifices in a state of ulceration, were observed. The pylorus was rather thickened, and nearly two inches to its left there was a considerable cluster of ovoid bodies, varying in size from that of a dried raisin to that of a boiled garden pea.

"The small intestines were greatly inflated—very vascular, pretty firmly glued together, and the inflammation extended over much of the omentum,

mesentery, and peritoneum. The pelvis was filled with thin feces, which had proceeded from a rupture in the coats of the ileum, a few inches above the cæcum. Between this lesion and the cæcum, another hairy concretion of one and a half ounces weight, and a little above the rupture a third similar mass of half an ounce in weight, were discovered. These consisted wholly of hairs of various sizes similar to those in the stomach. The other organs were healthy, except that the lungs were congested, and slightly emphysematous.

"This girl came from a distance, but it was ascertained by inquiries made at her mother, that she had for many years had a practice, from which she could not be restrained, of pulling hair from her head while employed at work as a factory weaver, and swallowing it.

"I have not had leisure to think of this case in reference to its relations with malacia in general, or to inquire whether any similar one has yet been recorded. Having happened to get an opportunity of transmitting the concretion to Edinburgh, I thought it better to do so, accompanying it by a simple statement of the facts, than to wait for any season of leisure, when I might do so more learnedly or at least lengthily."



The figure shows the largest of the masses described in the above letter, that which filled the stomach, and presented a mould of its form.

Dr Keiller stated that he had met with a case precisely similar in Dundee. The patient, a factory-girl, was in the habit of eating tow, and the tumour found in her stomach after death, resembled exceedingly that now before the Society.

EPIDEMICS OF MEASLES AND VARIOLA.

A communication from *Dr Ransford* of Alnwick was then read. During an epidemic of measles, which had recently spread widely about Alnwick, Dr R. had met with some remarkable cases occurring in adults. In three of these, the eruption had been well marked, but totally disappeared within two days of its first appearance; in two of them no particular aggravation of the catarrhal symptoms followed; the third, a delicate female, died of pneumonia on the 17th day of the disease, and thirteen days after a premature delivery. Had it been observed that the eruption of measles in adults remains apparent for a shorter time than in children? The solution of this question might afford important aids to the diagnosis of the disease.

Variola had been lately very prevalent in Alnwick. Cases occurring in re-vaccinated patients had been all slight. A lad of eighteen, who would not submit to revaccination, took the disease in its worst confluent form, and for many days his life seemed to be in extreme danger. A second case occurred in a lady who had not been revaccinated since infancy, and who had recently weaned her child. Fourteen days after the appearance of the disease, she had violent convulsions, dilated pupil, and loss of power in both arms, especially the right one. The face was flushed, vision was unimpaired; and she was perfectly sensible. These symptoms passed off in a few days, after the application of cold to the shaven scalp, of a blister to the nucha, and the administration of calomel in two grain doses every two hours, together with the inunction of ung. hydrarg. camph.

MEETING XI.—WEDNESDAY, *May* 2, 1849.—JAMES SYME, Esq., President, in the Chair.

CASE OF PRETERNATURAL ANUS.

Dr Douglas Maclagan communicated the history of a remarkable case of preternatural anus.

[The case will be published in a future Number.]

Mr Syme, who had also seen the patient, made some remarks upon the small encouragement held out by such cases for operative interference.

Dr Spittal read a communication from Dr M. W. Taylor of Penrith, giving an account of a case of involuntary grinding of the teeth. The patient, a clergyman, eighty years of age, had for many years been subject to headaches, and of late had become imbecile after two slight attacks of paralysis. The grinding of the teeth had commenced about six years ago, and of late had been almost incessant, the patient's jaws being never at rest (except during sleep) for five consecutive minutes. The sound emanated from the left canine and incisor teeth, from the crowns of which the enamel was quite rubbed away; it was so loud as to be heard all over the house, and even across a field, and strongly resembled the cry of the corn-crake. Dr Taylor did not consider it likely, that the cause of this symptom in his patient's case, was referable either to the gouty diathesis, or to uneasy sensations in the teeth themselves; he was disposed to refer it to disease within the cranium. A similar symptom, from an analogous cause, was frequently met with in cases of hydrocephalus; but when intestinal irritation from worms was accompanied with grinding of the teeth, the symptom was manifestly an example of reflex action.

Dr Bennett had under his care an epileptic patient, whose teeth were much rubbed away, by the action of the jaw during the fits.

AMPUTATION AT THE ANKLE JOINT.

Dr Richard Mackenzie read an account of a method of amputation at the ankle-joint, by single flap, which he had devised, and which had been five times performed on the living subject by himself and Dr Maclagan.

[The object of Dr M.'s paper was to recommend this operation in some cases to which Mr Syme's method was not easily applicable, especially when the integuments on the outer side of the heel were in an unsound state, the flap being derived from the skin covering the inner and lower sides of the calcaneum, and supplied with blood by the branches of the posterior tibial artery. Dr Mackenzie's communication will appear in a future Number.—Edits. M. J.]

Mr Syme remarked, that it was important to know, that by this mode of operating, a better flap might in certain cases be obtained. He considered the operation difficult, and believed that the great length of the flap might render it liable to sloughing. It was of consequence to state, that his own mode of operation had now been followed by himself in between thirty and forty cases, and in a number of cases by other surgeons, yet he was not aware of any instance in which sloughing had been observed, attributable to violence necessary in detaching the soft parts from the calcaneum. From this cause, therefore, experience had shown that no risk was to be apprehended. He took this opportunity to exhibit an apparatus which might be worn after the operation at the ankle. It was constructed by a boot-maker (Mr Thomas Robson, 18, William Street), whose son had undergone the operation.

Mr Spence and *Dr Mackenzie* made some remarks upon the distribution of arteries to the soft parts of the heel. The former thought the chief vascular supply was derived from the peroneal artery, and from the outer side of the ankle, while the latter contended, that the branches from the posterior tibial and on the inner side of the ankle, were fully more important.

ON THE MANUFACTURE OF CHLOROFORM.

Dr Taylor read a communication from the Messrs Smith of Duke Street, upon the most economical process of preparing and purifying chloroform. They detailed, with great minuteness, the most approved modes of testing the qualities of the materials, of conducting the distillation, and of rectifying the product. They could now sell a chemically pure article for 8d. an ounce, or 10s. a pound by weight, and doubted if the pure drug could ever be produced at a less cost. It had been stated in the Pharmaceutical Journal, by Mr Morson, that the best specimens of Scotch-made chloroform underwent spontaneous decomposition when exposed to light,—that free chlorine and muriatic acid were evolved, and might be detected by their reactions on litmus. A half-filled phial was exposed to the solar rays for four weeks, in Messrs Smiths' window, and even at the end of that period, the fluid had no action whatever upon litmus paper.

Note.—The Messrs Smith have recently noticed, that pure chloroform is decomposed by the direct solar rays when the thermometer is at about 100° F., but that *out* of the direct rays of the sun, the brightest sun light seems to have no effect upon it. June 18, 1849.

MEETING XII.—WEDNESDAY, 16*th May*, 1849.

JAMES SYME, Esq., President, in the Chair.

CONGENITAL EXTROPHY OF THE URINARY BLADDER.

Dr Mackay exhibited a child, a few weeks old, with congenital extrophy of the bladder. A sort of truss had been adapted to prevent the chafing of the clothes, and the escape of urine over the external surface of the abdomen.

Mr Syme remarked, that an operation had been proposed, and in one case executed, for the cure of this malformation. It was scarcely necessary to say, that no good was to be expected from such a procedure. The appropriate treatment consisted in applying a suitable receptacle for the urine. In one case of this malformation which he had met with, the patient lived for many years in a tolerably comfortable condition, being enabled to perform the duties of a public office, to take a good deal of walking exercise, and even to ride upon horseback. He wore a silver vessel, properly secured, over the seat of the disease, to prevent the dribbling away of the urine.

PREPARATIONS ILLUSTRATIVE OF THE COURSE OF TUBERCLE IN THE LUNGS.

Dr Bennett exhibited some preparations in proof of the sanability of phthisis pulmonalis. In one a large deep cicatrix was seen in the upper lobe of the lung. The patient had died in the Infirmary of delirium tremens. From the account given by his friends, it appeared, that about twenty years before his death he had been affected with the ordinary symptoms of consumption, but that he was restored to health, apparently in consequence of improvement in his circumstances and habits of life. In another preparation, two stellate puckerings on the surface of the lung were found to correspond to two caverns with indurated walls, which seemed to be undergoing the process of cure. In other two preparations, numerous small gritty masses were disseminated ; these were specimens of abortive miliary tubercle. These appearances were exceedingly frequent,—Dr Bennett had met with them so often in the post-mortem examinations conducted at the Royal Infirmary, that he had stated, in a communication read several years ago before this society, that evidences of tubercular disease were found in one-third or one-fourth of all who die after the age of forty.

Dr Christison remarked, that although it had been satisfactorily shown that tubercle often underwent spontaneous cure, yet authentic examples of recovery from *confirmed* phthisis pulmonalis were exceedingly rare. The case which had furnished the preparation first exhibited by Dr Bennett was peculiarly interesting ; it was probably an instance of recovery after the formation of cavities and development of the phthisical cachexia.

EXOSTOSIS OF THE FEMUR.

Mr Syme exhibited an exostosis which he had recently removed from a patient in the Infirmary. The most common seat of these tumours was the distal phalanx of the great toe ; they were also frequently found upon the *linea aspera* of the femur above the inner condyle. It was from this latter situation that the tumour had been removed. In size it equalled a small orange ; it was nearly spherical in form ; it consisted of bony tissue, and was coated on its outer surface with cartilage. It was not right to interfere with these tumours in general ; they were not malignant, and seldom caused much inconvenience ; when, however, they attained such a size as that now exhibited, it was obviously expedient to remove them. There was no difficulty in performing the operation ; it was easy to detach the tumour from the femur by means of the *cutting-pliers*, and the only bad consequence to be apprehended was tedious suppuration. In the present instance, notwithstanding the unusual size of the mass removed, the wound had healed readily.

DISLOCATION OF THE HUMERUS, WITH FRACTURE.

Mr Syme had lately met with a case of dislocation of the head of the humerus into the axilla, complicated with fracture about the middle of the humerus. This double accident was exceedingly rare. The patient had fallen through a trap door into a cellar, and his arm had become entangled among the spokes of a ladder during the fall. It was determined that the dislocation should be reduced, before an attempt was made to set the fractured bones. For this purpose the forearm was bent upon the humerus, and a bandage was then rolled round the whole arm as high as the shoulder. Over this a couple of Gooch's splints were placed ; the lac was then attached above the seat of fracture, and secured by another bandage. Extension was made in the direction of the long axis of the trunk, a large pad being first stuffed into the axilla. Reduction was readily effected, and the subsequent progress of the case had been satisfactory.

DISLOCATION OF THE ASTRAGALUS.

Mr Syme had at present under his care a case of dislocation of the astragalus. The accident had occurred three months ago in the usual way. There was no

external wound, but the astragalus could be distinctly felt under the integuments of the ankle. Although there was considerable deformity of the foot, the limb was like to be useful, and there was at present no necessity for cutting out the astragalus, as Dupuytren recommended in all such cases.

FEMORAL HERNIA.

Mr Syme gave some account of a case of femoral hernia, on which he had recently been called to operate. Symptoms of strangulation had existed for thirty-eight hours. A femoral sac was opened, and some clear fluid escaped. The sac contained no intestine, but its neck was occupied by a small portion of omentum which adhered to the surrounding parts. Enlargement of the external aperture did not relieve the parts; the stricture was accordingly divided as usual at the neck of the sac. Symptoms of strangulation continued next day. The wound was therefore re-opened, and a careful examination made, lest some secondary sac might have escaped detection at the time of the operation. On lifting the portion of omentum, a small sac was discovered; when it was opened, some dark reddish fluid was evacuated, and a knuckle of intestine seen. The stricture was divided, the bowel returned to the abdomen, and the patient ultimately recovered.

CASE OF HYDROPHOBIA.

Dr Christison read a communication from Dr Lucas of Dalkeith, describing a case of hydrophobia which had occurred there. [The case will be published at length in a future Number.]

The most interesting points in the case were thus summed up by Dr Lucas:—1st, That eight weeks elapsed between the infliction of the bite and the invasion of hydrophobia. 2d, The wound healed, and did not subsequently become irritated. 3d, With the exception of spasm of the respiratory muscles, no muscular effort was observed independent of volition. 4th, The physiological manifestations consisted in an intense exaltation of sensibility of the cutaneous surface, and of the reflex system of nerves. 5th, The psychological phenomena were a highly developed antagonism between the morbid emotional and normal reflex faculties. 6th, Death took place by exhaustion consequent upon the direct and sympathetic excitement of the nervous system.

Dr W. T. Gairdner had performed the post-mortem examination, and gave a verbal account of the appearances observed. There was some ecchymosis on the surface of the pleura, likewise in the mucous membrane of the pharynx, and among the muscles of the neck. The right side of the heart contained a small quantity of frothy blood, and a large amount of air; and there was no evidence to show that this appearance was attributable to putrefaction. Dr G. had seen a similar collection of air in the heart of a patient who had been poisoned by carbonic acid gas. The nervous system had been most carefully examined, especially the origins and course of the eighth pair, but no evidence of inflammation or structural lesion had been detected.

Professor Dick, of the Veterinary College, had long considered the hydrophobia of the human subject a disease of the imagination. It was generally found, upon inquiry, that the patient, after being bitten, became exceedingly apprehensive as to the probable consequences. A policeman, who died in the Royal Infirmary, twelve years ago, had, it was said, neither cared for the bite nor feared its results. Professor D. had himself made careful inquiry regarding his case, and found that the man had been very much alarmed, and had been constantly taking medicine by way of antidote. In the Dalkeith case, although the patient had made no allusion to the bite, he thought it not improbable that imagination was at work. The patient seemed to endeavour to deceive himself, to banish the fearful idea from his mind; yet, though he dared not himself suggest the nature of his disease, he made continual inquiries of those who stood about him, and manifested concern for his sufferings. It was hardly possible that his mother could conceal the truth from him, or that the boy could

himself be ignorant of it. Upon this subject Professor D. proposed to satisfy himself by private inquiries. If, as he believed, the disease was really one of the imagination, he need not point out how useless or how mischievous many of the vaunted remedies must be. With the disease of the lower animals, termed rabies, he was of course familar. Twelve years ago it was very prevalent in Edinburgh ; he had then seen 120 cases in fourteen months. From that time till recently he had scarcely seen a case at all ; but within the last six weeks he had seen a rabid dog in Glasgow ; and some rabid cattle in a Lanarkshire dairy. He believed it not improbable that the disease might soon prevail extensively. It was remarkable, and constituted an argument against the communicability of the disease from the dog to the human subject, that there was no fear of water manifested by rabid animals. Besides, the rabies of dogs was sometimes an epidemic ; dogs contracted it without being bitten, and there was not sufficient evidence that a bite ever produced it. The morbid appearances in the dog were ecchymosis about the upper pharynx, and in the nasal cavity, with vascularity extending over the schneiderian membrane, and over larynx, pharynx, trachea, and œsophagus.

Dr Bennett thought that the symptoms of hydrophobia clearly pointed out a lesion of the functions presided over by the eighth pair of nerves. It was true, that structural lesion had not been observed in this case, either at the origin or in the course of the eigthth pair; it must, however, be recollected, that in these excito-motory diseases, structural lesion was not essential. He quite believed that symptoms like hydrophobia might be induced by fright; a recorded case had proved that a bitten man might even die while the dog that bit him lived. There was however no doubt, that the disease termed hydrophobia could be communicated from animals to man, and Magendie had caused a dog to die rabid, by inoculating it with saliva from a human subject suffering from hydrophobia.

Dr Gairdner alluded to the experiments of Dr Hartwig of Berlin, as tolerably conclusive evidence, that rabies could be communicated, at least among the lower animals, by inoculation.

Professor Dick attached little importance to Hartwig's experiments, because although he had laboured at the subject for five years, his inoculations only took effect in very few cases. In similar experiments made in Edinburgh, Professor D. had been uniformly unsuccessful. So convinced was he that the disease was not to be propagated in this way, that he took no extraordinary precautions in handling rabid animals, and had once allowed the saliva of a rabid dog to rest upon a sore upon his hand without any unpleasant consequences.

Dr Christison had formed a very different estimate of the value of Hartwig's labours, which he thought set this controverted subject for ever at rest. Hartwig had in five years made fifty experiments, upon the inoculation of canine rabies, using in different experiments different fluids, such as the blood, mucus, and saliva of the rabid animal. In fourteen of these inoculations he had transmitted the disease from one animal to another, and had succeeded more uniformly with the saliva than with the other fluids employed. It was true that he had failed in some instances, but such failures did not invalidate the results obtained in about one-fourth of his experiments ; as well might it be argued, that because vaccination sometimes failed, it was therefore impossible to communicate the vaccine virus from child to child. Dr C. had, many years ago, in Paris, witnessed a series of experiments on the inoculation of rabies. They were performed at the Combat des Animaux, by some hospital "internes" under the direction of some able physiologists. It was found that when blood was used, the inoculation did not take effect ; by far the most effectual medium was the "barve," the peculiar froth which collects about the mouth of the rabid dog. By using this fluid, the French experimenters managed to induce the disease in several successive experiments, and in fact to keep up a *stock* of rabid animals for several months. It was stated by Hartwig

that dogs were subject to other diseases resembling rabies; but there was one symptom of the true disease which was never observed in its spurious forms. He alluded to the changed character of the animal's bark—a peculiar, checked, short, sharp, anxious howl. Hartwig, who, as head of the Veterinary College at Berlin, had immense opportunities of observation, laid great stress upon the importance of this symptom; and in the Parisian experiments, the peculiar bark of the rabid dog had been remarked both by the late Dr Cullen and himself. He suggested to Professor Dick, that in some of the experiments performed in Edinburgh, and in which negative results were obtained, the animals might possibly have been affected with some of the diseases simulating rabies.

Professor Dick proposed to repeat his experiments when opportunity presented itself, and would feel glad that they should be witnessed by members of the Society. He well knew the bark of the rabid dog described by Professor Christison, and whenever he heard it, he could at once pronounce that dog to be rabid, even without seeing it.

The President had, twelve years ago, occasion to treat the only case of hydrophobia which had been seen in the Royal Infirmary for forty years. The patient was the policeman to whose case Professor Dick had alluded. A very prominent symptom was the ejection of saliva by hawking or spitting, and he presumed that the "pretty copious salivation" described by Dr Lucas, in the case of the boy at Dalkeith, was of the same description.

THE PROPOSED PUBLIC HEALTH BILL FOR SCOTLAND.

Much discussion has taken place lately in Edinburgh, and in other parts of Scotland also, in regard to the "Public Health (Scotland) Bill" now before the Legislature, which is opposed on various grounds. The two Royal Colleges, the Town-Council, and the Police Commission, have prepared excellent reports, and presented petitions in favour of the measure and against certain clauses of it; and we are sorry to observe that a considerable number of petitions have proceeded from various parts of Scotland, which have for their object to prevent the passing of any such bill, even in an amended and modified form.

The general object of the bill may be briefly described to be, to provide machinery for combining authoritatively the purses and the influence of all in effecting certain hygienic changes in which all have a common interest. These changes chiefly relate to the substitution of sewerage for surface drainage in narrow and confined places—to the removal of pools of stagnant water and of accumulations of filth from the neighbourhood of human dwellings—to the observance of certain hygienic conditions as to the construction of dwelling-houses, and the width of streets, courts, and alleys—to the erection of slaughter-houses—to the sale of unwholesome meat—to smoke and other noxious vapours—to the conditions of interment in populous cities, and to other matters which, like these, are at all times very important to the health of the community, but have of late years acquired more importance, in the estimation of all reflecting persons, in consequence of the extensive prevalence of epidemic pestilences among us, which have penetrated from the recesses of poverty and misery into the chambers of opulence and of comfort, burdened our poor's rates with permanent assessments for widows and orphans; and thus eloquently proclaimed the heartlessness, folly, and short-sightedness of the vulgar parsimony which refuses the means of protecting the people of this country against calamities so appalling.

By referring to our account of the English Act (No. for Nov. 1848, p. 316), which is similar in its machinery and in its objects, it will be seen that we approve strongly of its principle: and, believing the medical profession to be quite unanimous on this subject, we are satisfied that we should ill discharge

our duty as expositors of its opinions if we did not give our hearty support to a measure so beneficial and so greatly needed.

But while we thus express unreservedly our strong sense of the value of the benefit sought to be conferred on Scotland, we think it equally our duty to declare that some of the provisions of the bill require to be amended; and, in saying so, we more especially allude to the clause by which it is proposed to place the working of it under the superintendence of the board which was constituted last year under the English act. Were the purposes of that act merely to carry out a system of mechanical arrangements for draining, and other matters of that description, the board might be held to be constituted in a very satisfactory way for the southern part of the kingdom. But these gentlemen have thought it their duty to undertake other functions, such as that of offering advice to the public on the subject of the contagiousness of epidemics, and the mode of arresting them, which they are ill fitted to discharge; inasmuch as their single medical associate cannot in any sense be held to be the representative of the opinions of professional men. The effect of this arrangement is, that the board is at this moment at war with almost the entire medical press of England. It must be evident that the opinions of a lay board on subjects strictly professional are of no value unless they take a reasonable degree of trouble to ascertain that these opinions are in accordance with the deliberate judgment of the best informed and most able men among us. This the board has not only not done, but it has thrown itself for medical counsel on an individual responsible only to itself, and has thus been induced to dogmatise on subjects of which it is necessarily ignorant, with a confidence which, without such aid, it would never have ventured to assume.

We have no wish that such a board should be exclusively medical; indeed, we are not clear that there are any of its proper functions which could not be performed by non-medical persons with the aid of occasional medical advice. But we are quite decidedly of opinion, that, whenever they require the aid of professional men, those whom they may consult, whether having a seat at the board or not, ought to be the representatives of the medical profession, and responsible to that profession for the advices which they give.

But we also think that a board for Scotland ought to be connected with that part of the country. The laws, institutions, localities, manners, usages, and even language of the northern division of the kingdom, are very imperfectly understood in London, and we are therefore clear that the powers of the act cannot be so usefully wielded by a London board as by a Scotch one.

If proof were needed of the injury inflicted on Scotland by the supervision of the English Board in these matters, we need only refer to what took place on the late outbreak of cholera in Edinburgh. A young physician, of whom individually we have no wish to say ought disparaging, was unfortunately placed in a false position as the representative of the London Board, among seniors in his profession, much better qualified to communicate than to receive instruction. The result was, as might have been expected, general dissatisfaction and the retardation of measures for staying the disease.

There were other objectionable clauses in the bill which struck us in perusing it. But we are happy to find that many of these have already been amended; and as none of them are of nearly the importance of that which we have noticed, we must refer to the reports of the public bodies already mentioned for all details. The public are indebted for their reports to some of the most eminent members of the medical profession in Edinburgh, and the ability which pervades them is at once a proof of the importance which these gentlemen attach to the subject, and of the confusion which would certainly be the result of adopting a course opposed to the judgment of such men, enjoying, as they do, the unlimited confidence of the people of Scotland.

Since the above was written, we have been given to understand that the Public Health (Scotland) Bill has passed through the Select Committee of the House of Commons. We have not yet seen the amended bill, but we are given to

understand that it embodies very many of the alterations suggested by the Police Commissioners and Royal College of Physicians of Edinburgh. The obnoxious clause constituting the English Board, the General Board of Health for Scotland, still remains. It has, however, been rendered a little less objectionable, by restricting its duties to the mere institution of, and deciding on, the preliminary inquiries, and withdrawing from it that surveillance over the Local Boards contemplated in the original bill.

REPORTS ON CHOLERA IN SCOTLAND, 1848-9.

DUMFRIES.—*From Dr Barker, Physician to the Dumfries and Galloway Royal Infirmary.*—Dumfries is situated, as it were, in the centre of a large cup, being completely surrounded by tolerably high hills in every direction, with a gap in their continuity at the north for the entrance of the river Nith into the valley, and another at the south for its exit. About two miles, at its nearest point to the south-east, lies the extensive morass of *Lochar Moss*, about ten miles in length, and averaging between two and three miles in breadth. During the last two years there has been a great accession of Irish labourers, employed in the formation of the Nithsdale railway, by which the aggregate condition of the town has been rendered worse and more squalid. The principal streets of the town are clean, but the back lanes and closes are very much the reverse, and ill ventilated. These remarks apply equally to both sides of the river, to Maxwelltown as well as Dumfries. There is no water in the town except that which is carried up from the river in water carts, rain water collected in cisterns, or the water obtained from wells. The drainage may be stated as equally deficient; although several large drains have been made under the principal streets, they do not extend to the back streets and closes, the places particularly requiring them. Besides, they are made of such immense dimensions, that it is perfectly impossible for them to be properly flushed.

All the time that cholera was in Dumfries, viz., from the 16th of November 1848 till about the 16th of January 1849, the atmospheric phenomena were very remarkable, the weather being exceedingly tempestuous. The river rose on one occasion higher than it had been seen for sixty or seventy years. The aurora borealis was several times very vivid in the months of October and November. There were two severe thunder storms, the one in the beginning and the other at the end of December. From the 16th of November 1848 till the 15th of January 1849, inclusive, the mean height of the barometer, at 9 A.M., was 29·79 inches; at 9 P.M., 29·78 inches; with a mean daily range of 0·32 inches. However, on two successive days, about the time cholera was leaving us, viz., on the 10th and 11th of January, the daily range was 1·08 in. and 1·11 in. respectively. The mean temperature at 9¼ A.M., was 39·54° ; and at 8½ P.M., 40·73°. The number of days on which rain or snow fell was 39. During the first half of the period, the prevalent direction of the wind ranged from south-west to north-west; and during the latter, from south-east to north-east. The wind was—*calm*, 1 day; *moderate*, 22½ days ; *brisk*, 13 days; *strong*, 16½ days; *boisterous*, 9 days. During the epidemic of 1832 the aurora borealis was, as in this one, very vivid, but the weather was considerably different, being constantly moist, calm, and cloudy. The epidemic was terminated by a thunder-storm. (See "The Leading Phenomena of Epidemic Cholera." By Ambrose Blacklock, Madras Medical Board. Madras, 1848.)

Fever was very prevalent, and dysentery may almost be said to *have been epidemic* for at least two months before the accession of cholera; but when the latter made its appearance, we had no more fever—even in the infirmary we had only one case—and dysentery was replaced by diarrhœa and cholera.

Of the patients admitted into the infirmary, speaking generally, the most of them came from the streets and closes which had previously furnished us with fever cases. Nearly one-half of them (eighty in all), were dissipated, or, at

least, questionable in their habits; and most of them were ill-fed and exposed to the weather. In my private practice, out of forty-cases, only five were of dissipated or questionable habits.

My time of acting-physician at the infirmary being from the 1st of September till the 1st of January, all the cases were under my charge except six, who were admitted subsequently to the latter date. I therefore feel called upon to correct the statements made in your "Journal" of this month (May) regarding the cholera in Dumfries. The greatest care was taken to keep the cholera patients separate from the others. As there was only one case of fever in the house, the four fever wards were set apart for the reception of cholera cases, and these wards occupy the whole of the upper storey of the house: the solitary fever patient remained in one of them, but that could not be helped. When these upper wards were full, a large ward in the extreme wing was used; but all the other patients were removed from it to other wards, or dismissed. One of the surgical patients acted as night-nurse for the cholera wards, for twelve or fourteen nights, when we had difficulty in procuring nurses. Another man presented himself in the waiting room as an out-patient, but was not ailing much; when, as he was not afraid of the disease, I engaged him to come into the house as a nurse, which he did for payment, and acted as such so long as we had cholera patients. We had thus, during its continuance, five nurses for the cholera wards solely, one of whom died almost at the commencement of the epidemic of cholera, after twenty-three hours' illness, which she could scarcely have caught, as reported, by going up into the town, as there were, at that time, almost no cases of cholera in the town, except in St Michael's Street, and its immediate vicinity, at the southern end of which the infirmary stands. At the time of her seizure, this was the principal nurse for the upper wards, and was, of course, the one who had most to do with the cholera cases. Of the officers of the infirmary, Mr M'Lauchlan, the acting surgeon, died on the 10th of December, as was reported to the Board of Health. As his time for attending the infirmary was the same as mine, and as we met there every day, I asked him to go up to the wards with me whenever he could spare as much time, which he frequently did, as he was much interested in the history of the disease; and he went round the wards with me within a week of his death. Thus we had one death out of *nine* resident medical men; none of the others had cholera, I believe, but more than the half of them were confined to the house, for some days at least, with the premonitory diarrhœa.[1] Out of twelve medical men who were brought to Dumfries during the visitation, two were attacked very severely, of whom one died. Whilst I am perfectly sure that cholera spread in Dumfries as an epidemic, I am by no means sure that it did not spread also by contagion, more particularly when a number of cases were congregated together, without a due amount of proper ventilation. It may not be out of place to mention here, the appearance of cholera at Clarencefield, although I cannot, at the moment, find a paper to give me the correct dates. Clarencefield is a small village in the parish of Ruthwell, nine-and-a-half miles south-east from Dumfries, between which places a postman went regularly every second day. Having been in Dumfries, in the beginning of December, this postman was seized with cholera after reaching home, and died. The medical man of the parish, who attended him, and who had not been near Dumfries, was next seized and died. A female, who waited upon one or both of them, was also seized, but recovered. And an elderly lady, who had been near none of these three, was also seized with cholera, and recovered: however, a relative of hers, a medical gentleman returned from India, was living in the house with her, and he had been frequently visiting both the postman and the medical man during their illness. In this instance there were no other cases, so that it would be difficult to consider the above an epidemic visitation.

[We have been favoured with a letter by Dr M'Cheyne of Clarencefield, in

[1] Two of the resident medical men died in Dumfries, in 1832.

which an additional case is mentioned ; that of a blacksmith, who had been in Dumfries, and had attended the funeral of a person who died of cholera. This was the first case in Clarencefield.—*Edit. M. J.*]

In the epidemic of 1832, the total number of cases reported in Dumfries, as given in the minutes of the Board of Health in 1832, was 837 ; and the total number of deaths, 421. The total number of cases reported in Maxwelltown was 237, and the total number of deaths, 127. The epidemic of 1848-9 was much milder, at least as regards numbers, than the former one, there being, as nearly as can be calculated, more than 240 cases, and 100 deaths, fewer in Dumfries ; and twenty-three cases and thirteen deaths fewer in Maxwelltown. In this epidemic a considerable number of the cases and deaths were amongst children, who, in the former one, enjoyed an almost perfect immunity.

The chief localities of the disease in Dumfries in 1832 were Queensberry Street, and St Michael's Street, inclusive of Burns' Street and the east side of Nith Place ; to which, for 1848-9, Irish Street may be added, in which there was a good number of deaths amongst persons of the upper classes. The chief localities in Maxwelltown, in 1832, were Church Street, College Street, Glasgow Street, Market Street, and Old Bridge Street. During the epidemic of 1848-9 the worst localities were much the same, with the exception of College Street, which was not so bad as some of the other streets ; and this is to be wondered at, as it is very narrow and dirty, and contains a great majority of the poorest inhabitants ; but it has the benefit of being very near the river in its entire length, and at a considerable elevation above it.

THE LATE DR WILLIAM BEILBY.

This gentleman, who has long occupied a prominent position among the medical practitioners of Edinburgh, has just been removed from this world. He was universally respected as a man of the greatest worth, integrity, and benevolence, and of the most unostentatious disposition of mind. He entered on the study of medicine later than usual, having been engaged in commercial pursuits during the earlier part of his life. He was a pupil of the Edinburgh School of Medicine, and took his degree from the University of Edinburgh in 1816. About this time he was intimate with the late Dr Abercrombie, with whom he continued ever after on terms of friendship ; and whose friendship was, of course, of great advantage to him. He devoted himself to the obstetrical department of his profession, and enjoyed a large share of public favour. Of the estimation in which he was held by his profession, the best proof, perhaps, which can be given, is the fact, that he was elevated by their suffrages to the chair of the Royal College of Physicians of Edinburgh, which he occupied at the time of the opening of their handsome new hall (27th November 1846). On that occasion he delivered an address, which was chiefly occupied with a sketch of the history of the College. Of that address we gave a pretty full account at the time (Medical Journal for January 1847, p. 552), to which we refer our readers.

NOTICE TO SUBSCRIBERS.

That the volumes of the Monthly Journal may coincide with those of the Retrospect, beginning in January and ending in December, it has been determined that the present volume shall run to December 1849. The usual Index, together with that of the Retrospect, will accordingly be given in December.

The Papers of Drs Mackenzie and Peacock have been received.

In consequence of a slip in the printing at the foot of p. 848 of our last number, we give corrected leaves at the end of this, to be substituted in binding.

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*On Stomach and Nervous Disorder, as connected with the Oxalic Diathesis.*[1] By James Begbie, M.D., F.R.S.E., Fellow of the Royal College of Physicians of Edinburgh.

(*Read to the Medico-Chirurgical Society of Edinburgh, June* 6, 1849.)

There is a class of patients,—a numerous class it is,—composed chiefly of those in the prime of life, and including more males than females. They are the subjects generally of the sanguine or melancholic temperament; men unaccustomed to laborious or active exertion, usually belonging to the upper classes, and accustomed to indulge in the good things, particularly the *sweets* of the table. They present themselves as sufferers from indigestion, from its mildest to some of its most aggravated forms; often with no disorder apparently, but the uneasiness arising from imperfect digestion, and languid assimilation, a sense of load at the pit of the stomach, with flatulence and palpitation some hours after a meal. Oftener, however, they have sufferings of a more severe kind, referable not to the digestive organs alone, but influencing very seriously the nervous system, and threatening the mental condition of the patient. They are usually peevish, sensitive,

[1] It is not my intention to speak now of *Oxaluria* in general. I am aware that the oxalate of lime is found in the urine, under circumstances apparently very different from those here described. That it is found in all ages, and in both sexes, and in different conditions of life. That it is found as the immediate exciting cause of a fit of gravel, where no mal-assimilation has prominently manifested itself; and that the records of public institutions for the management of the insane show, that it is very common in the urine of those suffering under melancholia. I limit my remarks at present, to a class of cases, of a well-marked character, in which its presence is a prominent feature, and from which important indications of diagnosis and treatment may be drawn.

and irritable, or dull, desponding, and melancholic. At times full of gloomy fears and dark forebodings, they are painfully distrustful of their own powers; suspicious of some lurking disease,—especially pointing to consumption or disease of the heart, as the source of their apprehensions; and from the dread of some threatened evil, they are not unfrequently hurried to the brink of mental derangement. In the milder forms, there is in these patients the anxious countenance and the look of ill health, a loaded tongue, a dry skin, and an irritable pulse. In the more confirmed, a dingy dark complexion, perhaps a red and tender tongue, with growing emaciation, falling off of the hair, eruptions of the scaly kind, sometimes of boils or carbuncles, pain of a dull but deep-seated kind in the back and loins, hemorrhage from the bowels and bladder, incontinence of urine, and complete prostration of the virile powers[1]. The progress of these evils may be varied and slow; under diet and regimen with pure country air, it may be checked and controlled; under medicine it may be entirely arrested;—but if neglected or maltreated, the malady will assuredly subject its victim to the sufferings and perils of stone in the kidney or bladder, or the still more serious consequences of malignant organic disease.

Such are the miseries that flow from the oxalic diathesis;—from a poison generated during the process of digestion and assimilation, carried into the blood through the ordinary channels, but limited in its pernicious consequences by the busy agency of the urinary organs, in separating it from the circulation, and discharging it from the system. By this process of elimination, we are enabled to detect the offending matter in the urine, in the form of the oxalate of lime, by tests at once simple and satisfactory, and are thus prepared with a plan of treatment, equally simple and successful in mitigating the pains and miseries of the patient, and in gradually subduing and ultimately removing the diathesis, which has been their lasting source.

These views have been well illustrated in the writings of Prout, Golding Bird, and others; and every day's experience confirms their truth and their importance. But it is still to be regretted that the careful examination of the habitual qualities of the urine, with the view of detecting those morbid conditions of the secretion, which originate in imperfect digestion, and defective assimilation, is so much neglected by those who seek to palliate or cure the sufferings of the dyspeptic, or the miseries of the hypochondriac; and that the victims of these sad and painful maladies, are too often left to run the endless, hurtful, and cruel round of routine practice; to experience, perhaps, a mitigation of one set of symptoms, at the expense of an aggravation of another; to exhaust the list of remedies, and the list of advisers; and to fall into the hands of the ignorant or designing

[1] These latter symptoms I have seen in several instances accompanied by feebleness and loss of power of the lower limbs, such as to lead to the suspicion of permanent paraplegia.

empiric, to the injury of themselves, and the disparagement of the profession.

With the view of lessening these evils, and recommending a still more careful examination of the urine in dyspepsia, and thus averting the many serious lesions which result from its continuance, I venture to submit the following histories to the consideration of the Society.

I. A clergyman, aged 36, of sanguine temperament, and spare habit, studious and devoted to his profession, which he had exercised for many years in North America, applied to me at the close of October 1845, for the relief of symptoms of stomach affection, under which he had laboured for many years. He was highly dyspeptic and nervous, and in his adopted country had been treated actively for "bile" and "liver," and had undergone, by different hands, a variety of prescriptions, from the mild anti-bilious pill to the large and salivating doses of mercury, with only one effect,—the aggravation of his sufferings. After long trial, and many disappointments, he was forced to relinquish his calling, and return to Scotland, in shattered health, and broken spirits. On his arrival, at the date referred to, he was pale and emaciated, and indications of mental and bodily suffering were discernible in his countenance and manner. His appetite was depraved, and he ate largely. He suffered from flatulent distension, and irregular beating of the heart; from langour and lowness of spirits; from restless nights, and troubled dreams; from unrefreshing sleep, and anxious awakings. He was incapable of mental application, and had well-nigh abandoned the hope of restoration to his duties, and to health. His skin was dry and parched; his tongue loaded; his bowels irregular, and the dejections unhealthy. His urine, which hitherto had not been examined, was of a deep amber colour,[1] and perfectly transparent, possessing a peculiarly rich smell, and a sp. gr. of 1·028. It was of acid reaction, slightly phosphatic, and contained urea in excess. Under the microscope, which alone can determine satisfactorily the presence of the oxalate of lime, a large number of the characteristic crystals of that deposit were discovered, and the treatment pursued was in accordance with the principles and practice maintained by Dr Prout, in the management of that diathesis. The bowels were cleared by repeated doses of gentle laxatives; the diet was regulated so as to exclude, as much as possible, all saccharine materials,—and milk, farinaceous vegetables, and animal food in small quantity, enjoined. Tepid sponging and warm clothing were also recommended, and the nitro-muriatic acid in doses of twenty drops twice or thrice a-day, prescribed.[2] On the 25th of November, nearly four weeks from the commencement of this mode of treatment, the urine was again examined and found of sp. gr. 1·024, while it maintained the former characters, both under the eye, in glass vessels, and under the microscope. The general appearance of the patient, however, was somewhat improved, and the dyspeptic and nervous symptoms were less distressing. A steady perseverance in the same mode of treatment was enjoined. On the 18th December, the urine was of a straw colour;

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[1] For a correct account of the characters of the urine containing the oxalate of lime, see a paper by my son, Dr J. W. Begbie, in the "Monthly Journal" for March 1847.

[2] I have generally adhered to the following formula, omitting occasionally the syrup and increasing the water.

℞ Acid. Nitric. Dil.
—— Muriatic. Dil.
Syrup. Aurant ā ʒss.
Aquæ ʒiss. Misce.

Sig.—A tea-spoonful, mixed in a wine-glassful of water, before meals.

of acid reaction, of sp. gr. 1·024; unaltered by heat or nitric acid, and presenting under the microscope fewer crystals of the oxalate of lime, together with some of those of lithic acid. The symptoms in general were mitigated; but as the patient complained of gastralgia, and the urine was more decidedly acid, the former prescription was intermitted, and ten grains of the white bismuth ordered to be taken three times a-day; and the same attention to diet, regimen, bathing, and clothing, as heretofore, continued. In the beginning of January the nitro-muriatic acid was resumed. On the 31st the urine brought for examination contained a considerable deposit of the lithate of ammonia, and readily cleared on the application of heat. It was still of high specific gravity, 1·025, and of acid reaction; while under the microscope the crystals of the oxalate of lime were fewer in number, and less in size. The nitro-muriatic acid was again suspended. Digestion being now performed with less uneasiness, and the general appearance of the patient improved, while his nervous symptoms were much abated, he was allowed, at his own request, to occupy the pulpit of a brother minister from time to time; and finding that he could satisfactorily discharge such duties, his spirits and health correspondingly advanced. He was ordered to use, alternate months, a vegetable bitter infusion, with the addition of the carbonate of potass, when the lithic acid was in excess, and the nitro-muriatic acid as formerly. Under this plan he gradually recovered flesh and strength, and presented, both in body and mind, the indications of renovated health. The improvement kept pace with the progressive diminution of the oxalate of lime in the urine, and when the peculiar diathesis appeared to be subdued, which happened within a twelvemonth from the commencement of treatment, he was able to resume his ministerial duties, and has for some time been settled in the west of Scotland, the pastor of a numerous flock, and the comforter and adviser of a populous parish.

II. A gentleman, aged 35, attached to no profession, accustomed to regular exercise, and rather luxurious in his habits; of sanguine temperament and strumous habit; about the beginning of 1844, according to his own account, began to complain of indigestion, characterised by loss of appetite and strength, by frequent perspirations over the forehead, on making slight muscular exertions, and by increasing emaciation. A papular eruption appeared on the chest and between the shoulders, and continued more or less for eighteen months. This was accompanied by an uneasy sense of weight at the pit of the stomach, loaded tongue, restless unrefreshing sleep, and great anxiety of mind. At this time he left Edinburgh, with the view of a permanent residence in the north of England, and in the hope of the enjoyment of better health. He was however disappointed, and after a trial of five months of the climate and practitioners of the south, he returned to his former home, depressed in mind and wasted in body. I saw him towards the end of October 1845, a miserable dyspeptic, and hypochondriac; suffering from headache, flatulence, and palpitation; from constant restlessness of mind, and painful anxiety regarding the future; ever desirous of change, but never attaining the desired haven. He was particularly suspicious of the presence of consumption, or disease of the heart. His tongue was furred; his bowels constipated; the evacuations discoloured, and frequently mixed with blood. He complained of pain in the region of the liver, and of numbness of the right arm. His skin was dry and parched, his eyes suffused and painful, and his hair falling out. He complained of a "lanky feeling" about the loins and abdomen, of uneasiness in the region of the bladder, and of the loss of all sexual desire. His urine, at this time, was of high amber colour, and of sp. gr. 1·040; of acid reaction, and with excess of urea. Under the microscope there were large numbers of the octohedral crystals of the oxalate of lime and much epithelial matter. He was ordered repeated doses of castor oil, a simple diet of farinaceous food, warm clothing, tepid sponging of the skin, and the nitro-muriatic acid. On the 25th of November, the urine was of sp. gr. 1·036, and

still loaded with numerous crystals of the oxalate of lime. The general symptoms had undergone little change. On the 18th December, the urine was loaded with the lithate of ammonia, and the crystals of the oxalate of lime were less numerous, the sp. gr. was 1·030; the dyspeptic and nervous symptoms were less severe. He was ordered to intermit the acids, and to use an infusion of snake root, with small doses of the carbonate of potass. On the 8th of January 1846, the crystalline deposit, as seen under the microscope, was still undiminished, and was mixed with a few crystals of lithic acid. There was little amendment in the general symptoms. The snake root was intermitted, and the nitro-muriatic acid resumed. On the 22d February the oxalate of lime had considerably diminished, and the lithic acid increased, the bowels were more easily managed, the tongue had improved, and the dyspeptic and nervous symptoms were less urgent. He was ordered to alternate monthly, the use of the acids, and the vegetable infusion. On the 5th April the oxalate was much diminished, and the lithate of ammonia seen in large quantity. He was ordered to persevere, and on the occurrence of warm weather to seek the country, and country sports. On the 3d of August the sp. gr. of the urine was 1·026, it was of straw colour, and contained numerous crystals of lithic acid, and a few only of oxalate of lime. The acid was intermitted for two months. On the 12th November the lithic acid had disappeared, but the octohedral crystals were present in considerable numbers and of large size. The acid was resumed. During November and December the oxalate diminished in number and size, the patient gradually improved in looks and spirits; he regained his lost powers; his skin became soft and smooth; the blood disappeared from his stools; his manner and conduct indicated that he no longer thought *only* of himself and his ailments, and in the course of a few more months, under the cod liver oil, he regained his lost flesh, and acquired new health. He has long since ceased his attendance on me, but I have frequent opportunities of observing his studious observance of those rules of diet, regimen, and exercise, which I hope will exempt him from the perils and sufferings of the oxalic diathesis.

III. A gentleman, aged 45, educated for the bar, but inheriting a patrimonial estate, to the management of which he was more devoted than to professional pursuits; inheriting also a predisposition to gout and cerebral disease, accustomed to a full table, and not indisposed to avail himself of his opportunities—particularly, it had been observed, in the matter of sweets;—had been a patient sufferer from indigestion during a series of years, in which he contrived, by a temporary abstinence, and the frequent use of Gregory's mixture, or some such household remedy, to allay the more pressing symptoms of his malady. He began, however, to be subject to boils, and to ailments of a gouty-rheumatic character, and became the subject of professional discipline. After trial of many remedies for an obstinate attack of a rheumatic kind, affecting chiefly the loins and lower extremities, and involving (according to the opinion of those to whom we are accustomed to submit with deference) the coverings of the spinal marrow, he was cured by the powerful agency of arsenic, and for some time continued in fair health. In the beginning of 1846, however, having relaxed the rules of diet and regimen prescribed as essential for him, he experienced a return of his dyspeptic ailments, accompanied with much depression of mind, and anxious and fearful prospects of the future; he lost looks and flesh; his clothes, to use a common expression, "hung upon him;" he shunned company, and regarded himself as "a victim." His tongue was foul; his pulse irritable; his bowels irregular. He was still a gross eater, and suffered from flatulent distention, giddiness and palpitation. He became again the subject of boils and carbuncles, affecting chiefly the trunk and thighs; these were tedious in suppurating, and slow to heal. He complained of uneasiness in the region of the bladder, which frequently lost its power of retention; and of pain and heaviness across the back and loins. His urine presented the characters of oxaluria; it was of high amber colour, and without sediment, of sp. gr. 1·030,

and disclosed, under the microscope, numerous crystals of the octohedral form. He was placed under a diet suitable to his diathesis, and a persistent use of the nitro-muriatic acid was enjoined; a tepid salt water shower bath and warm clothing were added as powerful auxiliaries. Under this mode of treatment he made rapid progress towards recovery; his boils—long languid—began to heal; his skin—long torpid—became soft and perspirable; his tongue cleaned; his pulse improved; his appetite—long restrained—returned to rules of health; he lost his dull despondency; he lost his irritability of bladder; he lost the oxalate of lime in the urine; he acquired the look of returning health; he acquired fresh spirits; and, before many months were over, he acquired, under the use of that valuable remedy—the cod-liver oil—both flesh and strength, which he preserves uninterruptedly to this day.

It were easy to multiply cases of this description, the record of which I have preserved; but it may be sufficient, in the further illustration of the subject, to limit the attention of the Society to the history of one other, in which the progress and termination were sadly different.

IV. Some twenty-five years ago, I formed the acquaintance, and became the professional adviser, of Mr D—, a practitioner of law, of a few years standing. He was a young man of superior talent, of persevering industry, and studiously devoted to his profession, in which he was destined to take a high place; but he was early in life the subject of dyspeptic derangement, and from this cause, and the irritability of temper, which so often accompanies it, his path was crossed and his prospects somewhat clouded. His energy of character and constitution of mind enabled him to surmount many difficulties, and he was still found contending against uncertain health and desponding spirits. During many years he was subjected to the discipline considered essential to the cure of indigestion and its consequences, and to those regulations of diet and regimen which experience had proved useful in such affections. He enjoyed the occasional advice of men eminent in the profession, and went the round of antidotes for the varying features of his multiform complaints; contriving to prosecute his sedentary profession, cheered and invigorated at one time, by the relaxation and enjoyments of a country life, and depressed and exhausted at another, by the toils and anxieties of the Parliament House. His malady though obstinate, had produced no serious lesion, and his life, on more than one occasion, had been pronounced eligible for assurance, and selected accordingly.

Years passed away, and he was still a dyspeptic. I regret that at that time I had not yet turned my attention to a careful examination of the urine in his class of ailments, as, I doubt not, it would have elicited much valuable information in regard to their nature, and assisted materially the successful treatment of the case. Mr D— passed from under my charge, he passed into abler hands, and, under the care of the late Dr Davidson, his symptoms underwent all that patient investigation which our friend was accustomed to exercise, and profited by that extensive knowledge which he was well known to possess. But after years of treatment, he was still a growing dyspeptic. Under the names of acute and chronic, of nervous and irritative dyspepsia, his ailments had been tried by a variety of remedies, but they had all failed to effect a cure. He fell into the hands of the homœopathist, and there for a time he rested, and fancied he obtained relief. It was not, however, lasting or complete. The globules of the followers of Hahnemann, and the regimen which accompanies them, were found, after a fair trial, to have accomplished nothing, beyond what the full doses and the varying dietetics of the regular practitioner had as yet effected. Hydropathy and the German Spas remained to be tried. I met him in the summer of 1845, on the outskirts of the city, on his way to embark for the Continent. He was pale and emaciated, and in his countenance I thought I

could discern the lineaments of organic disease. I urged him strongly to remain for a time in London, and consult Dr Prout, having, by this time, become sensible of the value of those discoveries with which his name is so much associated, and strongly impressed with the belief that my former patient was the subject of oxaluria. He neglected, for a time at least, the advice thus proffered; he proceeded to the Isle of Wight, and there spent some time at the water cure; but after a while, he returned to his native county in the north of Scotland, a hopeless dyspeptic and hypochondriac. It was then that a near relation of his own, belonging to the medical profession, thought it desirable to transmit to the eminent physician referred to, a written statement of Mr D—'s case. It was followed by a speedy reply, in which he expressed his decided opinion that the patient was suffering from the oxalic diathesis, and suggested a careful analysis of the urine, with the view of establishing the point. The examination confirmed the correctness of the view taken, and the patient was directed to the mode of treatment suitable to his case, including, among other means, the use of the nitro-muriatic acid. In a few months he made remarkable progress, and towards the close of the year he returned to Edinburgh with his health apparently completely re-established. I saw him immediately on his return, and was much struck with the great change in his appearance. He had indeed the look of health; and, in reply to all questions touching his former symptoms, assured me, that one and all had disappeared under the use of the acids which he had persistently employed for many months. He now returned to his professional duties, in which he engaged with renewed energies, and in prosecuting which he spent the depth of winter and the early weeks of the ensuing spring of 1846. During the months of January and February, however, he had been remarked to have fallen off in looks and flesh, and had shown a marked susceptibility to cold. On the 11th of March, in consequence, as it was supposed, of such exposure some days previously, he suffered from a febrile attack, accompanied by short dry cough, and was for the first time confined to the house. There were no symptoms present of an inflammatory character, but percussion discovered considerable dulness over the upper part of the left side, and auscultation failed to elicit the natural respiratory murmur. The symptoms, however, which occasioned most suspicion were the presence of considerable œdema of both ankles, and the coagulability of the urine by heat and nitric acid. The urine was still of amber colour, and of sp. gr. 1·018, and under the microscope disclosed a few crystals of the oxalate of lime. These latter characters, however, gradually disappeared, it became pale and serous, and of lower specific gravity; œdema of the ankles increased; dyspnœa and cough, accompanied with expectoration, came on; he was annoyed and exhausted by urgent diarrhœa; and was seized, on the 8th April, with pleuro-pneumonia of the upper lobe of the right lung, with great dyspnœa and restlessness, and he died on the 11th, exactly one month from the commencement of the fatal illness. Examination on the following day discovered those lesions, the existence of which might readily have been foretold by the symptoms during life; but it revealed the absence of others which might well have been expected to have presented themselves during the progress of a long-standing affection of the stomach. The upper lobe of the right lung was the seat of recent inflammation; it was condensed, the pleura inflamed and covered with recent lymph. The left lung was adherent to the ribs by old effusion; the upper lobe hepatised in part, and here and there studded with tubercles of considerable size, some of which had passed into the suppurative stage. Both kidneys were enlarged, pale, and waxy; the right, at its inferior edge, was the seat of a malignant fungoid growth, of the size of a large walnut. The stomach and bowels, which were carefully traced and minutely examined by Dr Spittal, who kindly conducted the dissection, were remarkably free from disease.

This is a painful and instructive history. It has its parallel in many of every day occurrence;—cases in which deranged digestion

and mal-assimilation have loaded the circulating fluids with impurities which have poisoned the health, enfeebled the nerves, and thrown an oppressive weight of duty on the kidneys and other emunctories. Over-taxed by the extra work, these last have become themselves the seat of irritation, congestion, and chronic inflammation; passing ultimately into that degeneration by which these organs refuse their office, thus loading the blood still more with those noxious particles they are unable to remove, and exposing the system to the risk or certainty of those intercurrent affections of dropsy and diarrhœa, of pleurisy and pericarditis, and many others, which, sooner or later, perhaps suddenly hurry on the fatal issue.

The remarkable and complete remission which took place in all the prominent symptoms of the case, after the administration of the acid treatment, is a striking feature in Mr D—'s history. No doubt the means employed overcame the oxalic diathesis, and the assimilating functions were restored to a normal condition; but still, I apprehend, that the fatal event was intimately connected with their previous long-continued disorder, and that even during the period of convalescence and temporary recovery, changes in the organic structure of the kidneys had commenced, which attained their full development, when the secondary inflammation in the chest indicated that the renal apparatus could no longer serve to eliminate from the blood, the urea and other impurities with which it was overcharged. These views receive a full confirmation from our knowledge of the progress of other renal affections, and particularly of that of Bright's disease, as well as from observation of the course of rheumatic fever in engendering the secondary inflammations of the membranes of the heart and lungs, which so very often are characteristic features of that disease.[1]

It has been alleged,—and I apprehend that the opinion is very generally entertained,—that the deposit in the urine of the oxalate of lime is of rare occurrence, and that the morbid condition of the blood in which it originates cannot be considered as identified with the ordinary forms of indigestion. On the contrary, I am clearly of opinion, that the deposit in question is of very frequent appearance, and that its continuance in the urine marks a peculiar diathesis which must have resulted from mal-assimilation, characterised by more or less of that train of symptoms detailed in the foregoing histories. These cases occurred to me in practice along with several others, all within a short period of each other; and taking a retrospective view of the sufferers from stomach and nervous complaints by whom I have been consulted during the last five years, I find that the large proportion of them have been the subjects of the oxalic diathesis; and that the dyspeptic and hypochondriac symptoms have yielded and

[1] See an admirable paper by Dr Taylor in the 28th volume of the "Medico-Chirurgical Transactions" of London.

disappeared, just in proportion to the diminution and retirement of the oxalate of lime in the urine. The indigestion and mal-assimilation which result in the *lithic acid diathesis*, and which are exemplified in gout and rheumatism, in febrile and inflammatory ailments, are, no doubt, of more frequent occurrence, but it is comparatively seldom that we have to deal with these as a form of dyspepsia, the primary disorder being generally overlooked in the more pressing urgency of other symptoms which follow in its train. The symptoms, again, which denote the *phosphatic diathesis*, though generally regarded as belonging to indigestion, are by no means so marked or so common as those connected with the oxalic poison, and are characterised by more serious disorder of the brain and spinal marrow. Many of the most obstinate and severe headaches which we are called to treat in young women, and in those of a certain age, and which are usually regarded as nervous and dyspeptic, are dependent on this diathesis, and yield only to a plan of treatment by which the mal-assimilation is corrected, the blood purified, and the earthy salts removed from the urine.

The truth of the ancient adage, that "the knowledge of a disease is half its cure," is more than confirmed in the diagnosis of the oxalic diathesis; for, as certainly as we discover the persistent presence of the characteristic crystals in the urine, so certainly do we possess the means of cure in appropriate diet and suitable remedies, unless where long neglect of functional derangement has led to uncurable organic disease. I take leave to point to the three cases first detailed, in confirmation of this statement, and as affording a great encouragement to all who are subjected to the vexations and disappointments inseparably connected with the management of the chronic dyspeptic and hypochondriac; and would even point to the last and fatal case of Mr D—, as at once a beacon and an encouragement, for that too bore testimony to the efficacy of the same treatment, which was adopted, unhappily, too late to save life, though not too late to renovate the health, and prolong the days, already doomed to be broken and cut short by the insidious yet resistless progress of malignant organic disease.

ARTICLE II.—*On Amputation at the Ankle-Joint by Internal Lateral Flap.* By Dr R. J. MACKENZIE, Assistant Surgeon to the Royal Infirmary, Edinburgh.

NUMEROUS objections have been brought forward against the tibio-tarsal or ankle-joint amputation, as proposed by Mr Syme in 1842. But, I believe, the chief obstacle to its general adoption, has been the difficulty of performing the operation. This objection has now, in a great measure, been removed. It is no longer found necessary to carry the incision across the sole so far forwards as at first practised

by Mr Syme. The dissection of the integuments from the *os calcis* is thus rendered much more easy, and the risk of endangering the vitality of the flap by injuring the vessels at its base is, in a great measure, avoided. The operation, as at first performed by Mr Syme, was tedious and difficult, and I believe that the stretching required in separating the long flap from around the *os calcis* occasionally proved injurious to the vitality of the flap.

With a view of facilitating the performance of the operation, and, at the same time, of preserving the thick integuments of the heel without incurring the danger of injuring the posterior tibial artery, either by the point of the knife or by undue violence in separating the flap, I proposed the operation, which I am now about to describe. These objects, I conceive, have since been attained by the modification which Mr Syme has made in his own operation, and I believe that the incisions now recommended and practised by Mr Syme, will, in the majority of cases, be found best calculated to ensure the formation of a good and useful stump.

I am induced, however, to direct attention to the following mode of operating, from the fact that, in some cases, it appears to me to possess advantages over the other operation; whilst, occasionally, cases may occur where the integuments of the heel may be saved to form a covering for the ends of the bones, only by the following incisions:—

On the Right Foot.—The body reclining on the right side, and the foot and ankle projecting beyond the table with their internal aspect upwards, the point of the knife is entered in the mesial line of the posterior aspect of the ankle, on a level with the articulation, carried downwards obliquely across the *tendo achillis* towards the external border of the plantar aspect of the heel, along which it is continued in a semilunar direction. The incision is then curved across the sole of the foot, and terminates on the inner side of the tendon of the *tibialis anticus*, about an inch in front of the inner *malleolus*.

The second incision is carried across the outer aspect of the ankle in a semilunar direction, between the extremities of the first incisions, the convexity of the incision downwards, and passing half-an-inch below the external *malleolus*.

The flap is now dissected up, care being taken that the knife cuts close on the bones, so as to preserve the whole thickness of the soft parts. By holding the base of the flap between the fingers and thumb, as it is detached from the bones, all risk of wounding the artery at this stage is avoided. The foot is then severed at the arti-

culation, and the ends of the *tibia* and *fibula* cleared so as to allow of the application of the saw, by which a thin slice of the extremities of these bones is removed, as in Mr Syme's operation.

This operation is very easily and rapidly performed, and, with a very moderate amount of care on the part of the operator, the artery runs no risk of being injured. It is in clearing the ends of the *tibia* and *fibula* for the application of the saw in this, as well as in Mr Syme's operation, that the artery is, I believe, in the greatest danger of being wounded. It is avoided by cutting close on the bones, a point to be attended to during the whole dissection in either operation.

The operation on the left foot is the same, but, as the patient reclines on the left side, the order of incision is reversed, the knife being first entered at the point above indicated, in front of the inner *malleolus*.

M. Sedillot of Strasbourg has since recommended a similar method of performing the operation. The incisions made by M. S. scarcely differ at all from those I have recommended; but their order is reversed, the first incision being made on the fibular aspect of the foot, the articulation opened, and the flap cut from within outwards.

I have amputated in the manner I have described in two cases of disease of the tarsus, and my colleague, Dr Douglas Maclagan, has performed the same operation three times. The results of the cases have ultimately proved satisfactory, although in one the cure proved tedious from the long continuance of sinuses around the stump, and in another gangrene of part of the flap took place, from the posterior tibial artery having been accidentally divided in clearing the ends of the *tibia* and *fibula*. In both, however, a satisfactory result was ultimately obtained.

The accompanying sketch represents the form of the stump which is left, which, it will be observed, differs from that formed by Mr Syme's operation only in the situation of the cicatrix, which is here on the fibular side of the stump. With an artificial foot, the patients are able to walk with ease, and but slightly perceptible lameness, the weight of the body resting on the face of the stump.

It has been objected to the operation by internal lateral flap, that the vitality of the flap is endangered from the external late-

ral vascular connections—(viz., the terminal ramifications of the external malleolar and interosseal branches of the peroneal artery) being divided. I feel satisfied that no apprehensions need be entertained on the ground of a deficiency of vascular supply. Numerous branches of the posterior tibial artery, of considerable size, are distributed in the substance of the flap, usually requiring the application of seven or eight ligatures at the time of the operation.

The cases in which, I think, this operation may with advantage be substituted for Mr Syme's method, are—1st, those in which the integuments on the outer side of the ankle are in an unsound state either from disease or injury; 2d, in elderly persons, whose arteries are not well suited to bear with impunity the stretching, which occasionally occurs in separating the integuments from around a projecting heel; lastly, it may, from the ease with which it is performed, be preferred by those who have not the frequent opportunities of operating on the dead body, which are requisite for enabling them to perform the other operation with facility.

ARTICLE III.—*On Typhoid and Typhus Fevers,—An Attempt to Determine the Question of their Identity or Non-Identity, by an Analysis of the Symptoms, and of the Appearances found after Death in Sixty-six Fatal Cases of Continued Fever, observed at the London Fever Hospital from January* 1847 *to February* 1849. By W. JENNER, M.D., London, Licentiate of the Royal College of Physicians.—(*Continued from p.* 828.)

THE PATHOLOGICAL LESIONS AND CADAVERIC CHANGES NOTED ON EXAMINATION AFTER DEATH OF THE BODIES OF THE SIXTY-SIX PATIENTS WHOSE SYMPTOMS HAVE BEEN ANALYSED.

MY object being to analyse the cases of typhoid and typhus fevers with reference to each other, *i.e.*, for the special purpose of comparison, and not with the intention of determining the symptoms and appearances found after death from the two diseases, I have thought it useless to include in this analysis the deviations from normal structure noted in the cases of typhoid fever which proved fatal after the thirty-fifth day, and in those of typhus fever which proved fatal after the twenty-ninth day, except so far as relates to traces of ulceration of the intestinal mucous membrane, and of the larynx, pharynx, œsophagus, and stomach, and the remains of blood in the cavity of the arachnoid. The same purpose has rendered unnecessary, that which would be very necessary if my object had been to determine the symptoms and the lesions of the two affections positively, and not comparatively, —viz., a third set of fatal cases, taken indiscriminately, of other acute diseases. Though I may occasionally be tempted to trace the relations between the symptoms and appearances discovered after

death, it would extend my paper far beyond the limits assigned to it, if I should do so in many instances. No such attempt is necessary for my special purpose, *i. e.*, the proof of the non-identity of the two fevers.

CADAVERIC RIGIDITY.

Typhoid Fever.—Sixteen bodies of the twenty-three patients who died of typhoid fever, were examined with reference to the presence and amount of cadaveric rigidity. The condition of seven of the sixteen was noted within twenty-four hours, six between twenty-four and thirty-six, one forty-two and a-half, one fifty, and one fifty-eight hours after death.

In one of the sixteen, examined twenty-two and a-half hours after death, there was no rigor-mortis, and in another, examined thirty hours after the fatal termination, it was moderately well marked only; in all of the remaining fourteen it was well marked at the times, respectively, at which they were examined.

Typhus Fever.—The bodies of thirty-four of the forty-three patients were examined with reference to this point; fourteen of them within twenty-four hours after death. In six of these fourteen the cadaveric rigidity was well marked throughout the body; in five it had in a great measure disappeared from the upper extremities; in one from the lower; in one it had entirely disappeared from the whole body; and in one it was little marked. In thirteen cases the body was examined between twenty-four and thirty-six hours after death. In one of these thirteen only was the rigor-mortis well marked in the upper and lower extremities; in four it had disappeared from the upper limbs; in three it was moderate in the legs and arms; and in five it was but little marked in either of the extremities. Of seven other bodies examined, with respect to the point we are considering, more than thirty-six hours after death, it was well marked in one forty-eight hours after death; it was moderately well marked in another forty-two hours, and had disappeared from a third forty-two hours, and nearly so from a fourth forty hours, after death;—while it was present in the lower extremities, examined in the remaining three of the seven, respectively, thirty-eight, forty-five, and fifty-two hours after the termination of life.

Thus, cadaveric rigidity was well marked in all the extremities of six of the seven bodies of the patients who died from typhoid fever and were examined during the first twenty-four hours after death with reference to the point in question; while it was well marked in all the limbs in six only out of fourteen dead from typhus fever examined during the same period; or if we take the whole of the cases, it was more or less absent in two of the sixteen bodies of the patients who died of typhoid fever, *i. e.*, one eighth of them, or in the proportion of 12·5 per cent.; while it had more or less disappeared, during the same period, from twenty-six of the thirty-four cases of typhus fever, *i. e.*, from more

than three-fourths of them, or in the proportion of 79·4 per cent. I was not aware of this remarkable difference having existed, till I made this analysis, or many points of interest connected with it might have been made out, but then I have the satisfaction of knowing, that if I have ascertained less on the point than I might have otherwise done, that in what I observed I could have been biassed by no pre-conceived opinion on the subject. Anxious to ascertain the real cause of the difference, I have examined the two groups, *i. e.*, those of typhoid and typhus fever, with reference to the state of the weather at the time the subjects lay in the dead house, the age and previous state of health of the patients, the local complications, and the duration of the disease in the separate cases included under each group. The result of my investigations on these points is, that none of these circumstances was the cause of the difference observed in the duration of the cadaveric rigidity, so that it could only have been due to some peculiar state of the system, induced by the disease itself. The only one of the fifteen cases of typhoid fever in which there was no cadaveric rigidity, was that in which the coma and prostration preceding death for some days, was the most extreme.

As the subjects were removed from the shell to the dead-house table by the arms and legs, it is evident that force was applied to the knees in such a mode as to prevent their flexure, while it was applied to the arms so as to make them bend, and this probably was the cause of the cadaveric rigidity being less marked in the latter than in the former limbs in so many cases, but then the same mode of removing the body from the shell was used in the cases of typhus and typhoid fevers, and therefore, if the cadaveric rigidity had not disappeared spontaneously from the upper extremities in the former, it was overcome with a much less amount of force than in the latter.

SPOTS.

Typhoid Fever.—In one case only was there a trace after death of any spot marked during life. It was a very faintly marked, brownish point, and would undoubtedly have escaped notice if the rose-spot which caused it had not, during life, been encircled with an ink mark. It could by no possibility have been confounded with the spots about to be described under the head of typhus fever. It was limited to the surface, and, on section, was seen not to extend into the cutis.

Typhus Fever.—On the surface of the trunk and extremities, small spots of a purple colour were detected in twenty-seven cases; in eleven of the forty-three cases of typhus fever I am analysing, the spots had disappeared before death; no note was made of their presence or absence in the remaining five cases.

The spots noted during life as becoming paler on pressure, were after death of a faint brownish purple hue. Those marked as unaffected by pressure during life retained generally the appearance they pre-

sented before death, and in some instances the ink-marks made during life indicating that a particular spot had passed through all the *three stages* described in a former part of this paper, under the head mulberry rash, were still visible, the spot after death presenting the character of the third stage, *i.e.*, a non-elevated purple spot, with well defined border, unaffected by pressure.

As during life, so after death, the spots situated on the most depending parts of the body, the subject being on its back, were the darkest in hue, and were often well marked when those on the anterior surface were scarcely visible. That this difference in hue was dependent on position, was in some measure proved by the fact, that the intensity of the hue gradually increased down the sides of the trunk, *i. e.*, passing from before backwards.

A piece of skin being removed from the subject, the following appearances were observed with the aid of a lens,—a faint brownish red or purple hue of the spots marked, as only fading on pressure during life, limited to the surface of the cutis. The purple colour of the well defined spots, unchanged by pressure during life, affected the whole thickness of the cutis, and even extended into the subcutaneous tissue ; a few minute vessels, still loaded with blood, converged towards the discoloured spot, but none were discernible in the spots themselves ; their colour appeared to be due to infiltration of the tissue by a solution of hematin. The hue of these spots was even darker below the epidermis than on the surface.

That the spots observed during life were the same as those noted to be present after death, was proved by the ink-marks remaining which had, before death, been placed around several of them for the purpose of securing attention to the same spots throughout their varying changes. The absolute identity of the spots was thus demonstrated.

As no spot was to be seen, with the one exception previously referred to, within the circle made during life around the rose-spots of typhoid fever, it was indisputably proved that those spots were not permanent. As no patient affected with typhus fever died while the mulberry rash was in its first stage, I am unable to say whether, in that stage, *i. e.*, while the spots constituting it disappeared on pressure, they would have been visible after death. I should suppose not, *a priori*.

Miliary Vesicles.—As I stated while analysing the symptoms, miliary vesicles were detected in four of the cases of typhoid fever after death, and in three of the cases of typhus. The difference in the proportion being, as I then pointed out, due to the *age* of the patients rather than to the *disease*.

VERGETURE OR DISCOLORATION OF THE POSTERIOR SURFACE OF THE TRUNK AND EXTREMITIES.

Of the difference on this point my notes enable me to say nothing positive, because I failed to record its varying intensity. To a greater

or less extent it was present in all the cases of both groups of which notes on the point were made. My impression—but how fallacious impressions are, none can be more deeply sensible than myself—is that it was much deeper in hue, and extended far higher up the sides of the trunk, the subject being on its back, in those dead from typhus than in those dead from typhoid fever.

GREEN OR PURPLE DISCOLORATION OF THE CUTIS INDICATING THE COURSE OF THE LARGER VEINS.

Typhoid Fever.—This cadaveric change was not present in a single instance.

Typhus Fever.—Excluding five cases that survived more than twenty-nine days discoloration in the course of the veins was observed in six, or in nearly one-sixth of the remaining thirty-eight cases.

GREEN DISCOLORATION OF THE WALLS OF THE ABDOMEN AND THORAX.

Typhoid Fever.—Omitting four cases that survived beyond the thirty-fifth day of the disease, two cases of which no notes on the point were made, there remain eighteen of the twenty-three cases of typhoid fever.—In three or one-sixth of the eighteen cases the abdominal walls were more or less discoloured, the subjects being examined, respectively, twenty-four, twenty-six, and thirty-six hours after death.

Typhus Fever.—Of the thirty-eight of the forty-three cases available for analysis, with reference to this point, the abdominal parietes were discoloured in ten, or more than a fourth. The patients being examined respectively, thirty-eight, twenty-five and a-half, forty-two, twenty-six, fifty-two, twenty-four, twenty-seven and a quarter, forty, twenty-seven, thirty-five and a half, hours after death.

That the discoloration was due to some cause acting from within, was proved by those parts which were protected from the action of —say a gas generated within—retaining their natural hue; when the liver projected below the ribs, that portion of the abdominal wall with which it lay in contact, preserved its natural colour, and in one instance, when the bladder was distended, its position was clearly marked out by the green hue of the abdominal parietes around. The intercostal spaces were always affected before the skin covering the ribs, and the cutis over the larynx before the sides of the neck; the skin around the mouth before the other parts of the face. In one case in which there was gangrene of the lung, the subject æt. twelve, being examined twenty-six hours after death, green discoloration of the intercostal spaces was well marked, while the skin over the ribs retained its normal appearance.

EMACIATION.

Typhoid Fever.—In eight, or nearly one-half of the eighteen cases,

there was marked emaciation; in five of the eight, extreme emaciation; in five, no emaciation; of five cases no note was made.

Typhus Fever.—In seven, or less than one-fifth of the thirty-eight cases, there was more or less emaciation; in three of the eight only was it extreme; in seventeen cases there was no emaciation; of thirteen cases no note on this point was made.

MUSCLES.

Typhoid Fever.—In one case only were the muscles noted to be particularly dark in hue.

Typhus.—In six, or about one-sixth of the thirty-eight cases, the muscles were noted to be of an unusually dark colour. In two cases there was a considerable quantity of dark loosely coagulated blood in the substance, and anterior and posterior to, but within, the sheath of the rectus abdominis; in both instances it was situated between the umbilicus and pubis, in one of the two in both recti, in one in the left rectus only; the clot in the former case extended for about six inches in length, in the latter two only. The muscular substance in both was softened at the part.

HEAD.

Typhoid Fever.—Fifteen only of the twenty-three cases of typhoid fever are available for analysis, the remaining eight either lived more than thirty-five days, or I was unable to examine the head.

The consistence of the brain generally, in the fifteen cases, was normal; there was no trace of softening or abnormal induration in any portion of the cerebral substance. Seven of these fifteen brains were examined more than twenty-four hours, one forty-four and a half hours, and one as late as fifty-eight hours after death. The two latter, however, were examined in the month of January, the thermometer, during the time the latter case lay in the dead-house, standing below 32°.

In one case there was no trace of the commissura mollis; the subject was a man thirty years of age—was examined thirty hours after death, in the month of October, the weather at the time being wet and cold. In five of the fifteen the supra cortical layer of white matter was more or less marked.[1]

Congestion of the cerebral substance.—Fifteen of the twenty-three cases of typhoid fever were examined with reference to this condition of the brain, and in two of the fifteen—*i. e.*, less than one-seventh, or in the proportion of 13·4 per cent., the red points were more numerous than natural.

Membranes.—The dura matter retained its normal appearance in every one of the fifteen cases. In one case only was there marked

[1] See Baillarger—Seances de l'Academie de Medecine, June 21, 1840.

congestion of the pia mater, and in only four others slight increased congestion of the larger vessels of the pia mater. While in the remaining ten cases they were normal, or paler than natural, so that the non-congested were to the congested in the proportion of two to one, and to the intensely congested, fourteen to one. In nine only of the fifteen cases was any note made of the facility with which the membranes were able to be stripped from the convolutions of the brain, and in one case only were these noted able to be removed with abnormal facility, and in larger portions than natural.

Fluid contained within the Cranial Cavity.—The quantity of fluid found after death within the cavity of the arachnoid on the lower surface, and at the base of the brain, was noted in seven of the fifteen cases available for analysis; in eight cases no note was made on this point. The amount varied from half a drachm to an ounce, the average quantity being about half an ounce. *In all the cases it was colourless and transparent.*

Fluid of a similar character was noted to be present in the lateral ventricles in thirteen cases; the quantity varied from half a drachm to an ounce, the average quantity being about two or three drachms. Immediately beneath the arachnoid, or infiltrating the meshes of the pia mater, there was some effusion of colourless serosity in nine cases; in six others no note on the point was made, probably from there having been in these cases no deviation from health. In one case only was it noted to be considerable; in the other eight it was described as "a little," "very little," "very trifling."

Choroid Plexus.—There was no congestion of the vessels of the choroid plexus in any of the cases examined with reference to the fact.

Typhus Fever.—Omitting the cases in which no examination of the head was made after death, and those which survived beyond the twenty-ninth day of the disease, there remain thirty-six of the forty-three cases for analysis. In four of these thirty-six the consistence of the brain was slightly diminished; in one of the four the softening had, in the septum lucidum and fornix, attained a considerable degree; in a fifth case there was slight softening of the fornix, the remainder of the brain having a normal consistence; while in a sixth case there was softening of the septum lucidum only. The first four subjects were examined respectively thirty-six, forty-eight, forty, and twenty hours after death, the last being the case in which there was considerable softening of the fornix and septum lucidum; the examination of the head in the fifth case was made fourteen, and in the sixth twenty-two and a-half hours after the death of the patients. In a seventh case, examined thirty-eight hours after the fatal termination, there was decided softening of the left half of the cerebellum. With these seven exceptions, the brain retained its normal consistence. In two cases, although carefully sought for, there was no trace of the commissura mollis; had this absence been only apparent—*i. e.*, due to softening

—the true nature of the case would have been manifested by an examination of the ventricular surface of the thalami optici; the serous membrane covering which would in such case have been wanting at the point where the cord of nervous matter emerges from the thalami. In one case there was an aperture in the septum lucidum, not formed by softening or handling; the edges of the perforation were firm and smooth, the serous membrane lining the two lateral ventricles being apparently continuous over its edges. The supracortical layer—*i. e.*, a film of white matter spread over the outer surface of the grey matter—was well marked in twelve cases, in two others imperfectly; no note with reference to the point was made in the remainder. For reasons into which I cannot at present enter, I believe this layer of white matter not to have been due to decolorisation of a portion of the grey by maceration.[1]

Congestion of the Cerebral Substance.—In fifteen of the thirty-six cases—*i. e.*, rather less than one-half, or in the proportion of 41·8 per cent.—the red points scattered over the cut surface of the brain were more numerous than natural.

Membranes.—In ten cases there was marked congestion of the dura matter; in five it was thickened and opaque—three of the five were more than fifty years of age.

In twenty-one of the thirty-six cases the larger vessels of the pia matter were more or less distended with blood. In eight of these twenty-one cases the smaller vessels were also congested; and in one case, in which the larger vessels were not abnormally congested, the smaller were minutely injected. In seven of the twenty-one cases, the congestion was trifling in amount. In nine cases the vessels of the pia matter appeared to be in a normal condition—at any rate there was in these nine no abnormal congestion. In four other cases there could have been no great deviation from a healthy state, as no note of their condition was made, although other points respecting the condition of the brain and its membranes were recorded. So that, if we consider the vessels of the pia matter to be normally injected in these four cases, the congested still bear to the non-congested the proportion of 160 to 100.

In seven of the thirty-six, or in one-fifth of the cases examined, there was intense congestion of the pia matter, while, as was before shown in the cases of typhoid fever, the moderately-congested bore to the non-congested the proportion of 1 to 2, and there was only one case in which the pia matter was intensely congested of the fifteen examined.

In eleven cases only was the facility with which the membranes separated from the surface of the convolutions noted, and in nine of the eleven they did so with abnormal facility, and in such large portions that my notes state that the membranes peeled off *en masse*. No

[1] See Baillarger, op. cit.

portion of the cerebral substance was in any case removed with the membranes; the facility, therefore, with which they separated from the cortical substance was not owing to softening of the grey matter.

I deeply regret that this particular was not more frequently noted, as, when well marked, it is one of the most characteristic appearances found after death from typhus fever, but by no means the anatomical character, as in one of these eleven they were removed with difficulty, and in another case with only slightly increased facility. The quantity of fluid found after death on the convex surface, and at the base of the brain within the cavity of the arachnoid was noted in twenty-five cases. In ten others, probably for the reason before assigned, no note was made. The quantity of fluid varied from about two drachms to two ounces; the average quantity being one ounce. In three cases it was slightly yellowish; in the others (excepting the cases of hemorrhage into the cavity of the arachnoid) it was transparent and colourless.

The lateral ventricles contained from a few drops to one ounce of transparent colourless fluid; the average quantity was from about two to three drachms.[1]

More or less serosity was infiltrated into the meshes of the pia matter, or effused beneath the arachnoid, in twenty-three cases. No note was made on the point in the remaining cases, probably for the reason before assigned—*i. e.*, that in these there was little or no deviation from health.

The quantity of serosity was decidedly greater in a large majority of the cases of typhus fever than in the cases of typhoid fever. The amount of the fluid having been in the former described by the terms "much," "considerable," and in two only of the twenty-three by the expression "very little."

Choroid Plexus.—In four cases the vessels of the choroid plexus were noted to be abnormally distended with blood.

HEMORRHAGE INTO THE CAVITY OF THE ARACHNOID.

Typhoid Fever.—In nineteen of the twenty-three cases the head was examined after death; and in none of these was there the slightest trace of blood in the cavity of the arachnoid.

Typhus Fever.—In thirty-nine cases the head was opened, and in five of them coagula of various sizes were found within the cavity of the

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[1] I think it right to state, that the amount of fluid was not *measured* in these cases; but as the quantities were estimated at the time the examination of the head was made by the same observer, it is probable that the comparative amounts are correct, although the absolute quantities are not available for accurate calculation. The fluid ought to have been removed by a pipette and measured, as it was by Dr John Reid, in his admirable researches into the pathological appearance found after death in fever. But as my guessed quantities differ but little from those obtained by accurate measurement by that philosophic observer and reasoner, I trust they are not far from the truth.

arachnoid. In every case the coagulum was in the form of a delicate red film, varying in thickness, and consequently in hue, in different cases and in different parts of the same clot. It was almost colourless where thinnest, bright red where a little thicker, and deep purple at the thickest parts. It was in every case situated on the convex surface of the brain, and in one stretched from the anterior lobe to the tentorium, and from the median fissure to a point corresponding to a line drawn transversely, just above the external auditory foramen. In one case it consisted of two or three delicate fibrinous films only. When the dura matter was reflected, part of the clot adhered to the layer of arachnoid lining that membrane, and part to the layer of arachnoid covering the pia matter. In three of the five cases the clot was double—*i. e.*, existed on both hemispheres of the brain. In two cases it was confined to the right side.

In one of the five it was accompanied by effusion of blood into the substance of the rectus abdominis. The substance of the brain was firm in four of the five, and apparently healthy in all. The vessels of the cerebral substance and its meninges were not particularly congested; the blood in the vessels of the pia matter fluid, but unable to be pressed out of them into the cavity of the arachnoid. No aperture could be found from which the blood had escaped—the sinuses were perfectly healthy: the source of the hemorrhage could not, consequently, be discovered.

(To be continued.)

ARTICLE IV.—*On the Local Treatment of Chronic Eczema and Impetigo.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine, and one of the Clinical Professors in the University of Edinburgh.

ECZEMA essentially consists of diffuse crops of minute vesicles, and impetigo of groups of small pustules, which breaking, and their contents concreting on the surface, form a crust or scab. In the first disease this scab is thin and laminated; in the second, it is rough, knotty, and prominent; and in eczema impetiginodes we observe the prominent crusts of the one, united with the laminated and often furfuraceous surface of the other. These characters, although they are easily distinguished in acute cases, become gradually more and more obscure in such as are chronic, until at length the skin assumes an unusual degree of induration, covered with a crust more or less thick, portions of which occasionally separating exhibit a uniform dark red surface. There is almost always either intense itching, or a feeling of burning or smarting often very painful, symptoms which continue when the eruptions have lost their original distinctive characters, and merged, as it were, into one another.

The acute form of these diseases will frequently be found connected with general derangements of the economy, without a proper manage-

ment of which no local treatment will be of much advantage. Of these in young persons a scrofulous taint, and in older ones various forms of dyspepsia, connected with oxaluria, or the lithic acid and phosphatic diatheses, are the most common. I need not dwell upon the importance in all such cases of instituting an appropriate constitutional treatment, which not unfrequently is in itself sufficient to remove the local disease. It is in this manner that sometimes acids, sometimes antacids, and at others cod-liver oil, or nutritive diet, and the mineral tonics, especially arsenic, are so useful.

There are other cases, however, where there is little, if any, constitutional disturbance, and where the disorder originates in, and is maintained by, local irritation, as on the hands and arms of stonemasons, grocers, bakers, cooks, reapers, and so on; and between cases almost purely local, and others almost purely constitutional, there is an infinite number of gradations, in which the one or the other is more or less predominant.

It may almost invariably be observed, however, that the more chronic the disease becomes, and the more dense the crust or subjacent integument, the more it seems to localize itself. The irritation and adherent crust appear to perpetuate and keep up the disorder. This is especially perceptible whenever it attacks parts covered with hair, where the scabs are separated with greater difficulty, from the exudation forming a dense mass with the imbedded hairs; so that chronic eczema and impetigo of the scalp, chin, and pubes, may be regarded, sooner or later, as almost entirely a local affection.

It is in cases of this kind that local applications are essential, and constitute the chief part of the treatment, whereas, in more acute cases, although they may be very useful, they are secondary in importance to internal remedies. The application I have employed for the last nine years has been a solution of ʒij. of the subcarbonate of soda, in a pint and a-half of water. But an alkaline lotion in itself, applied from time to time, as is most commonly done, will not prove of any great service. I have found it necessary to place lint saturated with the solution over the affected parts, and to cover the whole with oil-silk, in order to prevent evaporation. Keeping the surface moist seems to me a necessary part of the treatment. The usual effect is soon to remove all local irritation, and especially the itching or smarting so distressing to the patient; to keep the surface clean and prevent the accumulation of those scabs and crusts, which in themselves often tend to keep up the disease. After a time, even the indurated parts begin to soften, the margins of the eruption lose their fiery red colour, and merge into that of the healthy skin, and finally the whole surface assumes its normal character.

I am quite satisfied that this local treatment is far superior to the stimulating ointments and lotions, which are so frequently used one after the other, in the hope of overcoming these obstinate forms of eruption. Many of them, indeed, only seem to augment the disorder. In these cases there is an increased exudation from the skin,

not only of sebaceous but of purulent matters. Alkalies, we know, have the property of dissolving these, and acting as a calmative and emollient to the irritated part. Their constant action on the skin, therefore, in the manner I have described, may easily be imagined to be productive of benefit. But if, as is usually done, alkaline lotions and baths are only employed from time to time, they remove the sebaceous matter, leave the skin dry and harsh, and thus these applications often irritate. Keeping the surface covered with moist lint on the hand, while it serves to protect, removes the results of exudation and prevents incrustation.

I have now treated a great number of persons affected with chronic eczema and impetigo, and have succeeded in curing them in this way, after almost every other kind of local application had been tried in vain. I may cite a few cases as illustrative of these remarks —only premising that it is a matter of great practical importance that the diagnosis should be correct, because the chronic forms of lichen, prurigo and pityriasis, which are often mistaken for eczema or impetigo, require a totally different treatment.

Chronic Eczema of the Cheek.—Mr B—, a native of Riga, æt. 35, arrived in this country in the summer of 1847, with an eruption of chronic eczema on the left cheek. It presented a firmly adherent elevated yellow crust, about the size of a five shilling piece, a third of which encroached upon the left whisker, composed of thick and strong black hair. It had commenced nine month's previously in Paris, in a small spot, and in three months had reached its present size. He had sought the advice of several eminent dermatologists in Paris, and had applied numerous kinds of lotions and ointments, besides taking internal remedies, without effect. His general health was good—the tongue slightly furred. The digestive and urinary organs in every respect normal.

He was ordered to take five drops of the liq. arsenic., and tr. cantharid., in a bitter infusion three times a day, and to apply lint saturated in the alkaline solution to the affected part, which was to be kept moist by a covering of oil-silk.

In a fortnight he was in no way better, and I found that, owing to the prominent hair, the lint and oil-silk could not be brought in contact with the surface, and that the former was not kept moist. With some trouble I induced him to sacrifice a semicircular portion of his whisker, so that the surface could be kept moist with the alkaline solution. In a few days there was a marked improvement—the dry crust separated, exposing a bright red indurated surface—and the smarting and itching he formerly experienced entirely disappeared. Satisfied of the benefit which had been produced, he now cheerfully prosecuted the treatment. When he went out, and during the night, he fastened the lint, covered by the oil-skin, down by a broad piece of black ribbon, passing from the vertex of the head, in front of the ears, and tied under the chin. When engaged at home he simply kept the lint continually moist with the lotion. In three weeks the eruption was only half its former size, and in another four weeks it had entirely disappeared. The whisker has been again allowed to grow, and the skin is in every respect healthy.

That the local treatment was here the cause of cure, I feel persuaded; for, although the arsenical drops were continued throughout the treatment, the marked effect which followed the local applications, left little doubt in the patient's mind, as well as in my own, of their great benefit.

Chronic Impetigo of the Chin.—A clergyman, ætat. 38, otherwise in good health, consulted me, in the autumn of 1848, on account of an eruption which covered his chin. He had been affected with it upwards of three years, and had employed a variety of greasy applications, numerous lotions, and even submitted to the whole being cauterized on more than one occasion. When I first saw him the chin was covered over with an irregular crust, of a greenish-yellow colour, round the margins of which numerous impetiginous pustules, in all stages of development, existed here and there in groups. Independent of the pain and uneasiness, he was under the necessity, on account of the deformity occasioned, of keeping the lower part of the face constantly covered, and of late years had felt himself unable to perform his ministerial duties. I commenced the treatment of this case by, 1st, removing the crusts by means of successive poultices; 2d, causing the hair to be cut short with sharp scissors, and forbidding the use of the razor; and, lastly, after a week had elapsed in cleansing the surface, ordering it to be kept moist with the alkaline lotion. For this purpose a sort of cup was made of black silk, lined with oil silk, which fitted closely to the diseased surface, and was kept on by means of attached strings tied firmly behind. In this manner the surface was made constantly moist by the lotion, and he was enabled to take exercise readily. In a couple of days all smarting and irritation ceased; the beard was kept short by the scissors, and no other treatment was employed for two months, at the expiration of which time he was perfectly well.

In this case the treatment was altogether local, and of its good effects there could be no question.

Chronic Eczema and Impetigo of the Scalp.—I pursue the same practice, only in this case the head may be shaved from time to time, while the surface can easily be kept moist by means of an oil-skin cap, with a flap more or less long to cover the neck behind, should *it* be affected.

These diseases are so common that it is unnecessary for me to cite instances of their cure. I need only mention that the Crusta Lactea in infants, as a general rule, ought not to be treated locally at all; and that, in young children of scrofulous habit, good diet, exercise, and cod liver oil constitute the essential part of the treatment, although continued moisture with the alkaline lotion will, in proper cases, prove a valuable auxiliary.

Chronic Eczema of the Pubes and Abdomen.—In the winter of 1844 I treated an old gentleman for chronic eczema of the pubes, extending over the abdomen as high as the umbilicus. He was 60 years of age, and had laboured under the disease for upwards of eight years. The affected surface was of a dark red colour, covered with thin laminated crusts, which separated in triangular or square-shaped scales, some of which were an inch in diameter. He stated that the itching and smarting which he occasionally felt were often unbearable, and obliged him to leave any society he happened to be among, and to wash the parts with cold water, from which he always experienced temporary relief. He had taken arsenic and a variety of internal remedies, and tried all kinds of ointments, washes, and baths. He thought that a weak solution of the sulphate of copper did him more service than anything else. His general health was good, although the urine was strongly loaded with phosphates.

In this case the alkaline wash could not be kept constantly applied to the parts affected, on account of their position. I therefore advised him to procure a hip bath, and use it for one or two hours twice a day, with the alkali dissolved in the water. He had previously tried alkaline baths, and always with temporary benefit, and, therefore, willingly had recourse to them again. They

produced such good effects that he was in the habit of sitting in a bath, so arrayed that the fluid just covered the genitals, hips, and abdominal surface affected, for several hours consecutively. The phosphatic deposits in the urine were also removed by acid tonics. In two months the eruption was greatly diminished, both in extent and intensity, when he went to Harrogate, the water of which spring he drank for six weeks, but still continuing the alkaline baths. A short time afterwards he was entirely cured.

In this case, I think, there can be little doubt that the *continuous* bath was the chief agent in the cure. It is unnecessary for me to multiply cases, which would be merely repetitions of what has been stated, and which only vary on account of the mechanical expedients adopted to keep the affected parts moist, and an appropriate constitutional treatment, the importance of which I by no means undervalue.[1] Neither do I wish it to be understood that this is the *only* local application which can be useful in these cases, but that I have found it more extensively applicable, and more uniformly serviceable than any other.

ARTICLE V.—*On Anormal Nutrition in Articular Cartilages.* By P. REDFERN, M.D., Lond.; Lecturer on Anatomy and Physiology, and on Histology, at the University and King's College, Aberdeen.

SINCE the discovery of the structure and functions of nucleated cells in plants and animals by Schleiden and Schwann, it has become very important to determine with precision what part the blood-vessels perform in the functions of nutrition, growth, reproduction, secretion, and the production of such heterologous structures as cancer, tubercle, &c. In many of these processes there cannot be a doubt that, whilst blood-vessels are more or less necessary, the most important part is played by nucleated cells, which become the agents in the formation of tissues, whether homologous or heterologous, as well as the really efficient organs for the extraction of different substances from the blood, and for effecting various changes in them as they pass through the cell and become influenced by its development.

Perhaps there is no tissue in the body which affords a better opportunity for the examination of many of the changes which nucleated cells undergo, in the different periods of their existence, than that of cartilage; for in it changes are continually going on during the whole of life, such as take place at certain periods only in other

[1] In the Clinical Wards of the Royal Infirmary, when I am on duty, there will generally be found one or more cases of chronic eczema and impetigo undergoing this treatment, and open to inspection. At the present time (July 1849) there may be seen a case of eczema rubrum of the leg, of five years' standing, undergoing the process of cure from the treatment described.

textures. Moreover, these changes in the articular cartilages in man, are effected without any more direct communication with blood-vessels than exists upon their surfaces; for it is only when such cartilages exceed a certain thickness, about $\frac{1}{8}$th of an inch, that they are permeated by canals which contain blood-vessels, as occurs in the larger mammals. The passage of vessels through the substance of this tissue will be seen to be very different, at least in degree, from that complete irrigation of the textures usually styled vascular, for yet a large portion of the substance of the cartilage is very far removed from any vessel containing blood. Now, as it has never been shown that cartilage, under any circumstances whatever, becomes vascular, it is clear that it is in the best possible position amongst the textures for the investigation of those processes which are continually going on without the immediate agency of blood-vessels, and take place, properly speaking, in the texture itself.

It is stated by Professor Alison, that the essential peculiarity of inflammation is a peculiar perversion of nutrition or of secretion; and in Professor Bennett's admirable work on Inflammation as a process of anormal nutrition, it is plainly demonstrated, that all the changes which take place in the matters exuded in inflammation are alone referable to the general plan by which the nutrition of healthy textures is effected, and that thus softening, induration, hypertrophy, ulceration, &c., are produced.

That changes, which may be equally referred to an anormal nutrition of the texture, take place in the diseases to which articular cartilages are so liable, appears plain from the nature of the changes that occur in them in such cases, and which I shall endeavour to describe and illustrate, as they appeared to me during lengthened examinations of a number of cases, the particulars of which will be afterwards detailed.

It is said that inflammation, ulceration, softening, hypertrophy, atrophy, and ossification, are the diseases to which cartilages are liable. Of the existence of inflammation in them, there is perhaps no other evidence than that furnished by ulceration, which is unquestionably the most important of all the affections of this texture, and is generally believed to be the climax of the most severe affections of the joints. Whether atrophy occurs, except as the result of such changes as take place in ulceration, is yet doubtful; whilst ossification appears to be confined to the cartilages of the larynx, the costal cartilages, and the fibro-cartilages found uniting the bones at the synchondroses.

The most important views of the nature and progress of ulceration in cartilages may now be briefly stated, that it may be seen whether or not they are consistent with the changes which microscopic observations reveal in the textures.

Sir B. Brodie saw very plainly that nutritive changes take place in cartilages, and concluded that they contain blood-vessels. He argued, 1st, that there are instances in which vessels extend into the carti-

lage from diseased bones; 2d, that, notwithstanding the great amount of friction to which many cartilages are exposed, no change takes place in them, and yet the enamel of the teeth wears away in old age; 3d, that the structure of cartilage becomes changed in disease in such a way as to indicate that new matter is deposited and the old absorbed, changes which Sir B. Brodie believed to require the existence of vessels in the interior of the cartilage for their accomplishment. This author, in the 3d edition of his very valuable work on Diseases of the Joints, states his belief, that ulceration or absorption of articular cartilages may take place from the action of their own vessels, and that ulceration may arise from:—"1st, Disease originating in the neighbouring soft parts, especially inflammation of the synovial membrane; 2dly, it may depend on a morbid condition of the cartilage itself; or, 3dly, on a chronic inflammation of the surface or substance of the bone with which it is connected; 4thly, it may be the result of a peculiar alteration in the condition of the cancellous structure of the bones, which is met with in young scrofulous persons." In his additional notes on ulceration of articular cartilages, Sir B. Brodie states, that he has not been able to satisfy himself as to the exact nature of the ulceration resulting from inflammation of the synovial membrane; and then, referring to cases of inflammation of the synovial membrane, in which ulceration begins in the centre of the cartilage, he states that of such cases neither Mr Key's hypothesis, nor any other, affords any reasonable explanation. Of the degeneration of cartilage into fibrous structure, he remarks: "I have no doubt that it often exists where it is never followed by ulceration, but I am also well assured that, in many other instances, it precedes, and, in fact, forms the first stage of the disease."

Mr Key[1] describes three forms of ulceration of articular cartilages accompanying respectively acute or chronic inflammation of the joint, or strumous disease of the bone, as well as "that in which the structure appears to undergo a change in its organisation, independent of foreign agency." In three of these conditions, he says, "the cartilage is not absorbed *per se*, but through the agency of a structure, probably evolved for the special purpose of completing that process;" and the fourth is "the result of disorganisation of texture."

Mr Key[2] states:—"The absence of all action in the cartilage, and a total want of vascularity in those parts where ulceration appears to be most active, were the circumstances that first led me to look for some agent in the work of ulceration." This statement occurs in speaking of ulceration caused by inflammation of the synovial membrane from wounds. Mr Key believes that the highly vascular membrane which covers the cartilages of such joints causes ulceration to take place in them, and that it progresses in proportion to the amount of the inflammation and the vascularity of the newly-formed

[1] Medico-Chirurgical Transactions, vols. xviii. and xix.
[2] Ibid, vol. xviii. p. 235.

membrane. And yet, in his further remarks[1] on ulceration resulting from inflammation caused by wounds, the same author adds :—"In these cases the cartilage is found often to be extensively destroyed, and the bone laid bare, without any appearance of a membrane for the purpose of absorption." But he distinguishes between absorption and ulceration as two modes, by either of which cartilage may be removed :[2]—"In some parts may be seen the membrane adhering to the cartilage or to the denuded bone, in various degrees of activity or vascularity, according as its office is completed or in progress; and in others, a total loss of the cartilage may be observed without the development of a membrane. It is not unusual for one half of a knee-joint to be losing its cartilage by absorption, while, by a process of inflammation subsequently excited, the other is in a state of active ulceration."

Regarding the removal of the cartilages of old people, Mr Key says :[3]—"I am unable to offer any very conclusive evidence as to the exact nature of the process in all such cases; but I believe it will be found to be analogous to the processes which I have previously described." He remarks on the fibrous degeneration :[4]—"It appears to me to be an action altogether different from absorption, and analogous to the softening of the intervertebral substance." Again :[5]—"It is a disease of a peculiar character, and differing in many respects from the ordinary affections of joints that end in the destruction of the cartilage."

Mr Mayo[6] believes Key's views to be at variance with the facts revealed by dissection, and states that "there are three distinct forms of ulceration, or three varieties of ulcerative disease, which, although they may be occasionally combined, are oftener met with separately." These are—rapid and clean excavation on the surface, with inflammation of the synovial membrane; chronic ulceration on the surface, with fibrous or brush-like projections, and inflammation of the synovial membrane; and ulceration on the deep surface, with inflammation of the bone and synovial membrane, and, in some instances, "with sensible vascularity of the cartilage itself." He reports several cases of ulcerated cartilages, in which, he says, that they were evidently vascular. These, however, are of no value in deciding the question regarding vascularity, as in no case was the minute structure examined; and Mr Liston says, that the preparations to which Mr Mayo refers, exhibit nothing at all satisfactory on the subject.

Mr Liston believed that he had injected vessels passing from the bone into the substance of diseased cartilage; but Professor Goodsir has plainly shown[7] that there is every reason to believe that such vessels ramify in a new texture occupying the place from which the

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[1] Medico-Chirurgical Transactions, vol. xix. p. 148.
[2] Ibid, vol. xix. p. 149.
[3] Ibid, vol. xviii. p. 243.
[4] Ibid, vol. xix. p. 135.
[5] Ibid, vol. xviii. p. 241.
[6] Ibid, vol. xix. p. 49.
[7] Anatomical and Pathological Observations.

cartilage has been removed by absorption. Moreover, Mr Liston himself says,[1] that in such cases ulceration may be proceeding from the free surface; that "the cartilage is often previously swollen and softened," and that "more than one form of the ulcerative process may sometimes be observed in the same articulation."

Mr Toynbee states:[2]—"The investigations of Sir B. Brodie, Mr Mayo, and more recently of Mr Liston, leave no doubt, that in some diseases to which articular cartilage is subject, blood-vessels are distributed throughout its substance." He speaks also of researches which he has made upon "those morbid states of articular cartilage, in which blood-vessels are prolonged into its substance, and upon the manner in which the vessels are introduced into it."

Henle says, that cartilage is neither liable to inflammation nor hypertrophy, and that atrophy depends on a want of nutrition from diminished vascularity of the neighbouring textures. He and Mandl[3] agree in the view that the changes to which cartilages are subject in diseased joints, depend on maceration in the pus, altered synovia, &c. M. Richet, Prosector to the Faculty of Medicine of Paris, takes a similar view,[4] and even says, that portions of cartilage are not unfrequently met with in joints in a very advanced stage of disorganisation, *retaining all the chemical and physical characters*. He proposes that the terms inflammation and ulceration of cartilage should be rejected, and replaced by the phrases, softening, wearing, "usure," and erosion; and shows that cartilage may be softened, reduced to a fibrous state, or even totally disappear in joints, which certainly cannot be called healthy, but which sometimes present little or no symptoms of derangement during life. M. Richet's reviewer[5] in 1846 says: "It is indisputable that cartilage is frequently removed by the action of the adjacent tissues, *without itself having any active participation in the process*."

Professor Goodsir attributes the removal of texture in ordinary ulceration to the formation of nucleated cells, which take up, during their growth, the materials of which the surrounding parts are composed. Speaking of ulceration of articular cartilages,[6] he describes a gelatinous or a false membrane on the surface, both consisting of nucleated particles, intermixed in scrofulous disease with tubercular granular matter, and in simple inflammation with fibres and blood-vessels. He was the first to point out that the cells of the cartilage "become larger, rounded or oviform, and instead of two or three nucleated cells in their interior, contain a mass of them. At the very edge of the ulcerated cartilage the cellular contents of the enlarged

[1] Medico-Chirurgical Transactions, vol. xxiii. p. 95.
[2] Philosophical Transactions, 1841, Part I, p. 172.
[3] Mandl—Manuel d'Anatomie Générale, p. 381.
[4] Recherches pour servir á l'Histoire des Tumeurs Blanches. (Annales de la Chirurg. Française et Etrangère, Mai, Juin, 1844.)
[5] British and Foreign Medical Review, vol. xxi. p. 136.
[6] Anatomical and Pathological Observations.

cartilage corpuscles communicate with the diseased membrane by openings more or less extended. If a portion of the false membrane be gradually torn off the cartilage, the latter will appear rough and honey-combed. Into each depression on its surface a nipple-like projection of the false membrane penetrates." Mr Goodsir points out that the cartilage may be attacked upon its attached surface in a similar manner, a vascular and false membrane being, in that case, formed between it and the bone, a layer of nucleated cells always existing between the loops of capillaries and the ulcerated surface. He adds: "The cartilage, where it is not covered by the false membrane, is unchanged in structure."

Mr South believes firmly in the truth of Wilson and Key's notion of the deposit of a new and vascular substance upon the synovial membrane being the usual cause of ulceration of cartilage, and that the cause is not in the cartilage itself. In support of this view, he quotes largely from Mr Goodsir's observations, in pages 17 to 19 of Mr G.'s work before-named.

Mr Rainey states that the absorption of cartilage is preceded by conversion of its nuclei into drops of oil, which are sufficiently characteristic, and capable of being removed by the addition of ether. The intercellular substance is also converted into granules of oil, arranged in irregular lines running in all directions, grooving and excavating the cartilage.

In Guy's Hospital Reports for October 1848, Mr Birkett recorded the examination of a diseased knee-joint, and maintained "that the destruction of articular cartilage depends chiefly upon a diseased state of the capillary vessels of the bone, whereby its nutrition is suspended, and that then disintegration simply, and not ulceration, takes place. That this disintegration is mainly attributable to a metamorphosis or conversion, first of the nuclei, and then of the intercellular or hyaline substance, into fat." Mr Birkett believes bone to be analogous to the true skin, and cartilage to epithelium;[1] and states, in opposition to all other observers, that the cells of articular cartilage, near to the bone, have no regular arrangement.

Such is the nature of this very instructive process as it has appeared to others. Without further remarks, I shall now proceed to give the results of other examinations, and endeavour to show that the facts are capable of being reconciled with a true physiology.

OBSERVATION I.—*Disease of Cartilage of the Knee-Joint without Symptoms—Severe comminuted and complicated Fracture of the Bones of the Leg—Amputation—Death from Exhaustion.*

Charles Clarke, æt. 42, labourer, admitted into the Aberdeen Royal Infirmary, December 16, 1848, under Dr Kerr, for a severe comminuted fracture of both bones of the leg, extending into the knee-joint, and produced by the fall-

[1] M. Richet in 1844 made a similar statement as to the analogy in structure and mode of nutrition of cartilage and epidermis, nails, &c.

ing of a metal roof upon him. Has always been a stout healthy man, though of intemperate habits. Ten or twelve years ago, he injured his ankle on a stone before his door, but it is not known whether he fell. He was confined to the house at this time for three weeks, and was attended by Dr C. of this town, who does not recollect the occurrence. Five years ago, when at his usual work, at the gas works, he was much hurt and scalded about the head and shoulders, by the bursting of a boiler, but his knee was not injured. He completely recovered from these injuries, and was in good health, at his work, at the period of occurrence of the accident which caused his admission. He never suffered from lameness, or made any complaint regarding his knee.

Amputation was performed in the thigh—the posterior flap sloughed, and the patient died from exhaustion on January 3, 1849. The body and sound limb were not examined.

On opening the knee-joint of the amputated limb, a quantity of blood was found, which had evidently escaped from vessels lacerated by a fracture separating the articular surfaces of the tibia. After washing away the blood, the surface of the cartilage, on the upper part of the internal condyle of the femur, was found considerably elevated, reddish in colour, and presenting a covering of fibres attached at one end to cartilage, apparently healthy, and projecting into the joint by the other. This was over an extent of surface which could be covered with a sixpence, whilst at a little distance, and directly upon the trochlear surface of the femur, one-fourth of an inch distant from the edge of the cartilage, an irregular excavation was found, of the size of a split pea. The edges of this excavation were loose, and overhung its bottom, the deepest part being covered with a thin layer of apparently healthy cartilage, and the whole exposed surface reddish in colour, and split into fibres as in the spot before-named. The internal portion of the articular surface of the patella, and the central part of the vertical ridge separating this from the external part, were covered with cartilage in a similar state of disease. Here, however, there was neither excavation nor elevation of the diseased part; but, as in the other two diseased spots, the greater part of the disease was separated from the edge of the cartilage by a distance of one-fourth of an inch, occupied by the last-named substance in a perfectly normal state.

Microscopic Examination.—Portions of cartilage removed from the attached surface, in the regions affected by disease, and elsewhere, present a series of elongated cells, containing nuclei or other cells, placed vertically to the surfaces of the cartilage, and varying in length from $\frac{1}{900}$th to $\frac{1}{1300}$th of an inch. These cells, and the hyaline substance in which they lie, differ in no respect of structure from perfectly healthy cartilage.

Sections taken vertically at the diseased parts, show healthy cartilage at the part near to the bone, but on proceeding towards the free surface, numerous rounded, oblong, or oval cells are found, varying in length from $\frac{1}{100}$th to $\frac{1}{600}$th of an inch, and in breadth from $\frac{1}{220}$th to $\frac{1}{1000}$th of an inch. The smaller ones contain three or four irregularly rounded or oblong corpuscles, and many of the larger ones sixty or eighty similar bodies. These corpuscular nuclei vary in diameter from $\frac{1}{2000}$th to $\frac{1}{5000}$th of an inch, and the larger ones contain small nucleoli. At the same part, the intercellular substance is split into large bands, which, on arriving at the surface, are sub-divided into others, varying in breadth from $\frac{1}{500}$th to $\frac{1}{3300}$th of an inch, and all possessing the glistening and granular appearance of other parts of the hyaline substance—(Fig. I.) The cells above described, are found in the substance of the larger bands into which the intercellular substance has been split; in some places, their walls are indistinct; in others, altogether imperceptible, the nuclei, in the last-named places, being spread out in the texture, and either preserving the size and appearance which those within the cells present, or they are enlarged and seen to consist of a cell wall containing a nucleus—(Fig. II.) The whole texture at the diseased parts is of a reddish brown colour, and considerably darker than healthy cartilage.

Fig. I.

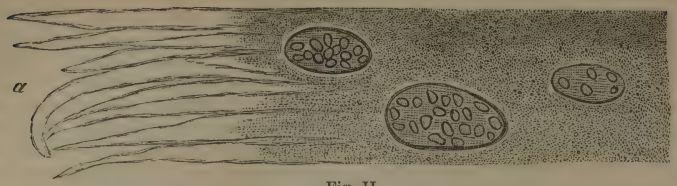


Fig. II.

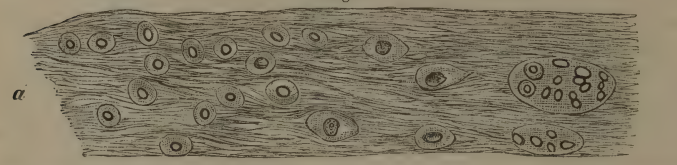


Fig. I.—*Portion of a vertical section through the diseased part; a, bands and fibres projecting into the joint; beyond b, the cartilage is healthy.*

Fig. II.—*Portion of a similar section; a, free surface. This band contains the original cells broken up into masses, or new cells, each one of which has its nucleus.*

Remarks.—The disease, which had proceeded in this case to the destruction of parts of the cartilage, through the greater extent of its thickness, without giving rise to any symptoms, commenced on the surface, at parts considerably removed from the edge, and progressed without the assistance of blood-vessels in the cartilage, or a newly formed membrane. It is evidently similar to that which was described by Sir B. Brodie, and adduced by him as a proof that ulceration of cartilage is caused by vessels passing into its substance, and referred to by Mr South, under the head of "self-ulceration of cartilage." The real characters of this disease, as shown by a microscopical examination, have not yet been made known, so far as I am aware. They consist in the gradual enlargement and rounding of the cells, which become filled with a multitude of corpuscles, which, on the bursting of the cells, are discharged into the texture, and removed from it by changes to be afterwards shown. The hyaline substance splits into bands and fibres, and is subsequently disintegrated and removed from the part. It will be afterwards shown whether these changes indicate a disease of a peculiar character, and altogether distinct from ordinary ulceration of cartilage, as is believed by Sir B. Brodie, South, and Key.

OBSERVATION II.—*Diabetes—Death from Diarrhœa—Extensive disease of the Cartilages of the Left Knee-Joint without Symptoms.*

A. R., æt. 38, labourer, admitted into the Aberdeen Royal Infirmary, December 5, 1848, under Dr Kilgour, for diabetes, the first indication of which he noticed during the previous summer. On admission, has every appearance of having been a strong and healthy man—is emaciated, complains of great thirst, and frequent micturition. Urine, 13lbs, sp. gr., 1040, gives every indication of the existence of a large quantity of sugar on the addition of yeast, the sulphate of copper test, and on microscopic examination.[1] During the treat-

[1] A full account of the symptoms, the progress of the case, and its treatment, is to be found in the hospital reports, but is here omitted as not being connected with the disease of the cartilages.

ment, the urine became much less in quantity, and of sp. gr. 1·025, but diarrhœa came on with extreme prostration, and he died on December 22, and was removed to the dissecting rooms, where the disease of the cartilages was discovered.

This patient walked perfectly well on his admission into the hospital, and made no complaint whatever of his knee. He stated that in his usual occupation of building dry stone walls, he had been much exposed to cold and wet. About ten weeks previously, he fell into a quarry thirty feet deep, and "injured his thigh and shoulder, which confined him to bed for a short time, but he was able to go about during his recovery; and, within a fortnight, to leave Fifeshire (where the accident happened) for home."

Examination of Left Shoulder.—The scapula presents two vertical fractures, one separating the glenoid cavity, and the coracoid process from the body, the other removing the acromion and a portion of the spine. The fragments are only slightly displaced, and very firm union has taken place. On the same side, five ribs below the first show a united fracture about the middle of each. The first rib, the humerus and clavicle show no trace of injury, but both clavicles were removed with difficulty at their sternal ends, owing to dense fibrous tissue around the articulations.

Examination of Left Knee-Joint.—The femur has its surface covered with healthy cartilage throughout. The central part of the external glenoid cavity of the tibia is evidently raised over an extent of surface equal in size to a small horse-bean. The swelling is highly elastic, and fluctuates indistinctly. It has no abrasion of any kind on the surface, but close to its edge, at the part looking towards the central and posterior part of the joint, there is a fissure extending backwards and inwards for one-fourth of an inch. The fissure is also oblique in relation to the surface, for, in passing down towards the bone, it inclines from before backwarks and outwards. It lodges a fibrous mass, which is adherent to the posterior lip of the fissure, and projects one-tenth of an inch beyond the surface. At its ends, the fissure appears limited, but from the anterior one, an indistinct depression leads forwards for two lines, bounding the raised part, and appearing like a trace of a healed fissure.

Two sections were carried through the external condyle of the tibia, from before backwards, perpendicularly to the surface of the cartilage, one passing through the centre of the raised portion, the other by the internal side of the fissure. These sections removed a portion of bone and cartilage, half-an-inch broad, of which the lateral surfaces are delineated in Fig. III. and IV. On both these sections the bone presents an even and perfectly distinct plate of compact tissue, from one-tenth to one-fifteenth of an inch in thickness, where it is in contact with the cartilage. The first section (Fig. III.), shows the car-

Fig. III.

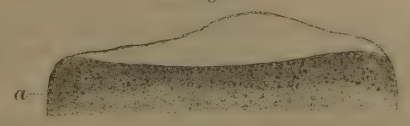


Fig. IV.

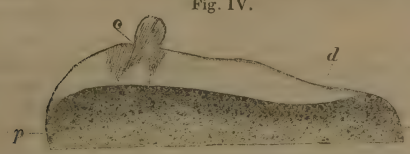


Fig. III.—*Appearance of the cartilage and bone on a section through the centre of the elevation on the external glenoid cavity of the left tibia;* a, *anterior,* p, *posterior, edge.*

Fig. IV.—*Appearance of a section parallel to the former, and half an inch internal to it;* a, *anterior,* p, *posterior, edge;* c, *tuft of fibrous tissue projecting from the fissure.*

tilage to be four-fifteenths of an inch thick at the raised part, and thinned off towards the edges, as represented in the figure, which is of the normal size. The other section, represented of the normal size in Fig. IV., shows the cartilage to be one-fifth of an inch thick at the most prominent part, and a quantity of fibrous tissue projecting one-eighth of an inch from the fissure.

The cartilage covering the internal glenoid cavity appears a little roughened, but it is found to be healthy. A section was also made through this condyle and its cartilage, from before backwards, and the appearance of the section is represented in Fig. V.

Fig. V.

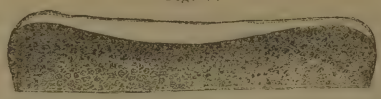


Fig. V.—*Section carried through the centre of the internal glanoid surface, from before backwards; a, anterior edge.*

Microscopic Examination.—The tuft, projecting from the fissure, consists of a multitude of large and small bands of hyaline substance, and of a mass of beautifully waved fibres, precisely like those of the lateral ligaments of the knee. These bands and fibres are attached to the cartilage at one end, and are free at the other. There are all degrees of this splitting into the waved fibres, many of the large bands being granular and hyaline at their attached ends, then indistinctly striated, and at their free ends a mass of pure fibrous tissue. The largest bands are about $\frac{1}{280}$ of an inch broad. Interspersed amongst the fibres and bands, are a number of beautifully rounded or oblong cells, generally in groups of three or four, containing nuclei of different sizes, situated excentrically. The cells vary in length from $\frac{1}{1000}$th to $\frac{1}{500}$th of an inch, and in breadth from $\frac{1}{1250}$th to $\frac{1}{830}$th of an inch. The nuclei contain a number of irregular, elongated, and bright granules, the largest of which are $\frac{1}{5000}$th of an inch, whilst many are not larger than $\frac{1}{15000}$th of an inch in diameter (Fig. VI.)

In some places there are patches of fibrous tissue of $\frac{1}{20}$th of an inch in extent, throughout which the cells, which are mixed with the fibres, are very uniform in size, measuring $\frac{1}{2000}$th of an inch by $\frac{1}{3300}$th, irregular, with thin and indistinct walls, and containing granular bodies similar to those just named, as belonging to nuclei, but of larger size. (Fig. VII.)

The bottom of the fissure does not reach the bone, but is separated from it by a thin layer of perfectly healthy cartilage. Owing to its oblique direction, sections can be obtained through the whole thickness of the cartilage, and having

Fig. VI.

Fig. VII.

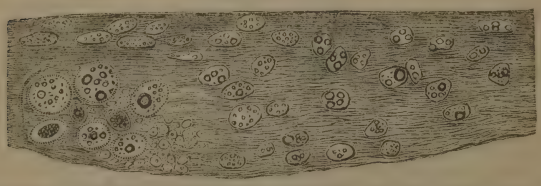


Fig. VI.—*Striated band, with cells interspersed, removed from the tuft of fibrous tissue, shown in* Fig. IV. c.

Fig. VII.—*Fibrous tissue, with small irregular cells, containing large bright granules.*

the fissure passing through them. Proceeding from the surface of such sections, the cells parallel to it are found but little changed in their characters, except in the immediate neighbourhood of the fissure; lower down, there are:—1st, large rounded or oblong cells, containing large nuclei; 2d, rounded patches of cells, which measure $\frac{1}{1400}$th of an inch by $\frac{1}{1700}$th, and contain nuclei or granules, the patches being $\frac{1}{200}$th of an inch long, by $\frac{1}{700}$th broad; 3d, small cells spread out in the texture, which has now, in the vicinity of the fissure, become distinctly split into bands and fibres, though nearer the surface it is granular and hyaline. Proceeding upwards from the bone, similar changes are seen, the lowest cells being in perpendicular groups, then enlarged and distended with small cells or corpuscles; whilst, nearer to the fissure, these are found mixed with the texture. The intercellular substance also, is hyaline and mottled near the bone, then striated, and, at the fissure, split into bands and fibres as on its opposite side.

Wherever the small cells or nuclei, which measure from $\frac{1}{1400}$th to $\frac{1}{3500}$th of an inch in diameter, are set free, and do not present their usual irregular outline, with their glistening and homogeneous appearance, they contain the irregular granules shown in the cells in Fig. VII., and, in that case, are almost always found in a dark and indistinct fibrous texture, which becomes so much darker and shrivelled, on being subjected to the action of a very weak solution of creasote, that the cells can then be seen with great difficulty.

On examining the general structure of the part not evidently thickened, as at Fig. IV., *d*, the horizontal arrangement of cells at the surface, and of vertical ones in the deeper part, are as distinct as in the most healthy cartilage, the cells themselves, and the hyaline substance, being also quite healthy. Even in the elevated parts, no change is noticed for some distance, except that the cells become less numerous and less regularly arranged in the tissue; a few are rounded or oblong, and increased in size, so as to measure $\frac{1}{500}$th by $\frac{1}{770}$th of an inch.

Left Patella.—Externally this bone is of the usual shape and size, but its articular surface is convex from above downwards, especially on the central ridge, and to the outer side of this; passing from without inwards, it is at first straight and then excessively convex, as shown in Fig. VIII., representing the appearance of a section, carried horizontally through the middle, on the upper half.

Fig. VIII.

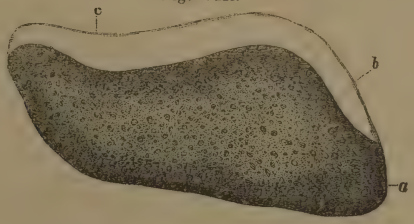


Fig. VIII.—*Horizontal section through the centre of the patella, shown on the upper half; a, internal edge; b, soft and spongy part; at c, the cartilage is soft and spongy in the middle of its thickness.*

The cartilage has a granular appearance on the surface, especially on its external half; and, on being placed in water, a number of short fibres float from it and give it a villous appearance. The whole surface has a finely striated appearance from above downwards.

Microscopic Examination.—The processes projecting into the interior of the joint, consist of bands of fine fibres, separated from each other at their free extremities, and only recognisable, as formed of hyaline substance, by tracing them to their attachment to the cartilage. Small elongated nuclei are visible amongst the fibres, and running in the same direction—(Fig. IX.) There are

also small cells to be found in some of the bands. Large masses, of a rounded form, and highly glistening character, are to be found loose amongst the fibres, attached to them at their extremities, or stuck upon them in different parts of their course, and forming a part of their substance—(Fig. X.) These bodies,

Fig. IX.

Fig. X.

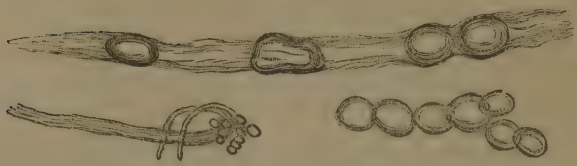


Fig. IX.—*Fibrous band projecting from the surface of the patella.*
Fig. X.—*Glistening bodies found loose and upon fibres which have no ordinary cells amongst them.*

when loose, have very much the appearance of starch globules without the hilus. They are of considerable consistence, and appear of the same density throughout. From their connection with the bands, their glistening character, and the absence of any save small and irregular cartilage cells where these bodies are found, it is tolerably certain that they are cartilage cells which have undergone this remarkable change in structure, and most probably one quite as great in their chemical characters. Lying amongst these loose bands and fibres, are a number of perfect octohedral crystals, completely insoluble in strong acetic acid. The side of the largest of these measures $\frac{1}{540}$th of an inch, of the others $\frac{1}{1700}$th, and many are so small as to be recognised as crystals only with difficulty.

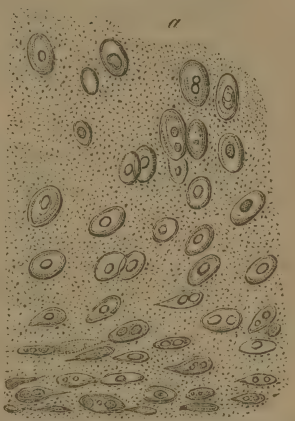
At the point of the section marked *b*, Fig. VIII., the texture is soft and spongy, the layer of cells parallel to the surface still remains, but its cells are considerably enlarged in both length and width, measuring on an average $\frac{1}{710}$th by $\frac{1}{1420}$th of an inch. A few are rounded, and these are much larger, measuring from $\frac{1}{300}$th to $\frac{1}{400}$th of an inch long, and $\frac{1}{400}$th to $\frac{1}{700}$th broad. The cells nearest the surface have lost almost all trace of their nuclei, and are evidently breaking up. They lie in a highly transparent hyaline substance, separated at some distance from each other. Those lower down contain nuclei or corpuscles of the usual forms, and these lie in a denser and granular texture, reddish-brown in colour, and darker than the healthy intercellular substance. —(See Fig. XI. and XII.) In the deeper parts, the texture is more healthy, the cells being of the usual form, and arranged in groups perpendicular to the surface, except where the intercellular substance is softened, and indistinctly fibrous; for, in all such places, the cells are much enlarged, and contain a number of smaller ones, of which the nuclei are the most prominent objects.

Near to the edge, on the opposite side, precisely similar changes have taken place, but the affection of the superficial cells has not proceeded so far. At the point marked *c*., Fig. VIII., the interior of the cartilage is soft and pulpy. The intercellular substance is indistinctly fibrous,—the cells are of large size, and many have discharged their contents (cells or nuclei), which are found mixed with the fibres. This change reaches the bone, but is most marked in the

middle of the thickness of the cartilage. The part of the surface which corresponds to it, is granular and reddened, and is at all points far distant from the edge of the cartilage.

Fig. XI.

Fig. XII.



Figs. XI. and XII.—*Vertical sections of cartilage from the surface of the patella; a, free surface.*

Remarks.—This was the case of a man, who, though walking about without any lameness, up to a fortnight previous to his death, had considerable disease of the cartilages of the patella and left tibia, involving, in some parts, nearly the whole thickness of the structure. There can be no doubt, therefore, that disease may go on to the complete removal of portions of articular cartilage in the centre of a joint, without any symptoms whatever; and, it is probable, that the whole of the cartilage might be so removed, were it not that other structures generally become involved in the disease. When it is considered that cartilage belongs to the simplest class of textures,—that its functions are mechanical ones—and that it is not furnished with nerves, these facts are not so surprising as they would otherwise appear. The fact of the existence of actual disease, in the two cases now related, is certain, from the changed condition of the structure. In the enlargement and crowding of the cells with corpuscles, and the splitting of the hyaline substance into bands, this case resembles the former, but here, fibrous tissue, in some parts absolutely incapable of being distinguished from that of ligaments, was formed by the splitting of the hyaline bands, whilst the nuclei of many of the cells were destroyed by being converted into granules. These granules were not affected by acetic acid, by the copious application of liquor potassæ for four hours, nor could they be seen to undergo any change on the addition of ether. In cells further advanced in disease, there are large globules of oil, but the granules are irregular in their outline, though like oil in other respects. Probably they constitute one stage in the conversion of nuclei into oil. In most parts the disease appears to

have commenced on the surface, but at *c*, (Fig. VIII.), both the cells and the hyaline substance in the deeper parts were considerably changed, whilst there was scarcely any affection of the surface, showing plainly that the disease may commence in any part of the cartilage. Whence the crystals of oxalate of lime arose, it is difficult to say. They were not to be found in any texture in which the diseased parts had been wrapped, and were intimately mixed with the fibrous processes.

OBSERVATION III.—*Scrofulous Disease of the Wrist and adjacent Joints—extensive Disease of all the Cartilages, and complete removal of many—Amputation—Phthisis.*

James Finlay, æt. 19, labourer, admitted into the Aberdeen Royal Infirmary under Dr Keith, on December 26th, 1848, for disease of the wrist. He has resided in Aberdeen for three years past—is of thin and spare habit—complexion pallid.

States that he always enjoyed good health until lately—that eight weeks before admission he injured his right wrist by swinging between two casks, and felt pain in it at the time. He continued at his work, as a shop porter, for some weeks, his wrist being at first a little swollen, and then so painful as to oblige him to give up his work. At this time, the pain was deeply seated—of an aching character, and always worst at night; in the morning the wrist felt stiff. Two or three weeks after the injury he placed his arm in a sling, and used poultices, without relief. When five weeks had elapsed, he applied to Dr Laing as an out-patient of the hospital. At this time there was a swelling on the anterior and inner aspect of the wrist, which, on being opened, discharged two or three table spoonsful of pus and blood; another opening was made lower down, but scarcely any pus escaped from it. These openings continued to discharge until his admission, the fluid becoming gradually more serous. A week previous to his admission, a small abscess, on the back of the carpus, burst, and discharged some pus and blood. His father is a stout and healthy man, but he knows nothing of his mother or other relatives.

On admission, there are the three openings above-named, leading to extensive sinuses; and from the posterior one, portions of rough bone are felt. The whole wrist is much swollen, and white in colour—the hand swollen and almost useless.

A week after admission, Dr Keith removed parts of the unciform and fifth metacarpal bones. Erysipelas came on, and extended to the axilla. It was stopped by the application of nitrate of silver, and in a week or ten days, abscesses formed on the outside of the limb near the shoulder and elbow, as well as in the palm. These were opened, and discharged a large quantity of matter. Emaciation and hectic supervened, and, in consequence, the arm was amputated near the wrist on March 3rd. The wound healed tolerably well—the ligatures separating within ten days, and his health improved very much; but since leaving the hospital he has suffered severely from an affection of the chest, which has all the characters of phthisis.

Examination of the Hand.—All the tissues about the wrist-joint are infiltrated with a gelatinous effusion, through which the sinuses run in different directions. On opening the wrist-joint, the bones are found to adhere so firmly in many places, that the few isolated patches of cartilage which remain tear off portions of bone and lie on the opposite surface. Where there is no cartilage, the bones are rough and bare. The adjacent surfaces of the radius and ulna are in the same state, but the lower part of the surface of the ulna is covered with cartilage over a great part of its extent. The bones are tolerably firm, and resist attempts to pass needles, or the points of forceps, into them.

In the articulation between the rows of carpal bones scarcely any cartilage remains, and the bones are more or less firmly united. Part of the head of the os magnum has broken away and adheres to the scaphoid.

The bones in the upper row separate readily, and are still covered with cartilage on their opposed surfaces; those in the lower row also separate easily; the cartilage of their opposed surfaces is removed, and in its place there is a soft fibrinous mass. The carpal end of the fifth metacarpal bone, and the lower part of the unciform, have been removed, and a mass of granulations covers their ends.

The carpo-metacarpal articulations open easily. The articular surface of the trapezium for the metacarpal bone is covered with cartilage, and appears healthy. Scarcely a trace of cartilage exists on any other bone, but the articular surfaces are covered with a soft granular exudation. The metacarpal bones are bare where they are opposed to each other, except the adjacent sides of the third and fourth, which are covered with cartilage. The inter-articular ligaments are entire and still very firm.

Microscopic Examination.—The cartilage on the lower articular surface of the radius (Fig. XIII.) presents a series of rounded cells $\frac{1}{1700}$th to $\frac{1}{1400}$th of an inch in diameter, which lie in rounded clusters of the diameter of about $\frac{1}{700}$th of an inch, and are in some places bound together by a mother cell; in others, they are much further apart. They contain a number of irregular granules of various sizes, as represented below. In other places, there are large rounded patches, $\frac{1}{230}$th of an inch long and $\frac{1}{280}$th broad, granular in appearance, and presenting traces of former nuclei. There are all sizes of the patches below the measurements given. Besides these, there are rounded cells of the sizes of the patches, containing a number of irregular and glistening corpuscles measuring $\frac{1}{2360}$th to $\frac{1}{1420}$th of an inch. Some of these corpuscles contain a number of granules. Many of the sections taken from these parts present a remarkably beautiful appearance, showing, at one part, small rounded cells with granules in their interior; further on, similar cells considerably enlarged, but still containing a few small granules; and, nearer to the surface, a further enlargement occurs—the granules disappear—the cells become mottled, and many are seen to have burst and discharged their contents. These changes are shown in the section represented in Fig. XIV.

Fig. XIII.

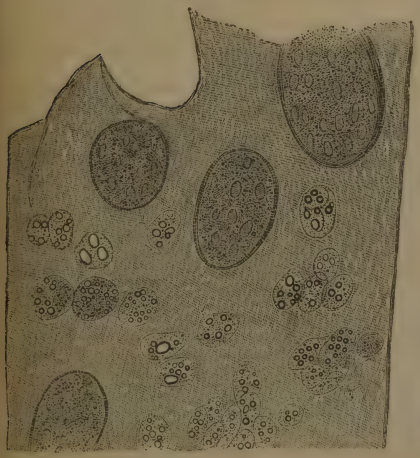
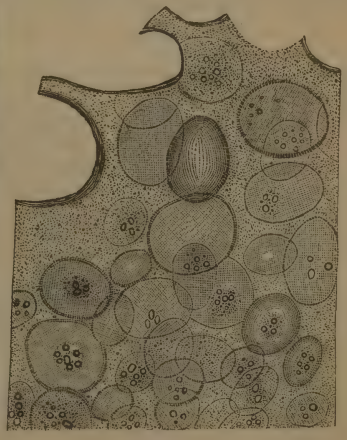


Fig. XIV.



Figs. XIII. and XIV.—*Oblique sections of cartilage from the radius and semilunar bones, showing,* Fig. XIII., *clusters of cells, with granules and granular patches; in* Fig. XIV., *the distension of the cells, and the destruction of their granules as they come to the surface.*

A small patch of cartilage on the radial surface of the semilunar bone, presents, in the middle of its thickness, a number of cells which are evidently breaking up. Their average size is $\frac{1}{300}$th of an inch by $\frac{1}{550}$th. They are full of glistening and slightly granular corpuscles, which are rounded, oblong, or semilunar in their shape. On the surface, there is a finely mottled hyaline substance, full of corpuscles, like those in the cells, but larger in size, measuring from $\frac{1}{2500}$th to $\frac{1}{1400}$th of an inch in diameter. These are so large and closely applied to each other in some parts, as to give to the texture the appearance of a portion of temporary cartilage. At the part of this cartilage which is applied upon the bone, there are a number of nucleated cells spread out in a granular substance, as shown in Fig. XV. These are probably produced by the development of the corpuscles before-named into perfect cells.

Sections taken vertically from the cartilage on the unciform surface of the semilunar bone, show at the deepest parts (Fig. XVI.) irregular cells, having

Fig. XV.

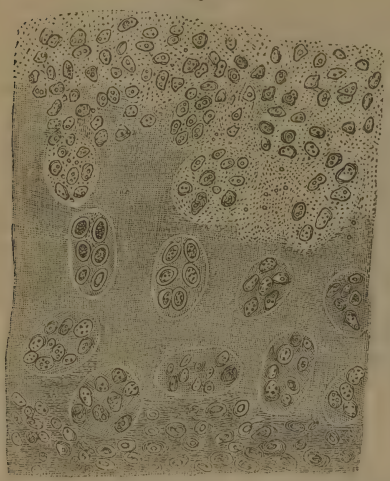


Fig. XVI.

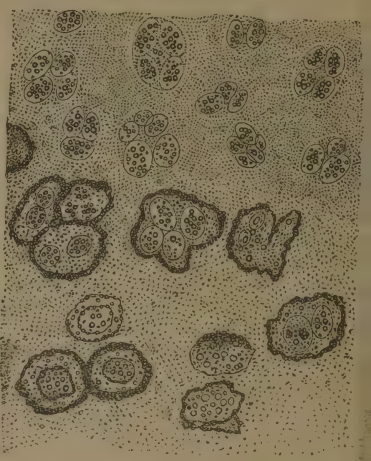


Fig. XV.—*Vertical section from the radial surface of the semilunar bone, showing the cells nearest to the bone at the lower part.*

Fig. XVI.—*Deepest part of a vertical section from the cuneiform surface of semilunar, showing the cells with thickened walls.*

large granular nuclei, and measuring about $\frac{1}{400}$th of an inch in diameter. The walls of these cells and of their nuclei are much thickened, and will not allow the light to pass through until after the action of hydro-chloric acid. In the middle of the sections there are cells or clusters of cells, of sizes from $\frac{1}{700}$th to $\frac{1}{1400}$th of an inch, containing irregular and differently sized granules in some places, and the ordinary corpuscular nuclei in others. On the surface, the cells are distended, and contain a few granules in a cluster in their centre, and some are opened up by bursting. Such cells measure as much as $\frac{1}{230}$th of an inch in their greatest diameter, and are represented in Fig. XIV.

At the anterior and posterior limits of the radial surface of the ulna there are portions of cartilage, the hyaline substance of which is converted into dark fibrous tissue, mixed with cells $\frac{1}{1400}$th of an inch in diameter, each containing four or five granules. Sections may be obtained having fibrous tissue and granular cells at one part; at another, similar cells almost free from granules, and lying in unchanged hyaline substance; and again, such cells in groups, as in Fig. XV.

Every other articular surface was examined with the greatest care, and similar changes were found to have taken place in every one. The soft lymph,

covering the end of the fifth metacarpal bone and of the unciform, is full of remarkably beautiful fusiform nucleated cells and blood-vessels. The fibrinous-looking mass covering many of the surfaces from which the cartilage has been removed, consists of fusiform cells splitting up into fibres, and plastic cells (Bennett). Where the mass is more distinctly fibrous, it is mixed with corpuscles, which are precisely similar to those found in other parts, as contents of cartilage cells. It may be noticed, that though the surface of the trapezium for the first metacarpal bone appears healthy to the naked eye, the cells on the surface are opened out, as is represented in Fig. XI. They are very large, some contain granules, and others are empty or contain a shrivelled nucleus. The deep cells of such parts are enlarged and full of the usual corpuscles.

Remarks.—This disease was the result of injury occurring in a scrofulous constitution. Large portions of the articular cartilages of all the joints about the wrist were removed, and anchylosis had commenced, but the removal of the limb was necessary to save the patient's life after the attack of erysipelas. The microscopic examination showed that the cartilage had suffered from similar changes to those in the former cases, though, in this case, they had been more rapid in their progress. The appearances of sections, in many parts, exhibit such remarkable changes in the characters of the cells in different periods of their existence, as to prevent their recognition without tracing them from one stage to another. In very few places was the hyaline substance converted into fibres, owing probably to the rapid molecular disintegration going on in it and in the cells. On one articular surface, the disease was evidently going on in both the superficial and deep parts at the same time. The corpuscles set free and enlarged, give to the texture the appearance of temporary cartilage, and when this is considered, together with the fact of the thickening of the deep cells, it is important to inquire whether the calcareous matter in anchylosis may not be occasionally deposited in portions of articular cartilage so changed, as it is in the primary formation of bone.

Observation IV.—*Old Disease and Anchylosis of the Bones of left Tarsus—Extensive Disease of the Cartilages of both Knee-Joints—no Disease in the Hip or Ankle Joints—Death from Fever.*

The patient, A. F., æt. 68, died of fever in the Royal Infirmary on February 14, 1849, and was brought to the anatomical rooms. The body was that of a strong muscular man—the second toe of the left foot had been removed by operation. His history has been collected from his former medical attendant, and from persons who have known him for fifteen or twenty years.

He was employed as a shop porter, and when so engaged, ten or twelve years ago, he thrust a nail into the plantar surface of the left foot, by treading on part of an old cask; suppuration and caries followed; he was confined to the house for twelve months; the second toe and portions of several other bones of the foot were removed, and the wound healed up. Not being able to carry on his usual occupation, he sat at a stall and sold fruit for some time, and then resumed his occupation as a common porter, and continued it until four days previous to his death. He was generally known, partly from his dissolute ha-

bits, and from his peculiar gait; for, in walking, he made little use of his knees, but threw the whole weight of his body alternately on the lower extremities, making progression by a sort of rocking motion from side to side. This peculiarity occurred after the injury to his foot, but did not interfere with his occupation, for he could carry very heavy burdens. He never suffered from recognised disease of the knee-joints, nor was he ever known to complain of them.

Examination of right Knee-Joint.—The patella is of the usual shape, and its articular surface is covered with cartilage throughout. This presents a granular and velvetty appearance over a large slightly depressed patch in the centre. The cartilage nearest to the circumference and one-fourth of an inch in breadth all round, is of the usual density, but is split by grooves running towards the centre. In different parts there are fibrous processes hanging loosely from the surface, and of one-eighth to one-fourth of an inch in length. A horizontal section was made through the middle of the bone. The cartilage is one-tenth of an inch thick at the most prominent part of the section, and one-seventh at the middle of the surface for the external condyle. The parts next to the bone, and the bone itself, appear healthy.

Right Femur.—The cartilage on the trochlear surface is velvetty and soft in the centre;—for one-fourth to three-fourths of an inch at the circumference it is dense, white, and split on the surface by grooves passing towards the centre. Where the trochlear surface joins the external condyloid one, the surface is remarkably irregular; in some places elevated, in others depressed, the cartilage being dense and white in patches—then flocculent, soft, and covered by a multitude of projecting fibrous processes. The depressed parts have a puckered appearance at their margins, the bone being covered at the bottom by fibrous and translucent tissue. Further down, and directly on the lower and posterior part of the surface of the external condyle, is a large and irregular patch of 1¼th inch in extent from before backwards, and from one-fourth to one-half of an inch in different places from side to side. On this patch, the bone is thinly covered by translucent fibrous tissue. The patch is separated from the edge of the surface by apparently healthy cartilage of one-fourth of an inch in extent on the outside, and somewhat less on the inside; behind, it runs within one-eighth of an inch of the edge of the surface, and is separated from it by cartilage which is velvetty and granular on the surface. On making a section from before backwards, passing through the trochlear surface and the depressed patch on the external condyle, the bone presents a perfectly smooth edge at every point, except in front of the patch on the condyle, where it is raised $\frac{1}{30}$th of an inch for an extent of one-fifth of an inch from before backwards. The cartilage is $\frac{1}{10}$th of an inch thick on the trochlear surface, and becomes gradually thinner on passing to the junction with the condyloid surface, where for more than one inch it is only $\frac{1}{50}$th of an inch thick. It then regains its thickness for half an inch immediately in front of the raised portion of bone, and for one-fourth of an inch behind it; whilst on the remaining part of the section, corresponding to the bottom of the depressed patch, there is only a thin membrane.

Proceeding backwards from the trochlea on the surface of the internal condyle, the junction of the two presents similar irregularities and fibrous processes, on the surface, to those on the opposite side. At one of such points the bone is raised, and it is bare at the apex of a small projection of one-eighth of an inch in diameter; posteriorly, the cartilage has a smooth and nearly healthy appearance, except on the outer edge of the condyloid surface, where it is granular and flocculent; and behind there is a large patch, on which the cartilage is rough, white, and presents a number of fibrous processes hanging loosely into the joint. On making a section from before backwards, through the left half of the trochlear surface and left condyle, the cartilage on the former appears healthy, and is one-tenth of an inch thick; at the junction of the two, and over a space of one-third of an inch from before backwards, the bone is

raised above the level of the cartilage in a circular spot, and is bare or covered with a thin membrane ; behind this, the cartilage is at first thin, but gradually increases in thickness, until at about the middle of the condyloid surface it is $\frac{1}{15}$th of an inch thick, again becoming thinner in proceeding backwards.

Right Tibia.—The cartilage of the internal glenoid cavity is apparently healthy behind, but soft and velvetty in front. One-fourth of an inch distant from its anterior part, there is a spot of the size of a split pea, in the centre of which the bone is bare, the cartilage adjacent presenting a number of fibrous processes. The cartilage on the external glenoid surface appears healthy, except in front and internally, near the attachment of the anterior extremity of the semilunar cartilage, where the surface is raised about $\frac{1}{15}$th of an inch over a rounded spot of the size of a split pea; and again, at the posterior part, where there is a depression which would hold half a split pea. At the bottom of this, there is a semi-transparent membrane with a puckered appearance on the surface; near this spot, there is a slight velvetty appearance on the cartilage, as well as on the semilunar cartilage. Near the depression, there are three distinct fissures, each of about half an inch in length, and with a stellate arrangement.

The semilunar cartilages are flocculent on their thin edges, and the external one is of like character on its surface, at the part corresponding to the depression. In other parts their surfaces appear healthy.

Microscopic Examination.—The fibrous processes on the patella, one-eighth to one-fourth of an inch long, consist of large and small, flat, and glistening bands, which are split up at their edges and free extremities into fibres of different sizes. Some of the bands are finely striated, many have large masses of hyaline substance interrupting their continuity, or stuck upon them, as represented in Fig. X. At different distances on the bands, there are perfectly regular oblong cells of different sizes, usually $\frac{1}{710}$th of an inch by $\frac{1}{900}$th. They contain a small finely granular nucleus, or a larger and more distinctly granular one. The cells are in considerable quantity only at those parts where the bands show but little tendency to split; where there is a distinct splitting, the cells are far apart, appearing to be destroyed by the splitting, or converted into the glistening masses on the fibres. At all the deep parts of the section, the cells and hyaline substance are healthy; but, on advancing towards the surface the cells become rounded, much enlarged, and filled with corpuscles, which are discharged upon the surface or mixed with the tissue. At the free edge, the hyaline substance breaks up into bands and fibres at a very definite point, and there are found in the bands the large nucleated cells before-named and figured in Fig. VI., or nucleoli spread out in the tissue, and amongst the fibres a multitude of small, elongated granular cells, as shown in Fig. VII. The dense and white portions at the circumference show traces of the superficial cells opened out; but, in most parts, the cartilage is converted into fibrous tissue, of which the fibres run horizontally. All stages of this transition can be seen in different parts ; whilst in all, there is a notched and very definite margin to the less changed hyaline substance, the notches being the result of the opening of cells on the surface.—(Fig. XVII.)

On the femur, the projecting processes are of similar characters to those on the patella; many containing large and bright nucleated cells ; others are striated and split at their edges and free extremities into fibres undistinguishable from ordinary fibrous tissue. Such bands contain numerous corpuscles in many places. These measure $\frac{1}{2300}$th by $\frac{1}{3500}$th of an inch. They are granular, in some places; in others, surrounded by an indistinct cell wall, and they are not to be found where a complete conversion into fibres has taken place.

The cartilage, exposed by both sections through the articular surface of the femur, is healthy in one or two small spots only ; in other parts, the deep cells are enlarged, measuring, in many places, $\frac{1}{280}$th by $\frac{1}{350}$th of an inch, and still nearer the surface, $\frac{1}{200}$th by $\frac{1}{250}$th of an inch. They are full of corpuscles. Similar corpuscles are seen in patches, in other parts, as shown in Fig. XV.

The horizontal layer of cells is very thick in some situations; its cells are so much enlarged as to measure $\frac{1}{220}$th of an inch long, by $\frac{1}{1660}$th broad;—they contain a central granular nucleus, and some are granular throughout;—at the surface, they can be seen bursting and forming a granular and fibrous mass, in which nuclei can be found elongating. (See Fig. XVIII.) In other parts, indeed

Fig. XVII.

Fig. XVIII.

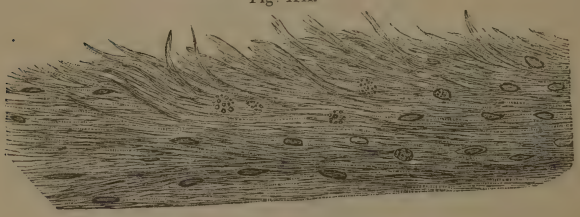
Fig. XVII.—*Vertical section near the circumference of the right patella, showing the notches produced by the bursting of cells, and the fibrous and granular mass on the free surface.*

Fig. XVIII.—*Vertical section of the cartilage of the femur, showing the superficial cells enlarged, granular, and bursting. The upper part of the figure shows the granular and fibrous mass on the surface.*

over a large portion of the surface, some of the cells, usually lying horizontally, exist; but the surface is covered with a fringe of fibres $\frac{1}{300}$th of an inch long, which is evidently the result of the splitting of hyaline substance. The thin semi-transparent membrane, covering the bottom of the depressions before de-

Fig. XIX.

Fig. XX.



Figs. XXI. and XX.—*Projecting processes from the patella and femur.*

scribed, consists of dark fibrous tissue, the fibres of which run horizontally, and yet form a short fringe on the surface by their loose extremities. Acetic acid renders the fibres transparent, and brings out a series of nuclei, running in the direction of the fibres, as shown in Fig. XXI. and XXII. Where this tissue joins with the cartilage, the discharged corpuscles of its cells can be seen, in all stages of elongation, to become mixed with the fibres, or actually converted into them. The elongation can be observed in almost every preparation (Fig. XXIII.); and there seems the best reason for believing that the nuclei are the changed corpuscles of the cells. The raised portions of bone present nothing remarkable.

Fig. XXI. Fig. XXII. Fig. XXIII.

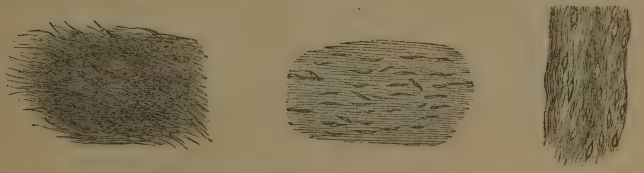


Fig. XXI.—*Fibrous tissue from the surface of the cartilage.*
Fig. XXII.—*The same, acted on by acetic acid.*
Fig. XXIII.—*Fibrous mass, with corpuscles elongating from the surface of the left femur.*

The horizontal layer of cells, from the central part of the internal glenoid surface of the tibia, has been removed, and the surface presents a number of excessively transparent hyaline bands of all sizes, projecting free into the cavity. Where these are attached to the hyaline substance, there are elongated, open spaces, indicating the original position of cells, which have caused the splitting. —Fig. XXIV. Still deeper, are large nucleated cells, or cells crowded with corpuscles; whilst, near to the bone, the cells are of their usual form and size, and the hyaline substance is healthy. Near the edges of the surface, and where the cartilage is still dense, the horizontal layer of cells is still to be seen, but its most superficial cells are enlarged and separated at a considerably increased distance from each other (Fig XXV.), the deeper parts being healthy. The cartilage around the spot of bone which is bare, is split in the usual way into fibres and hyaline bands.

Fig. XXIV. Fig. XXV.

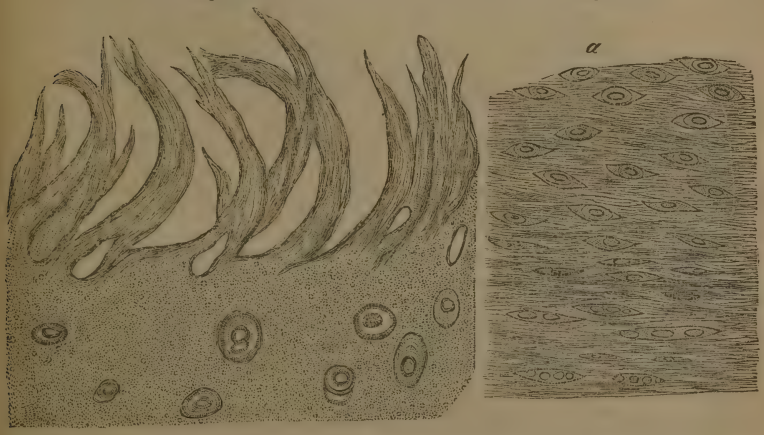


Fig. XXIV.—*Vertical section from the cartilage of the central part of the internal glenoid cavity of the tibia, showing the splitting into fibres on the surface.*
Fig. XXV.—*Changes in the superifical cells, near the edge of the internal glenoid cavity; a, free surface.*

The cartilage of the external glenoid cavity presents, in different parts, the various appearances before described. On the edges of the fissures, there is a loose fibrous mass, the cells in the neighbourhood being filled with corpuscles, and some of them $\frac{1}{100}$th of an inch in diameter. The raised portion, in front, is covered by a thin fibrous membrane, and under this are several small masses of dense calcareous matter, in the very substance of the cartilage. Below these, the cartilage is but little changed. The calcareous matter dissolves slowly, and with effervescence, in acetic acid. The solution gives no precipitate with ammonia, nor with ammonia and sulphate of magnesia, but when solution of oxalate of ammonia is added, a dense white precipitate of octohedral crystals immediately takes place. The crystals are insoluble in acetic acid, and have all the characters of oxalate of lime. The substance consists of carbonate of lime.

The flocculent appearance of the semilunar cartilages is produced by fibres and bands, which cannot be distinguished from those obtained from the proper cartilages—(Fig XXVI). Like the latter, these contain, at some distance from their free extremities, rounded nucleated cells—(Fig. XXVII). In many

Fig XXVI.

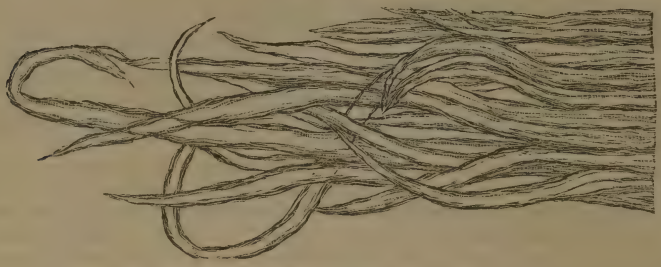
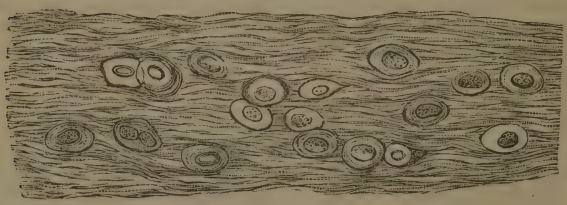


Fig. XXVII.



Figs. XXVI. and XXVII.—*Two portions of the same fibrous projection from the free edge of the semilunar cartilage, where it is flocculent.*

parts, the walls of such cells become gradually indistinct; and at last, nothing but their nuclei can be observed (Fig. XXVIII.), these being spread out in the tissue. The deeper and more external parts consist of an intricate interlacement of fibres, with an occasional nucleated cell or a corpuscle.

Many of the fibrous processes present masses of hyaline substance attached to them, as in Fig. X.

Examination of the Left Knee-joint.—The cartilage of the patella is velvetty and soft on the surface, owing to fibrous processes such as have been described. Amongst them are numbers of the octohedral crystals before-named. The superficial and deep cells have undergone the usual enlargement, and then present a granular nucleus, or a series of corpuscles, in their interior.

The articular surface of the femur is covered with cartilage throughout. On the external condyle this is but little changed; on the internal one, the cartilage is slightly velvetty, and on its external part, there is an abraded and vel-

Fig. XXVIII.

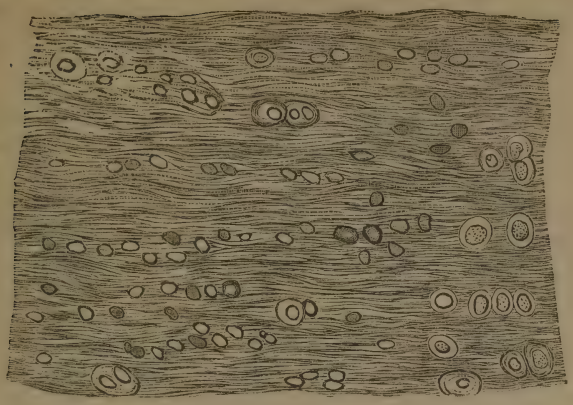


Fig. XXVIII.—*Section of the semilunar cartilage near the internal edge, deeper than the fibrous processes.*

vetty patch, three-fourths of an inch long and one-third broad. The trochlear surface has a similar patch above and outside. The changes of the cells, and the splitting of the hyaline substance, are of the usual kinds in all the diseased parts.

The external glenoid cavity of the tibia presents a large oblong patch of velvetty cartilage internally and posteriorly; but this does not extend to the edge;—the other parts are healthy. The diseased portion has lost its superficial cells; the deeper ones are enlarged, distended, and some have discharged their corpuscles into the fibrous mass on the surface, whilst the parts next to the bone are healthy. On all parts of this surface there are octohedral crystals, insoluble in acetic acid; and here and there a rhombic and imperfect crystal, soluble in the same acid. The cartilage of the internal glenoid cavity is healthy near the circumference, but similarly diseased to the external one at the central part.

The semilunar cartilages have in a few places undergone similar changes to the opposite ones.[1]

Remarks.—This was an instance of extensive disease on every articular surface of both knee-joints, occurring in an individual who carried the heaviest burdens, without inconvenience, up to four days previous to his death. The disease affected every cartilage in the right knee-joint to a greater extent than those in the left one, owing probably to more weight having been thrown upon the limb of that side, as the result of the previous disease of the left foot. Everywhere the disease commenced on the surface, and at the central parts of the cartilages. On all the surfaces, the hyaline substance was split into bands and fibres, one cause of which appears to have

[1] I have to express my warmest thanks to the medical officers of the Royal Infirmary for their kindness in affording me every facility in the examination of the cases under their care on all occasions. It is right to state that I alone am responsible for the selection and statement of the facts.

been the active changes taking place in the cells of the cartilage. The free fibres, when formed from the superficial parts, lie horizontally; but when from the deeper parts, they have a vertical arrangement, evidently due to the normal arrangement of the cells. That the cells burst and discharge their contents into the tissue, there can be no doubt; but it is not quite so clear what part these contents take in the formation of the fibrous mass on the surface. The corpuscles of the cells certainly elongate before they are discharged, and to a greater extent afterwards; but, as it is positively shown that the hyaline substance can be converted into fibres as small as those of white fibrous tissue, without the immediate agency of cells,—that part, at least, of the fibrous tissue on the surface of diseased cartilages is so formed; and that the corpuscles appear like nuclei in the tissue, after the action of acetic acid, we cannot believe that they actually elongate and constitute fibres. And yet, fibres are formed in the granular mass, without the developement of other and new cells.

(*To be continued.*)

Part Second.

REVIEWS.

Lectures on the Diseases of Infancy and Childhood. By CHARLES WEST, M.D., Fellow of the Royal College of Physicians; Senior Physician to the Royal Infirmary for Children; Physician Accoucheur to the Middlesex Hospital; and Lecturer on Midwifery at Saint Bartholomew's Hospital. London. 1848. 8vo. Pp. 488.—(*Continued from* p. 844.)

THE diseases of the *Nervous System* are considered in Lectures XI. and XII. Dr West remarks that, to the usual obscurity surrounding affections of this class in adult life, special difficulties of investigation are added in the case of children; because in early life locomotive power is not yet acquired, or, if so, may readily be lost again, in the course of ordinary ailments; and further, because children are not able to describe at all, or at least distinctly, the various impairments of sensation which may exist in different parts of the body, or the peculiarities of pain which may be present.

The subjects of *Irritation*, *Congestion*, and *Inflammation of the Substance and Membranes of the Spinal Cord*, are illustrated by a series of very interesting cases, on which, however, we cannot stop to remark. *Trismus* is then considered, but its rarity in this country, and the imperfect evidence in favour of any plan of treatment

hitherto suggested, may excuse us in passing on to the next affection noticed by Dr West, namely, *Night Terrors*. The occurrence sometimes happens thus :—

"A child who has gone to bed apparently well, and has slept soundly for a short time, awakes suddenly in great terror, and with a loud and piercing cry. The child will be found sitting up in its bed, crying out as if in an agony of fear, 'Oh dear! Oh dear! take it away! father! mother!' while terror is depicted on its countenance, and it does not recognise its parents (who, alarmed by the shriek, have come into its room), but seems wholly occupied with the fearful impression that has aroused it from sleep. By degrees consciousness returns, the child now clings to its mother or nurse, sometimes wants to be taken up and carried about the room, and by degrees, sometimes in ten minutes, sometimes in half-an-hour, it grows quiet, and again falls asleep. As the terror abates, the child in some instances grows quiet at once, but frequently it bursts into a fit of passionate weeping, and sobs itself to rest in its mother's arms. In some instances a quantity of limpid urine is voided as the fit passes off, but this occurrence is by no means constant. Usually, the remainder of the night is passed in tolerably sound sleep, and the following night the child may rest quite undisturbed, or the terrors may again return, and with precisely the same symptoms as before. The attack usually comes on after the child has been from half-an-hour to a couple of hours asleep, and two attacks do not generally occur in the same night. They are always more or less distinctly associated with the impression of some object which occasions alarm,—as a cat or dog, which is fancied to be on the bed; and this illusion continues even after the child has recognised those around it. The condition is not one of delirium, for the child has no other hallucination; but the attack may return night after night with precisely the same characters."—P. 128.

These nocturnal alarms come on under a variety of circumstances; they often recur for weeks, and occasion apprehension of an attack of hydrocephalus. Although at first there is no fear of this, it is well known that serious disease may ultimately result from long-continued irritation of the nervous centres. If diarrhœa exists, it must be checked, and irritation allayed, by the cautious administration of opiates and sedatives; if constipation, by a gentle aperient combined with a tonic. Diet, too, must also be carefully attended to. Dr West likewise gives the following judicious caution, which it would be well if those stern nursery disciplinarians who attempt to bring up young children on what is termed the "hardy plan," would consider well.

"At the same time, too, the child should not be left in the dark, or alone; the affection resembles night-mare; and in childhood dream-images seem to mingle with the waking impressions much more than in adult age. A light burning brightly in the room, and a familiar face meeting the child's eye at once on waking, will do much towards breaking the spell, and towards allaying its fears. Harshness in such cases is quite out of place, and few pieces of cruelty can be greater than forcing a timid child, in whom threatenings of these attacks have recurred, to go to bed in the dark, or to lie there without a candle, while its active imagination conjures up before its eyes, out of the bed-curtains, or other objects in the room, the outlines of all sorts of terrific forms."—P. 131.

Epilepsy.—At the commencement of the lecture in which this affection is introduced, Dr West says concerning it, night terrors,

chorea, and palsy:—"They are affections of the nervous system which are often independent of any change in the nervous centres, such as our means of anatomical investigation can discover, or which are but the expression of the sympathy of the brain and spinal cord with the sufferings of some distant organ."—P. 128. To this statement we think that he ought to have added, as regarded epilepsy, that sometimes the brain, and sometimes the spinal cord, is affected primarily; and that the influence, of whatever kind it be, is then propagated either downwards or upwards, producing consentaneously convulsions, and loss of consciousness. The notice of epilepsy is altogether the most unsatisfactory portion of Dr West's work. Its pathology is scarcely adverted to; its frequency is, we think, under-rated; its symptoms not described; and, although its importance is acknowledged, not a hint is given as to treatment. We have often met with cases which appeared to us to be epilepsy in its earliest manifestations, or at least more closely resembling it than any of the other forms of convulsive disorder in the young subject, and which are apt in after-life to lay the foundation for that fearful malady. The form of disease to which we allude occurs in children generally from two to eight months old—when the incisor teeth are rising in the gums. They inherit the strumous diathesis; they have clear pale complexions, and pale fine textured skins; they are lively—"nervous" in the common acceptation of the term; and, while they are not robust in constitution, they are by no means weakly children. The convulsive seizure may almost always be traceable to intestinal irritation, either from, or in consequence of, injudicious artificial feeding, or from disorder communicated through the nurse, and more especially during the period of teething. In general it comes on suddenly—without any marked premonitory sign. It commonly lasts from two to ten minutes, sometimes longer, and almost always recurs a few times, at intervals of hours, days, or weeks. In some instances we have found the convulsions supplanted by fits of laryngismus stridulus. The seizure is seldom witnessed by the physician, on account of its short duration; but it appears that the child is totally unconscious; the face is turgid, livid, or pale with blueness around the mouth; the eyes are turned up within the upper eyelids; there are spasmodic twitchings at angles of eyes and mouth; frothing at the mouth, or rasping of the teeth; and the body is bent backwards, and the extremities affected with carpo-pedal contractions. When the child is visited shortly after the fit, we find no unusual heat of head, fulness of veins of scalp, prominence of fontanelle, or flushing of face; and the pulse, although quicker than natural, has no peculiar strength or fulness. The little sufferer has at first only a wearied, sleepy aspect, and yawns occasionally; but, in general, soon looks as happy and cheerful as if nothing had happened. The gums are sometimes swollen and hot, the tongue is almost always loaded, and the evacuations are greenish and slimy. In cases like these the young practitioner is apt to be undecided as to

the practice he should adopt. In general, were he to use the lancet or leeches, unless several fits had occurred in rapid succession, and occasioned manifest embarrassment in the circulation of the brain, or tendency to inflammatory action, it is probable that irreparable mischief would result; neither are these the circumstances which call for cordials. During the invasion, the warm bath, in which a little mustard or turpentine is diffused, moderate cold to the head, and the free use of the gum lancet, if there is the least suspicion of a tooth approaching, are usually the chief measures to be employed. Immediately after the convulsion is over, mercury with chalk and Dover's powder, or an opiate enema, should be given if diarrhœa exists; or if there is constipation, an enema composed of castor oil, turpentine, and infusion of anise; and this last measure will be the more necessary if worms are the cause of the intestinal irritation. Attention also should be given to the quality of the mammary secretion and the other food supplied to the child; and exposure to the open air, or removal into the country, are among the best means for the restoration of health, and for the prevention of another attack. Bismuth we have found the best and safest of the metallic tonics. The affection above described must be recognised by those engaged much in practice among children; and if such attacks are not decidedly epileptic, we think it highly probable that they originate the peculiar erethism which in after-life assumes the unmistakeable character of epilepsy. We have frequently made inquiry at adult epileptics, if in infancy they had suffered from convulsions, and have received an answer in the affirmative. The importance, therefore, of counteracting this tendency at the outset of existence must be apparent; and the length to which these remarks have been extended, is, we think, justified by the omission in the work before us.

Before leaving this subject, we would draw attention to the circumstance already noticed, that fits of crowing inspiration sometimes occur in the course of that form of convulsive disorder which we have considered to be of the nature of infantile epilepsy. And bearing in mind the statement made by Dr West at the commencement of the twelfth lecture (which has been already quoted), we choose this opportunity of questioning the pathological propriety of dissociating—as is now-a-days done—*spasm of the glottis* from affections of the nervous centres, and classifying it among the diseases of the respiratory organs. Some of the older authors have called it *cerebral croup*, and all admit its intimate connection with the functions of the nervous system. We do not refer to that kind of spasm which arises from injury or irritation of the laryngeal recurrents themselves, in consequence of local inflammation, or the pressure of tumours or enlarged glands, but which originates from primary affection of the brain or spinal cord, or from disturbance of some remote organ or general function—whereby certain impressions are transmitted to the nervous centres, sympathised with, and in consequence manifested

in this local spasmodic seizure. We look upon the selection of the larynx for the exhibition of the spasm in such instances as incidental. It may probably be directed there in preference to the fingers and toes or other parts, on account of the delicate sympathies existing between the respiratory and nervous systems. Carpo-pedal contractions, however, are very common attendants of laryngismus stridulus, and this proves its more general relations to, and dependence on, affection of the nervous centres; and moreover, these attacks of crowing inspiration are sometimes introduced, often supplanted, and frequently followed, by general convulsions. We see no more reason, therefore, for signalising this affection of the larynx as a special disease, and for classifying it with affections of the respiratory organs, than for fixing upon the most prominent and invariable symptom of tetanus, namely, locked-jaw, as the term by which that disease is to be designated, and then for arranging it among diseases of the digestive apparatus, because it is the mouth which is implicated. The exciting causes, too, of laryngismus, namely, difficult dentition, digestive derangements, &c., can in no way affect the pathology of the disease, any more than wounds of the hands or feet, or exposure to sudden alternations of temperature, can change our opinions of the nature of trismus (we speak of the disease as it occurs in adults), which is only a part—a symptom—of a general disorder of the nervous system, arising from some peculiar condition of the spinal cord.

The remaining diseases in this part of Dr West's course, are *Chorea* and *Paralysis*. The former affection is treated much too shortly; and we are astonished that no mention whatever is made of the use of arsenic in it as a means of cure. The potency of this agent is now well established; and we can attest confidently the safety of its administration, even to young children. Paralysis is discussed much to our liking.

It would be agreeable to follow our author in his observations on the *Diseases of the Respiratory and Circulatory organs*, which extend through the next fourteen lectures. In this part of the course, perhaps, Dr West's strength chiefly lies. There are in it evident marks of high powers of observation, intimate knowledge of modern pathology, and orthodoxy of opinions regarding it. At the same time, he always appears as a sensible discriminating practitioner. As we have, however, nearly exhausted the limits permitted us, we cannot enter on a subject so extensive. We pass on, therefore, to notice some of the topics in the third department of the course, namely, the diseases of the organs of digestion and assimilation. We make this selection for the remainder of our review, as the derangements of these organs are of great practical importance, and have an extensive bearing on the class of cerebro-spinal affections which we have been considering.

Lecture XXVII. is entirely occupied with observations of an introductory nature to the examination of the *Diseases of the organs of*

Digestion and Assimilation. Dr West gives a very clear and concise account of the peculiarities of the digestive apparatus in childhood, and its adaptation for the rapid performance of functions; and remarks at length on the special suitability of milk to the purposes of infant nutrition. We are glad that he speaks out so strongly regarding the moral duty of a mother attempting to suckle her own child, and the impropriety of the endeavour, unless for very good reasons, to bring up any young infant by the hand, since that deprives it of the food specially intended by nature for its support, and the solace which accompanies the mode of obtaining it. Such deprivation is a fertile source of disease, as the statistics of foundling hospitals, and the private experience of every physician, may amply prove.

"But the infant is to be trained to become a man: its moral as well as its physical nature is to be cultivated; parental influence is to be the means of doing this; and Providence may have wisely determined that the infant shall for months be dependent on its mother for support, in order that her instinctive feelings may lay the firm foundation of that love which causes her to cling to her little one with a fondness that surpasses all other affections, and which gives her the patience, the gentleness, the untiring energy, that makes her the child's best guardian, friend, and teacher, during its earlier years.

"But whether it be right or wrong to seek in something higher than the material for the reasons of this physical law, it yet is a law, and one which cannot be violated with impunity. The infant whose mother refuses to perform towards it a mother's part, or who, by accident, disease, or death, is deprived of the food which nature destined for it, too often languishes and dies. Such children you may often see with no fat to give plumpness to their limbs —no red particles in their blood to impart a healthy hue to their skin—their face wearing in infancy the lineaments of age—their voice a constant wail—their whole aspect an embodiment of woe."—Pp. 330, 331.

We are glad, also, that Dr West advocates the immediate application of the child to the breast; and warns of the probable danger to young infants from the use of the milk of cows which are stall-fed. Such milk appears to produce more serious consequences than could be accounted for by a merely ascescent condition of the fluid. It is in reality diseased, and as such is unfit for infants' food. Dr West recommends, that when an infant under artificial feeding appears to be in declining health, before prescribing for complaints, which may arise solely from the unwholesomeness of its aliment, inquiry should be made regarding the milk made use of, and that a specimen should be submitted to chemical and microscopical examination, so as to determine its quality. Although, however, he previously stated that good milk should always present an alkaline reaction, he does not mention what are the microscopic characters which will prove it to be diseased. We have repeatedly examined under the microscope the milk of stall-fed cows, and have invariably observed granular bodies and epithelial scales present, and an excessive agglutination of the milk globules—appearances similar to those

found in diseased human milk, and described and delineated in the number of this "Journal" for August last.

In connection with this subject, we would supply a remark which might have occurred to Dr West, that the ailments of children at the breast may very often originate from the diet and regimen of the nurse. Her living is frequently most unnatural, and her habits luxurious and constrained. She is pampered with rich meats and strong drinks, coddled up in heated apartments, deprived of exercise, and the privilege of breathing the fresh air of heaven. Her condition, in fact, is analogous to that of the stall-fed cows; and the supply furnished to her infant cannot but be impure.

Lectures XXVIII. and XXIX. are not so lucid, nor the subjects so well arranged, as they might have been. In the former lecture Dr West treats of *Aphthæ*, then *Dentition* is fully discussed; and in the latter lecture follows the consideration of *stomatitis*, as presented under three distinctive forms, namely—*follicular*, *ulcerative*, and *gangrenous*. Now, whilst we would entirely exclude *cancrum oris* from this association, we consider also that aphthæ and the first and second varieties of stomatitis, are simply modifications of the same affection, originating from similar causes—namely, imperfect or perverted nutrition, and unhealthy season and locality; and that they are only different degrees of an affection in the mucous lining of the mouth, occasioning at one time a simple thickening of the membrane, at another an ulceration, and at another a pseudo-membranous deposit. It appears to us that they should have been considered simply as one affection—differing only in severity, and in the periods at which they respectively occur, — the slightest form, namely *aphthæ* or *thrush*, during the earlier days of existence and lactation, and the aphthous ulceration during childhood. We enter our protest against the tendency of writers of the present day to multiply terms and nosological distinctions which perplex the student, without serving any practical purpose. Surely the term *aphthous ulceration* is infinitely to be preferred to *stomatitis;* and no countenance should be given to the ugly appellation of *muguet*. With regard to the pseudo-membranous, or rather cryptogamic deposit, which takes place in the worst cases of aphthæ, and to which the very un-English word referred to has been applied, we cannot agree with Dr West in thinking that the formation of the aphthophytes in children in foundling hospitals or elsewhere, is "possibly dependent on transplantation of the sporules from one patient to another, by means of the cups and spoons," or that even probably, "the deposit of these sporules upon the surface of a healthy mucous membrane is followed by the development of the confervæ, and the alteration of the epithelium of the mouth."—P. 342. These parasitic growths are clearly owing to an atmospheric decomposition of a surface coated with vitiated mucus; they probably originate from acidity in the primæ viæ. Similar microscopic plants were found by Lebig, Andral, and Gavarret, to occur in albumen sufficiently

diluted, and exposed to the atmosphere under the influence of an acid.[1]

Cancrum Oris,—or what Dr West styles *gangrenous stomatitis*, we think, has little or no relation to aphthous ulceration; and, notwithstanding the opinion of Rilliet and Barthez, we do not consider that it always begins in the mucous membrane. It rather appears to us to originate in the sub-mucous textures, and from thence spreads inwards as well as outwards. It is a rare affection, and almost invariably occurs during convalescence from fever or other disease, which has produced general debility. Dr West describes the disease as always affecting the cheek by external perforation, and takes no notice of the destruction of bone as an occasional result. We have seen, however, three cases in which the cheek did not suffer, but in which the gangrene spread inwards, affecting the soft parts covering the maxilla, and terminating in pretty extensive necrosis. Mercury had no part in producing these results, and we coincide with Dr West, that mercury has in general nothing to do with the affection. We also agree with him in considering that the application of nitric acid is the most powerful means of arresting the progress of this severe malady. In the local treatment of the mildest form of aphthæ, Dr West judiciously recommends a solution of borax in water, and not a mixture with honey, as is the usual practice. We have always been convinced that anything saccharine, and consequently soon to undergo the process of fermentation, favours the formation of the confervæ, or aggravates the ulcers themselves; and for the same reason we are in the habit of prohibiting, as much as possible, food which contains much saccharine matter, and is likely to produce acidity. Dr West applies a solution of the nitrate of silver when aphthous ulcers have not a healing action. We have found a saturated solution of the sulphate of copper answer much better; one application generally produces a marked improvement on the sores. Very little is said by Dr West on the constitutional treatment of aphthous ulceration, apparently because he considers that the chlorate of potash, recommended by Dr Hunt, is quite a specific. The dose advised is three to five grains dissolved in water, every four hours, to children from three to nine years old. It is said to effect a decided cure in eight or ten days. During the first three months of the present year, when the complaint prevailed much in Edinburgh, even among the higher classes, we have found speedy success attend the use of small doses of hydr. cum cretâ with ipecachuan, rhubarb, and soda; lime water, quinine, and carbonate of soda; restriction to a non-saccharine diet, and the use of the copper wash.

We look upon the introduction of *Cynanche Parotidea* in lecture XXIX., as somewhat out of place. The *mumps* seem to have no

[1] Monthly Journal, 1847-8. P. 125.

connection with special derangements of the digestive apparatus. It is an acute inflammation affecting the parotid gland, and quite different from an ordinary strumous swelling of it; in it the state of the salivary secretion is not particularly affected; it is a disease which prevails apparently by epidemic influence, and becomes decidedly contagious, at times spreading in districts, schools, and families,—like measles, scarlet fever, or hooping-cough, and occurs but once in a lifetime. On these accounts we consider it entitled to a place among specific diseases.

Dentition.—Dr West's remarks on the management of teething children are very good. While it is highly proper, however, to warn against viewing every complaint which occurs during the period of dentition as necessarily connected with it, and consequently against undue interference with the gums, his strictures on this subject tend to a much too cautious and scrupulous use of the lancet. The frequency of severe illness, arising solely from the irritation of teething, and the ready aggravation which any ailment suffers when going on at the time of dentition,—are known to every practitioner; and those who have been in the habit of using the scarificator freely over the tooth or teeth—which, according to the age of the child, ought to have been already produced, or are close at hand, will not be deterred from the continuance of the practice by any opinion of its inutility or barbarity. To remove every probable cause of irritation in infantile disease, ought, we think, to be the general rule of practice. Even in some cases, although we cannot positively say that dentition has much to do with the existing affection, if there is any severity in the ailment, it is good practice to act as if it had, and that not by slight scarification or scratches, but by free crucial incisions down to the tooth supposed to be rising, leaving its future progress unopposed. Nay, if a robust child has passed a restless night—awaking with startings and fits of crying, and is feverish, or only irritable beyond usual, and there is a slight redness and swelling over a tooth which, in the ordinary order of irruption, may be expected soon, then we should consider it our duty—an act of humanity, to afford relief at once by the lancet, whatsoever else we may prescribe at the same time; because no one can tell what serious cerebral or nervous affection may supervene on a few hours' longer continuance of irritation.

Infantile Dyspepsia, in all its forms, is very sensibly considered by Dr West in lecture XXXI., and he enforces the importance of giving attention to the early indications of such disorders, because, when allowed to go on unchecked, they lead to impaired nutrition, and a long train of evils. Among other remarks Dr West reprobates the indiscriminate practice of administering mercurials to correct real or fancied disorder of the liver. Simple diarrhœa, with pale light yellow evacuations, he thinks, is best arrested by spare diet, and small doses of sulphate of magnesia, with tincture of rhubarb—such

as five grains of the former, and ten of the latter, several times a-day, to a child a year old. When the evacuations are white and putty-like, mercury is really needed. The hydr. cum cretâ is the best form when the odour is offensive, and this, combined with Dover's powder, when the motions are frequent and acrid.

The subjects of *Softening of the Stomach and Bowels* and *Hæmatemesis*, are discussed in the remaining part of the thirtieth lecture. On the first affection Dr West does not throw more light than his predecessors. All that can be said is, that it is discovered after death in various forms; that during life there are no characteristic symptoms of the affection whereby it may be certainly distinguished; and that there is no remedy for it.

The *icterus* of children receives as much notice in lecture XXXI. as its importance requires. In its ordinary form it is very common, and very unimportant. Dr West mentions the more serious varieties of the disease which depend on congenital absence or closure of the hepatic or cystic ducts from any cause, and he goes on to notice those singular cases of jaundice, in which unrestrainable hemorrhage takes place from the umbilicus, on the separation of the cord. We have seen two instances of this.

From want of space we cannot continue these slight analytical remarks on the remaining subjects embraced in Dr West's lectures, although both highly interesting and important. We can only enumerate them.—*Constipation, Imperforate Anus, Intus-susception, Diarrhœa, Prolapsus Ani, Peritonitis, Tabes Mesenterica, Intestinal Worms, Nephritis, Calculous Disorders, Diabetes, Incontinence of Urine, Abdominal Tumours, Syphilis, Remittent Fever, Small-pox, Chicken-pox, Measles, and Scarlatina.*

Although an English physician, we are glad to find that there is a Scotch simplicity in Dr West's practice. In general, the selection made of infantile therapeutics, and hygienic measures, is quite in accordance with our own notions. From one medicinal application, however, we entirely dissent—namely, the exhibition of mercury in syphilis,—which, he says, is indispensable in one form or other—(P. 451); and we are much astonished that cod-liver oil has not found its way yet into his materia medica. We hope that in the next edition of his work, he may be able to notice its efficacy, at least, in phthisis and in tabes mesenterica. Some of Dr West's favourite infantile medicines we have already noticed, but it may not be unacceptable to our readers to have a few more of those most recommended by him. These are the following:—*Aloes*, as a purgative, on account of its mildness and certainty of action:—given in decoction, concealed in extract of liquorice, or in powder with coarse sugar or treacle. *Sulphate of Copper*, as an emetic in cases where much depression exists: dose—a quarter grain or half a grain dissolved in water every quarter of an hour, until vomiting is produced. *Tincture of Hyosciamus*, as a sedative. *Dover's Powder*, as

the only form almost in which opium is used, and always in a guarded way. *Hydrocyanic Acid*, in paroxysmal cough, sometimes combined with Battley's liq. cinchonæ, if there is much debility. *Syrup of the Iodide of Iron*, and the *Ferro Citrate of Quinine* as tonics; and *Logwood* as a tonic astringent. The suitableness of all these medicines in the treatment of diseases of children, we can amply attest.

With these remarks we close our review of Dr West's work. We have differed from him in a few things, and some are not to be found in his pages which we think ought to have been there. On the whole, however, our impressions of the book and the author are most favourable. We have not been able to discover throughout the lectures, any new pathological view of importance, or any strikingly original suggestion in practice; but we have found him most thoroughly conversant with the progress and present state of science in Germany and France, as well as in our own country; and he has evidently viewed the opinions of others with keen scrutiny through the clear medium of his own mind, and cautiously weighed their suggestions in the balance of his own large experience. His reference to the opinion of others, too, is not so frequent as to oppress and distract the young practitioner, for whose benefit, chiefly, the lectures are designed. He has not, according to modern fashion, thought it necessary to tell what every one has said upon every subject, but has sifted and refined the golden particles from dust and dross, and brought to his readers only what is of sterling value, and may be made use of with profit. His own experience appears to have been most extensive—although chiefly among the humbler classes resorting to the infirmary for children. But this, too, has had its advantage, for it is in the substratum of society that the tyro to whom these lectures were delivered must probably commence his labours; and it is in that sphere that diseases are most rife, and assume the most malignant forms. From the highly philanthropic spirit in which he addresses his pupils, and the propriety and ease of his language, we are sure that they must have listened to him with interest and enthusiasm; and from his constant reference to first principles, they must have carried away from his course, the most clear and enduring impressions. But what we have admired most in Dr West's work, is the aptness with which he has introduced illustrative cases, and the distinct, truthful, vivid, pictorial delineations which he presents of the phenomena of disease. A. P.

A Remonstrance with the Lord Chief Baron touching the case, Nottidge versus *Ripley*. By JOHN CONOLLY, M.D., Fellow of the Royal College of Physicians of London, and Physician to the Middlesex Lunatic Asylum at Hanwell. 8vo, pp. 16. London, 1849.

OUR readers, by this time, are all aware of the extraordinary opinion delivered in the end of last month, by the Lord Chief Baron

of England, in the trial Nottidge against Ripley—namely, that no lunatic should be confined in an asylum, unless dangerous to himself or to others. If such be the state of the law, how many hundreds are illegally detained in the asylums throughout the country? And, if it be so, the sooner the law is changed the better. But it does not necessarily follow that what the Lord Chief Baron states from the bench is really the law. We can understand that an unchallenged decision from the bench should settle how the law stands; but this decision plainly receives a direct contradiction from acts of Parliament at present in full force. These acts make it lawful for the superintendent of an asylum to receive and detain any person whose insanity is certified by two medical men. These medical men are not required to attest that the person is dangerous to himself or others. The Legislature unequivocally makes the medical profession judges of what constitutes insanity. Judges and juries determine by the evidence of medical men; and it is only in exceptional cases, where the evidence of medical men is contradictory, that the bench or the jury can be said to take on themselves the responsibility of an opinion. But, respecting the insufficiency of the Chief Baron's test, as designed to mark out all the cases of mental disease which require confinement, we may safely assume that the medical profession are of one mind. The question, therefore, stands thus:—Acts of the Legislature admit insanity in that sense in which the medical profession receive the term, and that sense includes a multitude of cases which the Chief Baron's test excludes; it follows, then, that that judge has pronounced an opinion at variance with the law as settled by acts of Parliament.

But, to pass from this opinion as a mere exposition of the law, it is decidedly at variance with reason and common sense; and this Dr Conolly has shown demonstratively in the pamphlet, the title of which heads this article:—

"An acquaintance with persons of unsound mind, and further reflection, would soon convince your lordship that your rule is far too limited for the welfare of society, for the comfort of private families, and for the protection of many harmless insane persons themselves. It is not the dangerous lunatic alone who requires to be placed in an asylum. If the liberty of an insane person is inconsistent with the safety of his property or the property of others; or with his preservation from disgraceful scenes and exposures; or with the tranquility of his family, or his neighbours, or society;—if his sensuality, his utter disregard of cleanliness and decency, make him offensive in private and public, dishonouring and injuring his children and his name;—if his excessive eccentricity or extreme feebleness of mind subject him to continual imposition, and to ridicule, abuse, and persecution in the streets, and to frequent accidents at home and abroad;—his protection and that of society demands that he should be kept in a quiet and secluded residence, guarded by watchful attendants, and not exposed to the public."

The law affecting the insane should plainly be founded less on fanciful notions of liberty than on what is obviously for their

advantage. To both descriptions of the insane—namely, those who, in a pecuniary point of view, are a burden to their relations or to the public, and those who have the means of independence—a well regulated asylum affords the advantage of being the only probable means of cure. But lay aside this advantage, and consider an asylum simply as a place of seclusion from the world at large. No one will seriously maintain that the harmless pauper lunatic is not better cared for, as regards his personal comfort and happiness, in a well-regulated asylum, than as an object of derision in the streets; or that the imbecile at home, even in families possessed of sufficient means, is not far too often, for the credit of humanity, treated with a cruelty and disregard of his feelings, which could not occur in any establishment for the insane having the least pretension to be on a right footing. As regards the class possessed of pecuniary means, the law can hardly be too rigid in exacting sufficient proof of the actual existence of insanity, to guard against the cupidity of relations; but all who have extensive opportunities of seeing the interior scenes of domestic life know how many of this class at large would pass their lives more advantageously to themselves in an asylum, than subject to the painful caprices of their own will, or to the impositions of knavery, ever watchful to gain their confidence to the subsequent ruin of their peace. Those who are familiar with asylums know, that a well regulated establishment is far from being destitute of the means of enjoyment to those who are still capable of enjoyment. It is no paradox to say, that an asylum is the only place where a lunatic can command all the enjoyments of which he is capable, not even excepting liberty. Large asylums have the great advantage of permitting the inmates to be thrown into classes, capable of enjoying each other's society and amusements; and, as in all properly regulated establishments the principle is adopted of using the least restraint compatible with the condition of each class, a greater amount of liberty is obtained than could be allowed in their own families. To this statement there is hardly any limitation necessary, unless, indeed, it be proposed to throw pauper lunatics again abroad upon the world, at liberty to wander from house to house in our towns and villages.

To conclude, we cannot but agree with Dr Conolly, on reviewing all the circumstances of the case, that Miss Nottidge was quite a fit subject for confinement. We add one of our author's last paragraphs:—

"Your apparent disregard of all these circumstances, my lord, and your opinion, gravely, emphatically, and authoritatively given, that no insane person should be confined, except when dangerous, and none even then but under a commission of lunacy, takes away all protection from Miss Nottidge, and from many more who will become the victims of the sanctimonious knaves to be found in every rank in society. For some time to come it will also occasion great uncertainty and uneasiness in many families, who, after passing through many

afflicting scenes before making up their minds to seclude some insane relative, will now probably once more consent to incur the miseries of having the lunatic at large, and in their domestic circle. It will expose every feeble-minded person, possessed of property, to all the rascality of fanatics and swindlers. It will disturb the contentment of numerous persons, now happy and secure in asylums, where they enjoy a safe liberty, and every comfort. It will make every medical man so apprehensive of being unjustly censured for writing a certificate of insanity, as often to delay its doing until ruinous or fatal consequences have ensued. It will prevent many insane persons from receiving that early treatment, and being subjected to that timely superintendence, which can alone arrest insanity in its incipient stages, and prevent the supervention of stages of which the symptoms are more violent, and involve the lunatic and all about him in danger, and are such as make the case hopelessly incurable."

Surgical Anatomy. By JOSEPH MACLISE, Surgeon. London. Folio, 1849. Fasc. II. and III.

THE hearty commendation which we gave to the first Fasciculus of this work, continues to be deserved by the second and third, which are now before us. We are well assured that there are none of the cheaper, and but few of the more expensive, works on anatomy, which will form so complete a guide to the student or practitioner as these plates, if finished in the style of the three parts before us. To practitioners in particular, we recommend this work as far better, and not at all more expensive, than the heterogeneous compilations most commonly in use, and which, whatever their value to the student preparing for examination, are as likely to mislead as to guide the physician in a physical examination, or the surgeon in the performance of an operation.

There is one point in Mr Maclise's drawings, however, in which we observe a disregard of proportion, repeated so frequently as to call for notice, although not perhaps involving any great detriment to the general value of the work. If a mathematical test be applied to the arteries in those drawings which are much below the natural size, it will be found that they are not proportionately reduced. A certain latitude, in respect of the size of vessels, may indeed be allowed; but in Plates V. and X., the carotids will be found to be nearly, if not quite, the size of adult vessels distended with injection, while the rest of the parts are reduced fully one-half. This should not be; and, in any of the Plates which are yet unfinished, we hope that Mr Maclise will look carefully to this point. We have also to repeat very strongly our objection to the preposterous mannerisms which disfigure the otherwise excellent commentaries, frequently obscuring their sense, as well as destroying their simplicity. As one instance out of many, we would adduce the following extraordinary jargon, which is meant to tell that the left carotid and subclavian occasionally come off from the aorta by a common trunk of greater or less length :—" The peculiarities to the normal separate condition of A and B, the left carotid and subclavian

arteries of Fig. 2, display, in the relationary aggregate, a phasial relation of A and B, joining into a common trunk union," &c. After the allusions to this subject by nearly all the critics of this work, such "peculiarities *to* the normal condition" of the English language are, on the part of the author, quite inexcusable. If Mr Maclise cannot write simply, grammatically, and intelligibly, he should, at least for the sake of his readers, submit his crude productions to some one who could throw them into a correct form.

We are unwilling to mention these minor defects; but we are still more unwilling that the usefulness of such a work as the present should be in any degree marred by errors which can be so easily avoided in the parts yet unfinished. Mr Churchill's part in the work continues to be performed in a manner perfectly unexceptionable and worthy of the highest praise.

Questions and Observations in Hygiene; recommended to the consideration of Naval Medical Men; suggested to the Mind of the Author by the approach of the Asiatic Cholera. By FRED. JAMES BROWN, M.D., Lond., &c., Assistant-Surgeon, R.N. London, 1849. 12mo, pp. 64.

THE tone of this pamphlet is so admirable, and many of its suggestions so excellent in themselves, that we regret our inability, from the pressure of other matters, to do more than recommend it cordially to all concerned in the subject it refers to. It should be in the hands of all navy surgeons, the more so as its suggestions are mostly of a kind which can be made immediately available by each individual. The usefulness of works like the present is often injured, or, indeed, entirely neutralised, by such indiscreet or absurd proposals of reform as betray zeal rather than experience; and show that the author, however well versed in general principles, has been either ignorant of the practical bearings of the subject with which he is engaged, or obstinately and culpably neglectful of them. Dr Brown, however, is a real reformer, preferring practicable and useful alterations within his own sphere of duty to ostentatious and meddling displays of zeal, which sometimes actually retard the progress of the good they aim at, and when this is not the case, rarely accomplish more than an extremely fractional portion of it. We would particularly recommend the careful consideration of the suggestions on ventilation, on cleansing of necessaries, on sick-berths and their attendants, on the regulation of the allowances of rum and spirits, and on the examination of men entering the service, as these departments contain many useful hints on subjects which young officers must be apt to neglect, unless furnished with some such guide as this pamphlet.

At the time of writing this work, Dr Brown was an assistant-

surgeon in the royal navy; and we have heard that he has since resigned his commission from a strong conviction of the impropriety of the degrading position into which that ill-treated class of her Majesty's officers are thrown. That we find no personal grievances expressed in this pamphlet under such circumstances, is most creditable to the good taste and good sense of its author; but as we have already spoken strongly on this subject, in publishing the petition of the Royal College of Surgeons of Edinburgh (see p. 632 of this volume), and as our expressions cannot now compromise Dr Brown's interests or feelings in any way, we may be excused for referring to his resignation as a most lamentable instance of the injury inflicted on the navy by the continuance of the disgraceful regulations in regard to their assistant-surgeons. It is impossible to read this little work without feeling how much it is the interest of the Admiralty, and of the country, that thoughtful and efficient officers should be placed in situations of such immense responsibility, and that their position should be such as to give them an influence commensurate with their acquirements; and it may be undoubtingly predicted, that, should their claims continue to be neglected after having been fairly laid before the Legislature, as they have lately been, no more men like Dr Brown will be found willing to bury their talents in the medical service of the navy.

Memoirs on the Ganglia and Nerves of the Uterus. By ROBERT LEE, M.D., F.R.S., Fellow of the Royal College of Physicians, London, Physician to the British Lying-in Hospital, and Lecturer on Midwifery at St George's Hospital. (With five Engravings.) 4to. London, 1849. Pp. 35.

IT is clearly beyond our critical province to adjudge the disputed questions between Dr Lee and his scientific opponents. Even if we could pretend to a secure conviction upon points in regard to which anatomists of the highest character are at issue, the expression of such a conviction by an anonymous reviewer would not be entitled to the slightest consideration under these circumstances. Our readers are probably aware that the chief point involved in the inquiry is the nervous or non-nervous nature of two large masses, situated on each side of the uterus externally to the superior uterine veins, and shown in Dr Lee's dissections to receive fibres from the hypogastric plexus, and from the sacral nerves. These Dr Lee considers to be ganglia, and the filaments which spring from them in great abundance, are, according to him, the proper nervous filaments of the uterus, which, with the ganglia and plexuses that supply them, undergo great development and enlargement during pregnancy. Should these observations be proved to the satisfaction of anatomists, there is no doubt that we are indebted to Dr Lee for the dis-

covery of a new arrangement of the nerves of the uterus, unknown to Tiedemann and all prior observers; and in any case, we are indebted to him for much patient labour and research bestowed, we believe in perfect good faith, upon a highly important subject. It cannot, however, be denied, that many of the most eminent anatomists have not yet been convinced that the hypogastric ganglia of Dr Lee are really nervous structures; and in a question of fact such as this, the utmost scepticism is not only allowed, but required; so that we fear Dr Lee must not expect an unequivocal verdict in his favour from the public, until every eminent authority has been either convinced or clearly confuted.

Divested of the personalities with which the question has been unfortunately encumbered, it appears to us to stand thus:—Only one anatomist, Mr T. S. Beck, has given his time and labour to the attempt to follow the track of Dr Lee, and dissect the nervous structures of the uterus. The result has been a dissection in which the structures represented by Dr Lee as ganglia, do *not* appear as connected with the nervous system. As regards this dissection, Dr Lee says, that "the great blood-vessels, with all their accompanying ganglia and nerves, had been cut away, and that the right hypogastric ganglion had been torn in pieces." Thus Mr Beck, according to his opponent, "had stumbled at the threshold by destroying the ganglion, and lost the key to the whole nervous system of the uterus." It is evident, therefore, that the question as to the correctness of Dr Lee's observations rests upon a careful structural examination of these parts, which Dr Lee retained, and Mr Beck destroyed or disregarded. So far as we can learn, this important branch of the evidence is as yet insufficient; although Mr Dalrymple, a high authority undoubtedly, and the only one who appears to have gone over this branch of the subject with sufficient care, commits himself unhesitatingly to the support of Dr Lee. It has been alleged that Dr Lee has shown great unwillingness to encounter the microscopic proof. If this merely means that he has hesitated to submit his claims to the decision of microscopists, who, after a cursory examination, would be satisfied with their inability to discover distinctly nerve-tubes or ganglionic globules, we cannot think him wrong; as there is no doubt that the investigation in question would be one of the most delicate and difficult character, from the large and varying proportion of fibrous tissue which enters into all ganglionic nerves. But that this portion of the inquiry must be fairly entered upon before a verdict can be given, we are well assured; and its perfect relevancy is shown by the fact that, in the hands of Valentin and others, the microscope has actually been the means of decision in all disputed questions of nervous structure of late years.

We have not alluded to the decision of the Royal Society, by the award of their medal in Physiology, in favour of Mr Beck, and therefore against Dr Lee. We are unwilling to enter upon the

subject; nor do we feel called on to do so, as we believe that this attempt to prejudge the question cannot materially influence its ultimate decision. Dr Lee will find, if he will look to the future rather than the past, that there is a powerful court of appeal in this country prepared to judge him in a perfectly unbiassed manner; and the premature decision of the Royal Society will in no way affect the question at issue, if laid fairly and fully for investigation before the anatomists of this country.

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVIII.

MEETING XIII.—WEDNESDAY, *June* 6, 1849.—DR CHRISTISON in the Chair.

OXALIC DIATHESIS.

Dr Begbie read a communication (which is printed at length in the present Number), "on Stomach and Nervous Disorders as connected with the Oxalic Diathesis."

Dr Christison had met with many cases of oxaluria confirming the principles laid down by Drs Prout, Golding Bird, and Begbie. Most of his patients had been of the better ranks, or at least in comfortable circumstances, engaged in sedentary occupations, dyspeptic, and with the appearance of broken health. Twice, however, he had detected the diathesis in subjects of a totally different habit—in tall powerful active men. One of these patients had no dyspeptic symptoms, but complained of pain in the region of the kidney, and passed urine of very high density, in which Dr Christison could distinguish the octohedral crystals of oxalate of lime with the naked eye. All Dr C.'s cases had got well under the use of nitric acid, and due regulation of the diet. He recommended his patients to use little sugar, to avoid acid fruits, especially rhubarb, which seemed especially to favour the urinary deposit, and to take out-of-door exercise. It was important to know, that, even while the patient was using the mineral acids, the oxalic urinary deposits sometimes alternated or co-existed with lithate of ammonia. When the quantity of lithates was considerable, it masked or concealed the octohedral crystals. These could be separated by gently boiling the urine, and thus redissolving the lithates, pouring off the hot fluid from the surface, and diluting its lower stratum, where the oxalate was left, with distilled water. He could quite confirm the accuracy of Dr Begbie's statement as to the frequency of the oxalic diathesis; it was certainly far more frequent in Edinburgh than the phosphatic.

Dr George Paterson had detected the oxalic deposit in the urine of two hospital patients whose health was not broken, and whose dyspeptic symptoms caused them little or no discomfort. Both improved under the use of acids. The deposit had been casually detected in the urine of two patients in the female clinical ward of the Infirmary. They were affected with chronic cutaneous disease, and presented no symptom suggestive of oxaluria.

Dr Seller remarked that chemists had attempted to account for the oxalic diathesis, by supposing deficient oxidation to accompany the disintegration of the solids,—a degree of oxidation insufficient for the production of urea. It had not yet been positively determined how far sugar could be converted into oxalic acid during the process of assimilation. It was interesting to know that the urinary deposit might be induced by the use of rhubarb, which contained oxalate of lime ready formed; likewise that the presence of the deposit in the urine was not always accompanied with dyspeptic symptoms. He suggested that the constitutional symptoms of the oxalic diathesis might be possibly due to the presence of an insoluble salt in the system.

MORBID ANATOMY OF CHOLERA.

Dr W. T. Gairdner read a communication on this subject, which was given in the July Number.

MEETING XIV.—WEDNESDAY, *June* 20, 1849.—JAMES SYME, Esq., President, in the Chair.

EPIDEMIC OF SCARLATINA.

Dr Newbigging read an account of the scarlatina which prevailed in John Watson's institution, during September and October last.

[This communication will be published in an early Number.]

Dr Bennett made some observations on the microscopic appearances of scarlatina urine. It usually contained crystals of triple phosphate or other salts, epithelium, and rolls of fibrine, or albuminous casts of the tubes of the kidneys, indicating congestion or inflammation of the kidney, followed by exudation into the tubes.

Dr W. T. Gairdner thought that organic disease of the kidney was sometimes developed in a most insidious manner after scarlatina. Albuminuria was not its sure forerunner, yet long-continued albuminuria might imperceptibly induce it. The kidney, in such cases, might present the characters of the granular disease of Dr Bright; but the granulations were not essential; and the structure might be fatally deranged, although none were visible.

The *President*, *Dr Seller*, *Dr Spittal*, and *Dr Macaulay*, likewise made observations on Dr Newbigging's communication.

PINE SHAVING PASSED PER ANUM.

Mr Sidey exhibited a piece of *pine shaving*, about twelve inches long, which had been passed at stool by one of his patients. The man was 50 years of age, and, during early life, worked as a carpenter, when he was in the habit of chewing bits of shaving. For twenty years past he had not worked in wood, and was positive that he had swallowed no shavings during that time. In consequence of some uneasy sensations in the abdomen, Mr Sidey had recently recommended him to take a blue pill, followed by a dose of Ol. Ricini., and during the operation of the purgative, a round mass was discharged, covered with mucus. This, on examination, proved to be the piece of shaving now exhibited.

The *President* and *Dr Spittal* were inclined to be very sceptical as to the truth of such statements, which were often invented, especially by females, apparently for the sole purpose of imposing upon their friends or attendants. In Mr Sidey's case it was difficult to conceive how the shaving could have for twenty years preserved its form and structure while exposed to the secretions from the alimentary canal.

Mr Sidey remarked, that shavings seemed to have considerable power of resisting decomposition. It was often the practice of undertakers to fill up the interstices left between a body and its coffin with shavings; and he had been assured by good authority, that, on opening graves, these shavings were often

found tolerably fresh, when body and coffin had decayed and mouldered. He had no reason to doubt the accuracy of his patient's statement.

Meeting XV.—Wednesday, *July 4th*, 1849.—Dr Maclagan in the Chair.

TREATMENT OF ECZEMA AND IMPETIGO.

Dr Bennett read a communication on the local treatment of chronic Eczema and Impetigo. [This paper appears in the present Number.]

ON THE OBLIQUE MUSCLES OF THE EYE.

Mr Struthers read a communication on the muscles of the eye. The object of this paper was to bring comparative anatomy to bear on the question regarding the use of the oblique muscles of the eye. Mr Struthers came to the following conclusions :—

First, That the various views according to which they were believed to take part in the ordinary movements of the eye, by which the direction of its axis is changed, are erroneous ; and, *Secondly*, that these muscles were provided for the purpose of turning the eye in opposite directions on its antero-posterior axis. [The paper will be published in an early Number.]

The *President* and *Dr Robert Hamilton* made some observations on Mr Struthers' communication. The author afterwards explained the bearing which his observations had upon the operation for strabismus. If, as he believed, the axis of the eye could be turned outwards or inwards only by the external and internal recti muscles, there was no necessity for dividing the inferior oblique, as some had proposed. No benefit could be expected to result from the division of a muscle unconnected with the distortion.

ERRATUM IN REPORT OF FORMER MEETING.

Dr Taylor had received a letter from *Mr Spence*, requesting that the published minute of the 2d May might be corrected. Mr S. had not stated "that the soft parts of the heel were supplied with blood chiefly by the peroneal artery and vessels on the outer side of the ankle;" but had urged the importance of preserving both sources of supply to a flap formed of tissues not naturally very vascular.

REPORTS ON CHOLERA IN SCOTLAND, 1848-9.

Inverness, 17th May, 1849.—(*From Dr Manford*).—The disease made its appearance first in the village of Campbelton, situate ten miles east of Inverness, in the middle of March, and is attributed by many, particularly those holding contagionist views, to the washing of clothes, which had been brought to Campbelton, and belonged to a person who died of cholera in Glasgow.

On the 30th March, the first case appeared in Inverness—it was that of a man sixty years of age. Neither the patient nor any of his friends, so far as I have been able to learn, had any communication or intercourse whatever with Campbelton, or any other infected place. He died on the second day collapsed, having had the usual premonitory and other symptoms. The second case was a girl seven years of age. She died on the 7th day from the effects of secondary fever, after all the usual symptoms, except spasms had disappeared. The disease progressed slowly up to 26th April, when, in all, twenty-one cases had been reported, of which eleven recovered, and ten died. Nearly all the deaths were the first cases, which we attributed to delay in applying for medical advice. The people could not be induced to believe that it was cholera, but that the diarrhœa, &c., was the effect of the present unusual brackish quality of the water of the river Ness. Almost all the cases were confined to a low, dirty, damp locality, near the river, the water percolating through its gravelly banks,

and acting upon decomposed animal and vegetable matter. I may mention that the father of the second party attacked, had, a week before his child was taken ill, returned from Glasgow. He was attacked on the 10th, and died after thirteen hours' illness. So far as we could discover, he showed no symptoms of the disease before cholera exhibited itself in the child. He evinced beforehand, and while apparently in robust health, great apprehension of an attack of the disease. His wife also was attacked with premonitory symptoms, but recovered. No other case occurred in Inverness which could be traced to an infected locality. A young woman left her service to attend her sister, a married woman, labouring under malignant cholera—the sister died, and the house was shut up. The young woman took up her residence next door—she was attacked four days after, and died next day. She never had any fear of the disease, believing that it was the effect of the water, but she was much exhausted by fatigue in attending her sister, besides being of a delicate, unhealthy constitution, and predisposed by other causes. The woman employed to attend the last, as well as a previous and fatal case, was attacked and recovered. She was discovered to be a person of intemperate habits, but unfortunately she was the only nurse we could procure in the emergency.

From 26th April to 5th May, no cases occurred; but on the latter day a woman residing on the opposite side of the river, and her two sons, aged nine and twelve, were attacked. The woman recovered—the boys died; the first after twelve, the second after twenty-four hours' illness, of severe vomiting, purging, and cramps, ending in collapse. No case since.

Such is a brief history of cholera here at this time, and I may just state that from my observation of the disease now, and during its prevalence here in 1834, nothing has occurred to alter the views I have hitherto held regarding its epidemic influence, aggravated by local and other predisposing causes.

Campbelton, Inverness-shire.—(*From Mr J. Tolmie, M.R.C.S.E.*)—[We have received the following report from the Secretary of the committee appointed by the Edinburgh Colleges, to investigate cholera, as the committee considers its early publication to be important, the facts having been frequently stated in a very imperfect form.]

On the 22d February, the wife of a pensioner, residing in Campbelton, went to Glasgow for the purpose of bringing home the children of a brother, whose wife had died of cholera in the end of January. She returned here on the 3d of March, bringing with her two children, a box containing some blankets, counterpanes, tablecloths, together with the children's clothes, and some other articles of furniture, which had been used by her deceased sister-in-law. On the night of Wednesday the 14th March she washed the blankets, and other clothes brought from Glasgow, and emptied the suds into a covered drain which ran between her house and the house of her landlord, John Leitch, next door to her. Her landlord, assisted by his son, had been for two or three days employed in thatching his house; and the clay which was mixed up for the purpose of securing the straw thatch, lay at the pensioner's door, and close by the grating of the drain. The younger Leitch, who was in the habit of lighting his pipe, and resting himself, in the pensioner's house, was, on the morning of Friday the 16th, attacked with diarrhœa, which continued with exhausting severity all day. In the evening, about five o'clock, he fainted on coming into the house, and was carried to bed by his father.

At midnight I was called in for the first time, and found the father and son in the last stage of Asiatic cholera, collapsed and pulseless, suffering much from cramps, and still passing colourless dejections. The father, a man of seventy-six years, was taken ill in the evening. Both died on the 17th, within three hours of each other. The third case of cholera occurred on the night of the 18th, in a house adjoining that of John Leitch, in the person of a labourer named Draper, who had been talking to Leitch on the 16th, and who had taken some purgative medicine on the 17th. This man eventually recovered, though

he passed no urine till the 22d ; but the female who nursed him during the first day of his illness—a cleanly, but not very temperate, person—was seized on the morning of the 20th, and died on the 21st. The father of this young woman, who nursed his daughter, was attacked by the pestilence on the evening of the 20th, and died on the 22d.

Alex. Fraser and Jean M'Donald, who lived about 200 yards from the house where cholera first appeared, were likewise taken ill on the 20th. The former died on the 21st, the latter recovered. In these two cases, I could not trace any direct communication with cholera houses. On the 22d, widow Urquhart, aged about sixty, became ill ; also a child of two years of age. The latter had suffered for several days from diarrhœa accompanying dentition, and lived in an ill-drained and filthy locality. Both died on the 24th. On the 23d, widow Macbean, an old woman, who had seen widow Urquhart on the 22d, took ill, and on the same day, John Mackenzie, one of the nurses. The former died on the 24th ; the latter, whose case was one of great severity, recovered—but his wife, who nursed him assiduously, became ill on the 28th, and died on the 29th. And the son of widow Macbean was attacked on the 24th, in the room which his mother occupied, and died on the 26th.

A sister, a brother, and a brother-in-law, of widow Urquhart, two of them living in the same house, who had all three been ministering to her wants, took ill successively on the 25th, 26th, and 29th.

From the 16th March to the 1st April, there occurred forty-one cases, and fourteen deaths.

In the cases cited above, it will be observed that, with very few exceptions, intercourse with the affected could be distinctly traced ; and this fact was almost uniformly observable throughout the epidemic. Atmospheric and telluric agency, which exerts a powerful influence in the propagation of the pestilence, did not appear to operate very actively in the present instance ; for persons in the locality where it appeared, who secluded themselves and families from all communication with infected houses, escaped ; while their susceptibility, as regards unhealthy situation, insufficient or innutrient sustentation, modes of living, and neglect of personal cleanliness, were apparently the same as those seized. When the disease appeared in Campbelton, there was no cholera locality nearer than Glasgow. The pensioner or his wife had no symptom of the disease ; although the man, to appease the clamour of the people, volunteered to become a nurse, and performed all the duties diligently and fearlessly for upwards of a fortnight. One of his children had diarrhœa. The preceding remarks bear exclusively on the manner in which the disease was imported or propagated in Campbelton. As regards the treatment, I can offer little or nothing new—but must mention that I considered turpentine in small and repeated doses useful as a stimulant, and as forwarding the torpid functions of the kidneys.

I must add that my impressions of the non-contagious nature of cholera, formed from my acquaintance with the disease in 1832, are very much shaken.

Jedburgh.—(*From Dr Thomson.*)—During the prevalence of cholera in Scotland in 1832, Jedburgh enjoyed a complete immunity from its ravages. The circumstances which marked the introduction of this terrible plague into the town, at its second invasion, are of a somewhat peculiar character. An offender apprehended in Dumfries, while cholera raged virulently there, for a serious crime committed in Roxburghshire, was lodged in the county prison in that town until the proper officers arrived to convey him in safety to Jedburgh. The day previous to his removal, 17th Dec., he was under medical treatment for *vomiting* and *purging ;* the diarrhœa continued along the journey, and when he arrived in Jedburgh on the 19th Dec., he manifested unequivocal signs of Asiatic cholera, of which he subsequently died. Very soon after, the 21st Dec., the disease made its appearance among the community, and rapidly diffused itself over the whole town. The first case, which proved fatal in fifteen hours,

occurred within fifty yards of the house occupied by the prisoner *pro tempore;* and nearly all the other cases can be traced to direct communication with persons labouring under the disease. One case more especially ought to be recorded, where the patient was attended by four relatives *successively*, each of whom was seized with cholera and died. It deserves to be noticed, that the residences of the sufferers, with very few exceptions, were of the worst description, low, damp, and confined; and their habits intemperate and improvident.

OBSERVATIONS ON THE EPIDEMIC CHOLERA OF 1848-9,

CHIEFLY AS IT PREVAILED IN THE 13TH MEDICAL DISTRICT OF THE CITY OF GLASGOW PARISH. BY JAMES MAXWELL ADAMS, SURGEON; MEMBER OF THE FACULTY OF PHYSICIANS AND SURGEONS, GLASGOW.

During the late epidemic of cholera in the City of Glasgow parish, I had favourable opportunities of observing the disease, having had the charge of one of the seventeen medical districts, into which the parish is divided. The following observations refer chiefly to the disease as it appeared in that district; but include, also, the results of cases treated in private practice, and which occurred in various localities throughout the city.

The thirteenth medical district comprises that central portion of the city which is bounded on the upper or north side by George Street; by the west side of High Street to Stirling Street; by Stirling Street to Candleriggs; thence to Argyle Street; by Argyle Street to Buchanan Street, and thence to George Street. It forms pretty nearly an oblong quadrangle, running from east to west, half a mile in length, by a quarter in breadth. The population of the entire district can scarcely be under 14,000; and of this number, I consider that about 5,000 are of that class who avail themselves of the services of the parochial surgeon. By far the greater proportion of this class reside in the north east corner of the district, within a space 280 yards in length, by 120 in breadth, and it was within this limited area that fully nine-tenths of the cases of the late epidemic occurred.

The condition of the great mass of the inhabitants, and the general sanitary state of this portion of the district, are probably as bad as can be well conceived. The locality consists chiefly of five or six narrow streets, and about a dozen of the dirty "closes" or "wynds" for which Glasgow has acquired a discreditable notoriety. There are very few sewers, or gratings which conduct to sewers, and the greatest amount of drainage consequently takes place on the surface. The receptacles for filth consist of large open dungsteads placed either in the centre of the closes, or of the lower flats of inhabited tenements, with an open window through which the refuse is cast, and in these places of deposit, the filth accumulates till it is in such quantity as to necessitate the removal of a portion from want of space for further deposit. The cases are exceptional in which regular arrangements are made for this purpose, and in all they are miserably inefficient. Thus, in three of the worst closes of the district, I occasionally observe a frail old pauper paddling about the principal dungstead with a broom or shovel, and the entire sanitary operations of these localities are due to his solitary exertions,—in return for which, he receives the shelter, rent free, of a wretched cellar in the neighbourhood.

The supply of water is very scanty; a single pipe or fountain is made to suffice for one or more of these closes, and the trouble of going such a distance as is required in order to procure a supply, seems to be sufficient excuse for the people to limit the use of this essential element to the narrowest limit compatible with necessity.

The houses are of a wretched character, excessively over-crowded and filthy. They generally consist of one apartment, but occasionally of two, of from eight to twelve feet square, seldom containing less than four, and very commonly as

many as twelve, inmates for each apartment. A great proportion of the population is migratory, and only lodge with the party who rents the premises, paying a small pittance nightly or weekly. Beds are rarely seen—the inmates lying on the floor, often without *doffing* the clothes worn during the day. In numerous instances where the parties work in-door upon soft fabric goods procured from warehouses, the webs of cloth so obtained, serve as coverlets until the period when they are returned to the manufacturer or warehouseman. There are few of these houses in which there is not a pauper receiving out-door relief.

Fever may be considered as endemic in the district, and the expense which the parish incurs in the maintenance and attendance of the victims of epidemic, endemic, and contagious diseases, and in making the various provisions which are, in consequence, obligatory on parochial boards, cannot fall far short of the whole rental which the proprietors of these lazarettos draw from their inheritances.

During six months only of 1847, as many as 400 cases of typhus fever occurred within this portion of the district, and were treated at the cost of the parish. The total cases of disease occurring within the district, within the last two years, and treated at the cost of the parish, was 1900, of which number, fully 1000 were cases of epidemic, endemic, and contagious disease.

Cholera first appeared in the district on 9th December 1848, in the ground flat of a building in 22 Shuttle Street. The patient, a female aged fifty-six, was of occasionally intemperate habits, and her previous general health was not good. She had not been in communication with any infected district or locality so far as could be ascertained. She was immediately conveyed to hospital, where she died on the following day. I immediately got the house limewashed, and the straw-bed on which she had lain destroyed, but the inmates would not consent to removal.

The next case occurred in the same house. A female child, aged two years, became affected on 12th December with bilious diarrhœa, which was followed on the 17th by all the symptoms of cholera. She died on the following day.

The third case occurred in 51 Shuttle Street, being on the opposite side of the street, and at a distance of about thirty yards from the last locality. The patient, a female aged twenty-six, was of irregular habits, of infirm intellect, and subject to occasional attacks of dysenteric diarrhœa. She resided in the top story of the tenement: and for a few days prior to December 18th, had been very little out of doors, and had not, so far as I could ascertain, been in communication with the sick. I had her conveyed to hospital, where she died on the following day.

The fourth case occurred in the same house with the preceding. The patient, a female, aged fifty-three, of irregular habits, and average good health, became affected with bilious diarrhœa on December 23, and on the following, with all the best marked symptoms of the malignant disease. She recovered after a somewhat protracted convalescence.

The fifth case occurred in 34 Shuttle Street, in the top flat of a building closly adjoining that in which the first case occurred. There is no direct communication between the buildings. The patient was a healthy temperate female, aged eighteen. She had slight diarrhœa on December 24, and next morning, soon after breakfast, she was seized with vomiting, speedily followed with purging and cramps. I had her immediately sent to hospital, where she died, soon after admission.

Up to this date, there had occurred, in all, of cholera and diarrhœa, only seven cases, all within a circle of a few yards, and at the extreme north-east corner of the district; but within the next *three* days, I was called to *twenty* cases in various parts of the district, and of this number, eleven were malignant cholera. The farther progress of the epidemic will be best illustrated in the following table, which includes all cases of cholera, and of other diseases which occurred in the district, within the period specified.

No. I.

Table of Cases of Disease occurring in 13th Medical District, from December 9, 1848, to March 10, 1849.

Divisions of Table.	Classification of Cases.	Total Cases.	Number of Cases in the week ending												Total Deaths.
			Dec. 16.	Dec. 23.	Dec. 30.	Jan. 6.	Jan. 13	Jan. 20.	Jan. 27.	Feb. 3.	Feb. 10.	Feb. 17.	Feb. 24.	Mar. 3.	Mar. 10.	
A. Cases of Choleraic Disease.	1. *a.* Bilious purging	47	2	...	13	3	11	3	11	2	...	...	1	...	1	2
	b. Bilious purging, with vomiting *or* cramps	23	...	...	1	9	2	6	4	...	1	...	...	...	...	...
	c. Bilious purging, vomiting, *and* cramps	12	...	1	1	4	2	1	1	...	1	...	...	1	...	...
	2. Vomiting and cramps	7	...	...	...	1	1	1	...	1	2	1	...	...	...	...
	3. Serous purging	12	...	...	...	5	2	...	1	1	2	1	...	...	...	...
	4. *a.* Serous purging, with vomiting	14	...	...	4	2	3	1	...	2	...	...	1	...	1	5
	b. Serous purging, with vomiting and cramps	67	1	2	21	12	11	7	7	2	1	2	1	...	...	32
	Total cases	182	3	3	40	36	32	19	24	8	7	4	3	1	2	39
	Of the above there was sent to hospital of Class 4	17	1	1	3	3	3	...	3	3	...	...	...	...	...	...
	Deaths from Class 4 at home	24	...	1	5	10	2	3	1	...	1	...	1	...	...	...
	... ... in hospital	13	1	1	2	3	3	...	2	...	1	...	...	...	...	...
B. Diseases *not* Choleraic	Acute diseases, or of recent origin	67	4	2	0	4	8	7	7	8	4	8	3	7	5	5
	Chronic diseases, or of very trivial nature	104	5	2	4	4	13	8	8	8	17	7	10	7	11	...
	Deaths	5	...	...	...	2	2	1	2	...	...	...	...	...	...	...

In this and subsequent tables, I have classified all cases of disease which partook of the epidemic character, according to the manner in which certain very evident and easily recognised symptoms were grouped. This will enable me better to convey the result of my individual observation and experience; the vague and ill-defined use of the term "cholera," leading to most unsatisfactory and contradictory inferences. One practitioner loses fifty per cent. of his cases of cholera, and another boasts of 1 death in 100. It is therefore highly desirable that every medical man, who contributes his share of experience to the general stock, should define rigidly the meaning of any important term of which he makes use, so that his labours may be made available by subsequent *collaborateurs*. With this object, I have endeavoured to analyse the cases of epidemic disease in such a form as will enable any one who studies them, to draw his own inferences.

In the first class of cases, the dejections were characterised by the presence of bile or fæces. In the third and fourth classes, the dejections, to all appearance, and from such an examination as could be made at the bedside of a patient, contained no bile or feculent matter. With the exception of two cases of bilious diarrhœa which continued for several weeks, and from their protracted severity, exhausted the strength of the patient, no fatality occurred in the three first classes. It is, therefore, to the last class that I restrict my application of the term "*cholera*," viz., to cases characterised by a purging and vomiting of serous fluids without admixture of bile or feculent matter, and generally accompanied with spasms of the extremities. At the same time, I have no doubt that all of these cases partook of the same character, and originated from the same source, and that the differences observed were only those of degree, or in other words, were results of the same morbific influence, modified by individual peculiarities, circumstances, predisposition, &c.

In the first class, the stools were generally of a dark blackish colour, always loose and watery, and containing bilious matter, with a feculent odour; occasionally they resembled thin pea-soup, and between these extremes, there were various degrees of appearance and consistence. The first call to stool was, in many cases, sudden, and they were afterwards very frequent. They occasioned little or no pain, and were rather accompanied with a feeling of relief. In a few cases, spasms were the forerunners of the diarrhœa.

In the second class of cases there was no purging, the bowels rather *tending to constipation*.

These two classes of cases were very amenable to treatment. In many the effects of medicine were immediately evident, and, with the exceptions already quoted, all terminated favourably. They included, nevertheless, numerous serious cases, requiring assiduous watchfulness and active treatment. In eight cases there was morbid suppression of urine; in twenty-eight there was induced such an amount of prostration, with marked coldness of surface, that I have termed them collapsed, and in twenty cases there existed a febrile condition characterised by high pulse, dry red tongue, heat of skin, thirst, &c., and lasting for several days.

The third class of cases had neither bile nor feculent matter in the dejections, which were identical with those of the early stages of cholera. These cases were also very amenable to treatment. One case of cholera was seen and treated in this stage and was doing well, but the patient improperly went out and exposed himself for several hours to cold and wet, which was followed by a return of symptoms in an aggravated form, and the case ended fatally.

Very few cases of the fourth class, or that to which I apply the term *malignant cholera*, occurred without the patient being affected with premonitory symptoms. Some individuals had, for days previous to the attack, headache more or less severe, nausea and sense of oppression at pit of stomach, rumbling sensations in the bowels, and a peculiar pallid aspect. But as few persons living in the affected localities failed to experience, at frequent intervals, these and other

anomalous disturbances of the general health, which were not, however, followed by any other or more severe affection, I am not disposed to place much weight on their presence as a forerunner of cholera. Some cases occurred in which I could not learn that the patients had been affected with any premonitions of disease, and who had suddenly, and within a few hours, passed into the extremest state of cholera; but in most of these cases the parties had been, for several hours or days previously, indulging in spirituous drinks, and in that way undoubtedly lowering the tone of vitality, and rendering the system more susceptible to the impressions of epidemic influences. In other cases of a sudden nature, the symptoms occurred close upon the taking of a full meal, and especially where the food was of unaccustomed kind, or of difficult digestion. In no instance did I observe a case of *cholera foudroyant*, as it is termed, *i.e.*, where the patient, while in the full possession of his accustomed health, and without any premonitory symptoms, is alleged to have been at once, and like a lightning stroke, prostrated by the disease. On the contrary, I found that besides the premonitions already spoken of, there was in sixty-nine cases a loose state of the bowels, unaccompanied by any other urgent symptoms, for a period varying from five or six hours to as many days, and very commonly two days. During this period the stools were bilious and feculent, and the general condition in nowise to be distinguished from that attending an ordinary case of diarrhœa, excepting that there was not the same amount of pain or uneasiness that generally attends a bowel complaint. So little alarm or inconvenience did this state of health occasion, that the patients spoke of it with unconcern, and often attended to their ordinary avocations. This relaxed state of the bowels was observed so frequently to precede an attack of cholera, that I regarded it as the most practically valuable indication of the approaching disease, and a symptom, therefore, that should never be neglected during the prevalence of a *cholera epidemic*. In a very few instances I have heard statements of a contrary kind; but in investigating into this point, a source of misapprehension requires to be guarded against. The attention of the lay community is naturally directed to that period of a disease in which the condition of the sick becomes alarming, and evidently dangerous. They attach little importance to symptoms, which, in their view, can have had little influence upon the progress of the complaint, and they are guided in these impressions in reckoning the outset of illness. In this way reports of cases are circulated which convey exaggerated impressions regarding the fearful rapidity of fatal cases, and the blind uncertainty with which this disease strikes its victims—reports which are calculated to paralyse the measures of prudent precaution, by exhibiting the apparent futility of such attempts. From the same causes some practitioners allow themselves to be misled, and without any or sufficient investigation, adopt statements which, when subsequently given forth by them, tend greatly to embarrass the researches of others, who, with greater interest and nicer discrimination, seek to discover the landmarks of medical science.

After more or less of premonitions, as already described, the patients affected with cholera passed into the *active stage*, in which there was profuse and frequent evacuations from the stomach and bowels, generally accompanied with spasms. The stools, always watery and opalescent, varied in appearance—water tinged with milk, and having scales of bran suspended through the fluid, or forming a small sediment at the bottom of the vessel, would give a tolerably accurate idea of their general character. Sometimes they were more opaque, and had a cream-like froth at top, at other times they were of a dark tinge resembling moss water. Latterly they became as clear and limpid as pure water. There was commonly no offensive odour, although occasionally the odour was peculiar and very offensive. The quantity passed at one stool varied greatly. At the commencement of the active stage, it was not uncommon for three, four, or five pints to be discharged at one evacuation. After one or two large stools, the others varied in quantity from a few ounces to a pint, and continued to be passed for several hours, as often as one every few minutes or half

hour; the number of stools being generally in the inverse proportion to the quantity passed at each stool. For in some cases, after one or two large evacuations, which seemingly drained the body of its whole fluids, the patient sunk at once into collapse, and any further discharge from the bowels oozed away without effort or consciousness. Vomiting or retching, which sometimes preceded, and always accompanied the other symptoms, was frequently very distressing to witness, and in the early period of the epidemic, I was anxious in devising measures for its alleviation. I am now satisfied that it is not always an unfavourable symptom, having observed that in most of the cases which recovered, vomiting or retching had been a very severe symptom, and little under the control of curative measures. In some instances I have even ventured to give mustard in hot water, as an emetic, and, as I have supposed, with good effect. The act of vomiting did not often seem to occasion great distress to the patient, although the contents of the stomach were thrown off with great force; that viscus alone seemed frequently to be the sole propelling power,—the abdominal muscles not being called into play, or not in the same degree as is observed in ordinary vomiting.

After the purging and vomiting had continued for some time, generally an hour or more, spasms supervened, affecting chiefly the flexor muscles of the toes and legs. Less frequently the upper extremities were affected, and in such cases, they assumed more of the character of clonic spasm, were less painful and more persistent. The greatest suffering, and, I may add, the greatest danger, seemed to be in those cases in which the abdominal muscles, and especially the diaphragm, were mainly affected, as they often were towards the termination of those cases which ended fatally.

Spasms were present in sixty-five cases, but in some they existed only in slight degree, and were not much complained of.

Two cases were singular with regard to this symptom. The patients were both females, and during the active stages of a very severe attack, had not in the slightest degree been affected with spasms; but when fairly convalescent, and at a distance of eight or ten days from the accession of their illness, they were seized with spasms of remarkable severity, affecting by turns every part of the body, and causing severe suffering for fully twenty-four hours.

The *active stage* of the disease, after continuing for a period varying from two to twenty-four hours, was succeeded by that of *collapse*. In this condition, the surface of the body became perceptibly cold to the hand of a bystander, and the strength of the patient seemed entirely prostrated. In numerous cases this state of depression was very extreme. The coldness was more intense than that of a dead body, and the surface became bedewed with a cold unctuous sweat; the voice was reduced to the faintest whisper, the eyes deeply sunk in their sockets, and surrounded with a livid areola; the features pinched and of a ghastly expression; the skin of a pewter, or deep livid colour; the members contracted, and the whole body shrunk in its general volume; the expired air felt cold and raw, and the pulse could not be felt at the wrist. In some cases it was with difficulty it could be ascertained that the patient was actually in life. From this extreme state of collapse I have observed very few recoveries.

In a larger proportion of cases, collapse was present in a less intense form, the degree of severity varying in individual cases. In two cases, there was not such an amount of depression as to warrant me in terming them collapsed.

When death did not take place in collapse, there succeeded within a period varying from a few hours to several days, the stage of *reaction*, a condition more or less febrile, in which the pulse and natural warmth gradually returned; the face became flushed, and occasionally there was slight delirium. The secretions were also gradually restored, bile appearing in the stools, and urine being passed in natural quantity,—after which recovery generally took place. Sometimes a smart attack of hiccup seemed to be the first indication of favourable reaction. In other cases, this reaction, or rallying of the powers

of nature, was characterised by a smart attack of vomiting of fluids mixed with bile, and by the stools becoming excessive in quantity, or containing vitiated bile, or bloody mucus as in dysentery. This dysenteric condition proved fatal in one case after a lapse of several weeks, and in some others, it caused the convalescence of the patient to be considerably protracted. Reaction occurred in seventy-nine cases, but in seven of these, the primary symptoms suddenly returned; and the patients passed for a second time into collapse, and died within a few hours. These cases were truly disheartening, as in some, there had been for several days the most encouraging signs of amendment, and their recovery was hardly a matter of doubt.

In twenty-one cases, reaction was succeeded by a low fever closely resembling our common typhus, and which, in most instances, seemed to depend on structural lesions of the viscera of the abdomen, chest, or head. Where the head was implicated, the patient lay in a drowsy or semi-comatose condition, capable of being roused, and of answering questions coherently; the sclerotic became intensely congested; the pupils contracted as if from opium, and the eyeballs turned upwards. This state generally ended in complete coma and death.

In some cases from the commencement, and in all within a few hours, the patients became tormented with an insatiable thirst for cold drinks. No supply seemed sufficient to give more than momentary relief; and in many, the fluids swallowed were immediately and invariably returned by vomiting. The quantity swallowed was often enormous, and the desire continued, even in the extremest state of collapse as long as the sufferer retained his consciousness, and was able to intimate his wishes.

In every case seen before reaction had taken place, the tongue was cold and moist, and of a pale or white colour. The pulse was commonly slow, feeble, and like a thread under the finger, often very indistinct, and sometimes altogether imperceptible for many hours before death. The action of the heart, which was not always noted by me, was generally low and the sounds indistinct; sometimes it was tumultuous, and occasionally the impulse was very violent and strikingly perceptible. The respiration was short and laborious, and often accompanied with rough gurgling rales. A constant uneasy restlessness was present in almost every case, but especially in those where the collapse was not extreme; and in all the severe cases of collapse, the patient complained of intolerable burning pain around the heart. I have frequently observed, that before the stage of collapse, and while the body continued warm, the patient complained of a feeling of cold, while, in the stage of collapse, an opposite feeling was manifested.

In several cases, not only of true cholera, but of the less severe forms of choleraic disease, there was observed an eruption of a measly character, chiefly confined to the neck and trunk, and one case where this eruption was strongly apparent, ended fatally.

I have not met a case of that kind where death is alleged to have taken place without purging; so contrary is my experience, that I regard the disease as purging, and the danger to depend, *cæteris paribus*, on the amount and suddenness of the evacuation. The amount of fluids discharged from the stomach and bowels, was productive of very different effects in different cases, as might naturally be expected. A person of meagre spare frame will be thrown into severe or fatal collapse from the loss of a quantity of fluids which would not cause any serious results to another whose body was of an opposite build.

Nothing short of actual observation can give a proper conception of the extraordinary change which takes place within a few hours in the appearance and condition of a cholera patient, and it is even more difficult to expect the recovery of a patient in such apparently hopeless circumstances. In six cases, however, where the pulse at the wrist had, for several hours, been quite imperceptible, and in fifteen cases where the surface was of marked livid hue, recovery took place.

On the subject of treatment, I have little satisfactory to communicate. It would indeed be difficult for me to select a case of recovery in which I am clearly satisfied that the treatment had produced that result. That treatment was beneficial, may, however, be inferred from the following analysis of cases, showing the period which had elapsed from the onset of active symptoms, till that in which the case was brought under treatment.

	Cases.	Deaths.	Recoveries.
Not longer ill than six hours,	33	7	26
Ill more than six, and not longer than twelve, hours,	24	8	16
... twelve, ... twenty-four,	20	9	11
Upwards of twenty-four hours,	16	10	6
	93	34	59

Those cases, therefore, which were earliest brought under medical treatment, recovered in the largest proportion.

As to the remedies employed either in combating individual symptoms, or in dealing systematically with the disease, it would be tedious and unprofitable to enter into detail. My routine practice latterly, and in the majority of cases, was to rely, in the premonitory and active stages, upon the acetate of lead, with opium and capsicum, in small doses, frequently repeated. In the stage of collapse, I gave calomel sometimes, in small doses, and at very short intervals, sometimes in 10 grains or ℈j doses at longer intervals. Large doses of calomel had a decided effect in allaying the vomiting, much more so than any remedy which I tried. Moderate warmth was kept up with hot bricks and bottles of hot water, and frictions were employed to relieve the cramps. Turpentine was chiefly employed for this purpose; but I believe that the real efficacy lay in the rubbing alone. Cold fluid drinks were permitted, in small draughts at a time, *ad libitum*. Plain cold water was the drink most anxiously desired, and was always permitted. When practicable, I substituted the albuminous drink recommended by Dr A. Buchanan, consisting of water, sweet milk, white of egg, and a little common salt. In a number of cases in the stage of collapse, I administered this albuminous emulsion in the form of tepid enemata, and with very satisfactory results. That these enemata were largely absorbed, there can be no doubt, as, in some cases, I have injected several pints *per diem* for two days in succession, not one spoonful of which was returned.

Mustard sinapisms, or other rubefacients, were used in most cases, but with very questionable effect, either beneficial or otherwise. In no case did I think it necessary to prescribe spirituous stimulants internally. Often, however, they were administered prior to my being called, and sometimes they were tolerated, where, from the condition of the patient or other circumstances, I did not feel justified in opposing a popular prejudice. A very favourite remedy with the poor was whisky or brandy stirred thick with black pepper. In numerous cases I prescribed as a stimulant carbonate of ammonia, in the proportion of forty grains to the pint of water, a table-spoonful of which was given at frequent intervals, and I think, with occasional benefit.

A very important subject is the treatment of the sick in the hospitals.

In addition to the cases already commented upon, and which were treated at the houses of the sick, there were seventeen cases of cholera sent to Clyde Street hospital, only four of which recovered. This result could not be owing to difference in treatment, as the sick in hospital wanted nothing that skill and humanity could suggest. Neither was it owing to the cases being of a more malignant character than those treated at home, nor to delay in transmitting the patients to hospital, nor to there being an undue proportion of the early cases of the epidemic. The first table shows the period of the epidemic at which the cases occurred; six of the cases were sent to hospital within six hours of attack, four cases within twelve hours, three within eighteen hours, and four were sent after reaction had taken place. The cases were of average severity.

A very large proportion was of the female sex, and none were under ten years of age. What, then, was the cause of so marked a difference in the result?

Almost from the instant of attack, a cholera patient may be considered as engaged in a death struggle. To be raised in this dying condition, carried along crooked stairs and narrow passages to a cholera van, to be there rattled and jolted for a distance of half-a-mile or upwards, followed by a second transference to the hospital ward, cannot be considered an unimportant process by any medical man who has witnessed the disease. I set aside any consideration of the probable effect on the mind of a patient, as I have observed that in cholera the patient is singularly apathetic, presenting in this respect a contrast to a fever patient. At first, when I had all my experience to gain with regard to the treatment of cholera, I was favourably disposed to the employment of hospitals, and looked with painful apprehension to the treatment available to the sick poor, residing in dwellings abounding in negatives, *sans* food, fire, bedding, clothing, light, air, quiet, attendance, &c. &c. I am now, however, clearly satisfied that a pauper patient, lying on his wisp of straw or bare floor, with a relative or other attendant to supply him with a draught of cold water, and to surround him with a few hot bricks, has his chances of recovery fearfully diminished by removing him to all the comforts and *refined* treatment of an hospital. If my experience on this subject were singular, I would hesitate to venture so decided an opinion, but, from careful inquiry which I have made among many of the parochial surgeons, I find their experience so entirely corroborative, that I feel justified in condemning the principle of hospital treatment for cholera patients. Hospital provision must undoubtedly be made for cases which cannot otherwise be dealt with, such as those which have been abandoned by relatives or others; these hospitals should, however, be within a very limited distance—in short, mere temporary hospitals, on the simplest and smallest scale for limited districts. Motives of economy and expediency may be pleaded in urging the adoption of large central hospitals, but much information of a satisfactory kind is wanting to show that their use is advantageous or even economical, and to satisfy a medical man that he is not casting heavy odds against the life of a patient who is conveyed thither by his order.

(*To be continued in next Number.*)

SCOTCH MARRIAGE AND REGISTRATION BILLS, PUBLIC HEALTH BILL, AND HEALTH OF TOWNS BILL.

ALL these bills have been withdrawn for the present session. We are no judges of the sublime mysteries of politics. Whether the parliamentary friends of these important measures acted well and wisely, or the reverse—whether the parliamentary enemies of them were guided by simple ignorance of their importance, or by a far more culpable spirit of partizanship—are matters beyond our ken. But nothing that either of these classes of legislators have said, has, in the slightest degree, affected the opinions we expressed in April of the Registration Act, or those which, in our last number, we expressed regarding the Public Health Bill. To these numbers we refer our readers for our mature opinions, which it is the less necessary to repeat, because the profession to which we belong is unanimous in supporting the views which we there expressed. The Royal Colleges of Physicians and Surgeons of Edinburgh have always been unanimous in their support of the principle of the Registration Bill, both in this and in former sessions. They were also unanimous in supporting the principle of the Public Health Bill. They did their best to remove all objectionable matters from both bills. Those members of our profession in Scotland who are not Fellows of the Colleges, have, to their great honour, acted in exactly the same way, with scarcely any exceptions worthy of being noted. It is, therefore, strictly true, that these bills are thrown out in direct opposition to the expressed opinion of the whole medical profession in Scotland, who have maintained that the principles on which they are based

are important to the community, and that they ought to be carried into effect with promptitude.

We have no quarrel with those, whether members of Parliament or of the general community, who, while they, *in good faith*, support these general principles, have opposed, in a very determined manner, the objectionable portions of the bills in which they were proposed to be carried into effect. On the contrary, we consider this class of men as among the best friends to the measures themselves. But no one can have paid even a slight attention to the public discussions regarding them, without discovering that opposition to the *details* of these bills was often associated with opposition to their *principles*, and that this was found to be a very successful method of carrying on the warfare against those principles. We do not say that it was a dishonest method, for it was not *always* coupled with the pretence of friendship to them. Some have not scrupled to say that they are opposed to these bills, not only as they now stand, but in all forms, and that they will continue to oppose them in future sessions. We believe that the money clauses are at the bottom of the opposition. The opponents would have the advantage without paying for it, or their minds are not capable of perceiving advantage in any thing for which money *must* be paid. This may seem at first sight a severe judgment; but it appears to us to be just. The men we speak of do not seek to have the bills referred to a committee; they take no pains to cure their defects. But when a muster occurs for strangling them, they are sure to be in their places, whether these places be at Westminster or at minor boards in Scotland. This reckless spirit of opposition demands the strongest censures of all enlightened men, whether in the medical profession or not. Fortunately, no one can conceive that measures of such extreme usefulness are to be permanently baffled by such opponents. We trust, therefore, that this short-sighted policy is doomed to early disappointment; and that the friends of improvement will, ere next session, show so unequivocally their own determination, and the strength of their cause, as to secure for it an early consideration and a triumphant victory over all factious opposition.

We are far from being insensible to the defects which are to be found in these and all other legislative measures. In saying so we are entitled to point, as an evidence of our earnestness, to the opposition which we made in our last Number to some of the provisions of the Public Health Bill. In the next session we trust these important matters will not be lost sight of; and, in any case, we assure our readers that we shall be found at the post of duty. Meanwhile, it is the business of the members of our profession to do what they can to convince their countrymen of the reality of the interest which every British subject has in getting these bills passed into laws in as short a time, and with as few defects, as possible; and we cannot entertain a doubt that the profession, which has hitherto been guided by principles of enlightened humanity, will persevere in this course till their purpose has been effected.

CHLOROFORM IN HYDROPHOBIA.

We have received from Dr G. Smith, Assistant-Surgeon, Bengal Horse Artillery, a strong testimony to the efficacy of this anæsthetic in subduing the spasms and alleviating the sufferings of a boy affected with hydrophobia.

"The lad was kept alive rather more than two days; the spasms were much modified by the anæsthetic, and the impression left upon the practitioner's mind is, that the disease might ultimately have been subdued, had his stock of chloroform not most unfortunately failed him, just when perseverance was most required. One curious fact was observed, namely, that, in the intervals between the administration of the chloroform, the poor patient drank liquids most freely—to the extent of several pints—without any difficulty, and without any consequent recurrence of spasm."

We understand that in the case which recently occurred in Dalkeith, chloroform was also found to have a great, though only temporary, effect in subduing

the spasms. Though both these cases terminated fatally, we think the favourable effects of this agent worthy of being recorded, as a cautious trial of it in any future case, even if unsuccessful for the chief object of saving life, might prove the means of obviating much unnecessary suffering.

A case, in which hydrophobia is said to have been *successfully* treated by chloroform, has lately been published in America (see "American Journal of Medicine," for April); but as even the author of this case seems to admit of considerable doubts as to its true nature, and as these doubts are increased by a perusal of the case itself, we do not feel justified in laying it before our readers as a case of cure of this most formidable disease.

BOOKS RECEIVED.

Gream on the Misapplication of Anæsthesia in Childbirth. London. Churchill. 8vo. 1849. Pp. 71.

Official Documents, relative to the Trials of Warburg's Fever Tincture in Intermittent and other Fevers, instituted by order of the Austrian Government. Translated from the German. London. Parker. 1849. 8vo. Pp. 87.

Bouchardat—Annuaire de Thérapeutique pour 1849. Paris. Bailliere. 12mo. Pp. 304.

Plagge (M. W.)—Handbuch der Pharmakodynamik. Braunschweig. 1847. Pp. 678.

Dendy (W. C.)—Portraits of the Diseases of the Scalp, with the safest and most efficient Modes of Treatment. No. I. London. Highley. 1849. Royal 4to.

Esdaile (Dr)—Reports of the Mesmeric Hospital, Calcutta. 8vo. Pp. 7.

Journal of Psychological Medicine. Edited by Forbes Winslow, M.D. Nos. I. to VII.

Wardell (Dr)—On the Scotch Epidemic Fever. London. 1848. Pp. 114.

Braithwaite's Retrospect. Jan.—June. 1849.

Lee (Dr Robert)—Practical Observations on the Diseases of the Uterus. (With coloured Lithographic Plates from Original Drawings). Part II. Royal 4to. London. Churchill. 1849. Pp. 20.

Lee (Dr Robert)—On the Ganglia and Nerves of the Uterus. London. Churchill. 1849. 4to. Plates. Pp. 35.

Maclise—Surgical Anatomy. Fasciculus III.

Dalrymple—Pathology of the Human Eye. Fasciculus II.

Maunoir (M.)—La Porrette et Monte-Catini. Florence. 1848. 8vo. Pp. 255.

Smith (John)—Fruits and Farinacea the proper Food of Man. (Second Edition.) London. Churchill. 1849. 12mo. Pp. 342.

Gairdner (Wm., M.D.)—On Gout. London. Churchill. 1849. 12mo. Pp. 232.

Nasmyth (Alex.)—Researches on the Teeth. London. 1849. 8vo. Plates. Pp. 230.

Transactions of the Provincial Medical and Surgical Association. Vol. xvi. Part 2nd. London. Churchill. 1849. 8vo.

Report on Quarantine. General Board of Health. 12mo. London. 1849.

Parkes (Dr E. A.)—Inquiry into the earliest Cases of Cholera in London. London. 1849. 8vo. Pp. 28.

TO THE EDITORS AND PUBLISHERS OF OUR EXCHANGE JOURNALS.

We have particularly to request that our Exchange Journals be in future addressed to the Editors of the "Monthly Journal of the Medical Sciences," care of Messrs Sutherland and Knox, 23, George Street, Edinburgh.

German Journals should be forwarded to the house of Hartmann in Leipsic, care of Messrs Williams and Norgate, Henrietta Street, Covent Garden, London.

French and Italian Journals should be forwarded to the care of J. B. Baillière, Rue de l'Ecole de Médecine, Paris.

American Journals should be forwarded to Mr Wiley's American Library Agency, Paternoster Row, London.

Attention to these directions will spare the Editors of the "Monthly Journal" much inconvenience, as well as the frequent loss of journals incorrectly addressed to them.

NOTICE TO CORRESPONDENTS AND SUBSCRIBERS.

The very large number of Original Articles in our hands has obliged us to curtail the Retrospect by eight pages this month. Several Reviews have also been unavoidably delayed. Notwithstanding these arrangements, we are still obliged to request the indulgence of some of our Correspondents for another month, when we hope to have more space at our disposal. We intend to give a Summary of the "Cholera Reports" next month.

That the volumes of the Monthly Journal may coincide with those of the Retrospect, beginning in January and ending in December, it has been determined that the present volume shall run to December 1849. The usual Index, together with that of the Retrospect, will accordingly be given in December.

THE

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OF

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*Observations on the Occurrence of Spermatozoa in the Fluid of Hydrocele.* By T. B. Curling, Lecturer on Surgery, &c., at the London Hospital.

In May 1843 the late Mr Liston announced to the Royal Medical and Chirurgical Society of London, a discovery of considerable interest, in connection with encysted hydrocele of the testicle, namely, the occurrence of spermatozoa in the fluid contents of the cyst. They had also been observed, about the same time, by Mr Lloyd, of St Bartholomew's Hospital, who communicated to the Society the results of his observations in June of the same year. The circumstances under which these bodies occur in so singular a situation have not yet, however, been satisfactorily made out; and the subject is one which requires further investigation.

During the last six years I have met with spermatozoa in as many as twenty cases of encysted hydrocele; indeed, in the greater number of instances in which I have searched for them. They were found at various ages, from 30 to 75, and in cysts of all sizes, from that of a filbert to the largest which this form of hydrocele attains. The fluid, in some instances, contained these bodies in remarkable abundance; in others they existed sparingly. When very numerous they gave to the fluid an opaline opacity, or an appearance resembling cocoa-nut milk, which is so characteristic, as to enable the surgeon to predicate their presence from the appearance of the fluid alone without minute examination. If the fluid be allowed to remain at rest in a glass vessel, the spermatozoa subside to the bottom, rendering the lower portion more opaque than the upper. The fluid also exhibits slight traces of albumen when tested

in the usual way, which is not the case with the ordinary pellucid colourless fluid of encysted hydrocele. The spermatozoa were often as lively as in fresh semen. They were observed more frequently in the larger cysts than in the smaller. I once found them in fluids removed from two distinct cysts attached to the epididymis of a man about 60 years of age. I have detected them in the fluid from encysted hydroceles tapped for the first time, and also in the examination of small cysts connected with testicles removed after death. I removed from an old man, aged 75, who had encysted hydrocele for eight years, that had never been tapped before, as much as thirty-two ounces of fluid, which contained abundance of spermatozoa. They were also detected in fluid taken from a man aged 54, who stated that the tumour had existed for eight years, and had never been operated on.

Various opinions have been broached to account for the existence of spermatozoa in the encysted form of hydrocele of the testicle; but before alluding to them, I shall briefly describe the structure of these accidental cysts, the mode in which they are formed, and their relation to the excretory part of the testicle. The cyst is composed of a thin delicate serous membrane, lined with a tesselated epithelium, and is developed beneath the visceral portion of the tunica vaginalis investing the epididymis. Small cysts not larger than a pea, and even smaller, may frequently be found beneath the serous membrane covering the head of the epididymis, in which they produce a slight depression. I have seen as many as five and six perfectly distinct cysts connected with this part. Sometimes one or two small cysts are so imbedded in the substance of the epididymis that they cannot be recognised without dissection. They sometimes project the tunica vaginalis before them until they become so far separated from the epididymis, where they were originally developed, as to be attached only by a narrow peduncle, formed by a contraction of the connecting tunica vaginalis. Such is the mode of development of those small pendulous pedunculated cysts, containing a limpid fluid, often found hanging from the head of the epididymis. They never acquire a large size; indeed, I have seldom found them to exceed that of a currant. From their exposed situation they are liable to be ruptured, leaving vestiges consisting of fimbriated folds of membrane. So common are small cysts connected with the epididymis in the various states and stages I have described, that it is impossible to examine many testicles, especially of persons beyond the age of puberty, without finding them. According to M. Gosselin, they are liable to be developed from the period of puberty to the age of 30 or 35, but are rare at this period. After the age of 40 they are very common, having been met with by him in at least two-thirds of the testicles examined.[1] Now, when one or more of these cysts, instead

[1] M. Gosselin has given an elaborate account of the cysts connected with the epididymis in two papers published in the 16th volume of the "Archives Géné-

of becoming pedunculated, enlarge so as to form a tumour in the scrotum, they constitute the encysted form of hydrocele of the testicle. I have observed this description of hydrocele in all its various modifications, from the enlargement simply of a single cyst to the complication occasioned by the varied development of several. As a cyst enlarges, the testicle becomes displaced, and the epididymis flattened. The outer surface of the cyst is in close contact with the ducts of the epididymis, areolar tissue only intervening.

Mr Liston suspected that the presence of spermatozoa in encysted hydrocele might be explained by the circumstance, that the cyst was formed by dilatation of a seminiferous tube. This view, however, is obviously erroneous; for it has been shown that the cyst is quite distinct and completely separated from the ducts, though in close proximity to them. Others have supposed that these bodies were accidently introduced, owing to a wound of an adjoining duct in the operation of tapping; but this is disproved by their having been observed, as I have already stated, in cysts when tapped for the first time, and in cysts which have never been opened until after death. It has occurred to some, that the spermatozoa having been formed in the ducts of the testicle, had afterwards penetrated the walls and entered the cyst by transudation, or a sort of exosmose; but this is an hypothesis open to so many and such obvious objections, that it can scarcely be said to have met with supporters. Mr Paget suggests, as the most probable explanation, "that certain cysts, seated near the organ, which naturally secretes the material for semen, may possess a power of secreting a similar fluid."[1] Physiology does not warrant this supposition. The secreting tissue of the cyst is admitted to be different from that of the seminal duct. Now, we have no evidence that one texture can fully perform the office of another of different structure; yet the spermatozoa commonly found in these cysts are as perfect and as well developed as any taken from the vas deferens and vesiculæ seminales. M. Gosselin supposes that the presence of spermatozoa may be accounted for by a rupture of some of the seminiferous tubes, or efferent ducts—a rupture, perhaps, consequent on those obliterations of the tubes in the head of the epididymis which he has particularly described. The sperm having escaped from its natural passages, an accidental cyst gradually surrounds it, forming the cyst which furnishes a fluid in addition to its primitive contents: in the meanwhile, the ruptured duct or ducts, compressed

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rales de Médecine." He makes two varieties of them, the small and large; and states, that spermatozoa are found only in the latter. I cannot admit this distinction, as I believe the small cysts to be simply the early stage of the larger. M. Gosselin assumes the credit of having been the first to describe the small cysts. They were, however, minutely described by me in my work on the "Diseases of the Testis," published in 1843, of which M. Gosselin could scarcely have been ignorant, since he refers to my account of this form of hydrocele.

[1] Medico-Chirurgical Transactions, vol. 27, p. 401.

by the development of the tumour, become obliterated so as to leave no trace of the original alteration.[1] That this theory is untenable, may be shown—first, by the fact admitted by M. Gosselin himself, that minute cysts containing spermatozoa have scarcely ever been observed; whereas, if his view of their origin were well founded, we should, without doubt, occasionally find cysts containing these bodies in the early stage as well as in the advanced. Secondly, it may be objected, that after removing the fluid from one of these cysts by operation, that fresh secreted is often found to contain spermatozoa; in some instances, as numerous as before, which could not happen if an obliteration of the ruptured duct had taken place, as M. Gosselin supposes, and, as we may infer, would be the case, had an accidental cyst been formed around an extravasated fluid.

The explanation which seems to me the most reasonable and probable, is that which I offered shortly after the occurrence of spermatozoa in encysted hydroceles was first discovered[2]—viz., that their presence is probably owing to the accidental rupture of a seminal canal, and the escape of its contents into the cyst of an existing hydrocele. The close proximity of the efferent tubes, the slight texture of the ducts and delicate walls of the cyst, and the liability of the part to contusion and injury when a swelling, even of moderate size, exists, seemed to me to favour this view. The circumstance that spermatozoa are never found in very small cysts, show that they are not originally formed there, but are a subsequent addition to their contents. Since my attention has been drawn to this subject, I have investigated the history of the cases of encysted hydrocele containing spermatozoa which came under my notice. In nearly all instances the patients assured me that the swelling had gradually formed after an injury to the testicle; and in two cases, it was clear, that a small cystic swelling had long existed in a stationary state, but after a slight blow had begun to enlarge. I strongly suspect that in these cases, a duct had been ruptured by the contusion, and that the irritation consequent on the injury, and perhaps on the addition of such lively bodies as spermatozoa to the fluid contents of the cyst, had led to its further development. I have, it is true, failed in establishing the fact of a communication between the duct and the cyst by anatomical examination. In two instances of large encysted hydrocele containing spermatozoa, which I had an opportunity of examining, I injected the vas deferens with coloured size, but the duct was so clogged with semen that the fluid could not be made to reach the head of the epididymis, to which the cyst was attached. In a preparation of large encysted bi-locular hydrocele containing spermatozoa, shown me by Mr Bush, of the Dreadnought Hospital, the vas deferens had been injected with mercury, but none of the metal reached the upper part of the epididymis. The cyst evidently arose from the head of

[1] Archives Générales de Médécine, 4 Série, t. xvi., p. 178.
[2] Practical Treatise on Diseases of the Testis. Appendix, p. 541.

the epididymis, and was embedded a little in its substance. In these cases, no opening communicating with a duct could be discovered on examination of the cyst, but this is not surprising, since the communication must be extremely minute, so as readily to escape detection, or it may even have been obliterated.

Spermatozoa are stated to have been found in some two or three instances, in fluid removed from the tunica vaginalis. It is not improbable, that these cases may have been encysted hydrocele mistaken for simple. The diagnosis is sometimes very difficult, and in the cases of the cyst examined by Mr Paget, this error was made before death by an hospital surgeon. I have, however, found spermatozoa in the sac of the tunica vaginalis, and the following case will account for their presence :—

A man, aged 54, died in the London Hospital of disease of the kidneys, of one of the ureters, and of the bladder, which appeared to be consequent on a severe blow on the loins, about six weeks before. The tunica vaginalis of one of the testicles contained two ounces and a-half of slightly opaque fluid, in which a few spermatozoa were found. There were three small cysts containing fluid immediately connected with the epididymis, and also at one spot an irregular ragged membranous appearance, evidently caused by the rupture of a cyst. It is most probable that the spermatozoa had escaped from this cyst, which may indeed have been burst at the time of the injury.

I have examined the fluid from the tunica vaginalis in a large number of instances, without finding these bodies, and I believe their occurrence in the common form of hydrocele to be extremely rare.

The presence of spermatozoa in hydrocele is entirely of pathological interest, as it does not influence the results of treatment.

New Broad Street, London,
June 1849.

ARTICLE II.—*Case of Preternatural Anus.* By DOUGLAS MACLAGAN, M.D., F.R.S.E.

(*Read before the Medico-Chirurgical Society of Edinburgh, May* 2, 1849.)

MISS ———, aged twenty-six, early in the summer of 1840, received a smart blow nearly on the umbilicus, from a sportive movement of her brother, a child six years old. She made little complaint of the injury at the time, but in the course of the succeeding six months she experienced frequent attacks of pain in the site of the blow, and also was occasionally affected with irritation of the bladder; this latter symptom, however, appears to have depended upon causes unconnected with the injury in question. For these ailments she requested the advice of her medical attendant, the late Dr David Hay. Dr Hay found in the seat of injury a circumscribed hard mass, which appeared to be partly in the abdominal parietes, and partly in the textures within. It was leeched with some benefit; but symptoms of the formation of an abscess subsequently manifesting themselves, poultices were applied; and, in November 1840, six months after the infliction of the blow, Dr Hay opened a large abscess by an incision over the umbilicus. In the pus thus discharged, Dr Hay, either at the time of opening the abscess or very shortly afterwards, observed a carraway seed; and, although there was no fæcal matter in the discharge, he at

once inferred that there was a communication between the abscess and the bowel. The subsequent discharge of fæces by the wound soon showed the correctness of this opinion. The case was seen at this time by Mr Syme, and the opening of the abscess was dilated, by which a large quantity of very offensive matter was discharged, and the patient much relieved. The wound thereafter gradually got into a callous state, the contents of the bowel were constantly passing through it, and a fæcal fistula was thus established at the umbilicus.

During the next three years the patient improved in condition both locally and generally. Her health was re-established, and the orifice became contracted, so that, by means of a bandage and compress, she could retain the fæces effectually; and, notwithstanding the nature of her complaint, was able to mingle a little in society, and take active exercise. Gradually, however, some change was observed in the condition of the parts. The orifice became smaller, and discharged fæces less freely, but still the greater part of the excrements passed out by the opening; for, although there were some discharges per anum, they were irregular in occurrence, and scanty in amount. There was, also, a gradual increase in suffering connected with the fæcal discharge; there were frequent pains—"forcing pains," as the patient called them—at the time when the contents of the bowel escaped; and deeper seated colick pains were experienced at the same time, which were often very severe.

The opening at the umbilicus had gradually assumed an aspect which fully entitled it to the appellation of a preternatural anus, as it bore a very close resemblance in appearance to the natural orifice of the rectum. It presented, in the place of the umbilicus, a circular funnel-shaped depression, with puckered folds of the skin, which, meeting together, concealed the real orifice, which was a narrow slit, about half an inch in length. There was usually a small florid granulation at the upper end of the slit, which, as often as it tended to cicatrise, was torn up by the expulsive efforts. The patient was thin, but not emaciated; had a keen appetite, and good digestion; and a remarkable vivacity and cheerfulness of spirits. Such was her condition when she came under my care in 1844, at the period of Dr Hay's death.

The occasional and now more frequent escape, however, of some of the feculent matter, in spite of the most careful application of the bandage, and the consequent insecurity which the patient felt from this cause, her gradually losing a little flesh and strength, and the loathsome nature of her ailment, made it very desirable to obtain, if possible, the closure of the opening.

An attempt was first made to effect this by the actual cautery, which was applied several times, an interval of some weeks being allowed to intervene between the renewal of the application, so as to permit of due contraction of the cicatrix after separation of the sloughs. No ultimate benefit, however, was obtained from this, and my patient, therefore, in the summer of 1845, readily assented to a proposal to close it by operation,—Mr Syme having again seen her in consultation, and approving of making the attempt. To accomplish this, a small incision was made through the skin, extending about half an inch above and an equal extent below the depression in the integuments, and the edges of the cavity between these two points were cut away, the whole being made raw from the level of the surface down to the edges of the slit leading to the bowel. The wound was closed by three points of twisted suture, the needles used being long, and passed deeply through the textures, so as to bring as much of the incised portions as possible into contact. At the lower end of the incision, where a fold of the abdominal parietes buried the needle in the skin, and caused much complaint on the part of the patient, I was obliged to withdraw the needle for the twisted suture, and insert a stitch instead. The patient's bowels had been evacuated the day previously by a gentle laxative, and the food was restricted to strong soup, and amylaceous matters, so as to leave, for a day or two, as little excrementitious matter as possible, to pass along the intestines. All promised very well for the first day, the edges of the wound

kept in good apposition, and nothing passed out. But in less than forty-eight hours from the time of the operation, the whole scheme was signally defeated. After a great deal of pain within the belly, an irresistible attack of forcing pains came on, fæcal matters escaped again, and with much pain, from between the sutures, although they were very close together; the stitch at the lower end of the incision tore through the textures which it held; and, seeing that there was no hope of a primary closure of the wound, and that the patient was in great pain from the retention of fæcal matters, I was reluctantly compelled to remove my sutures.

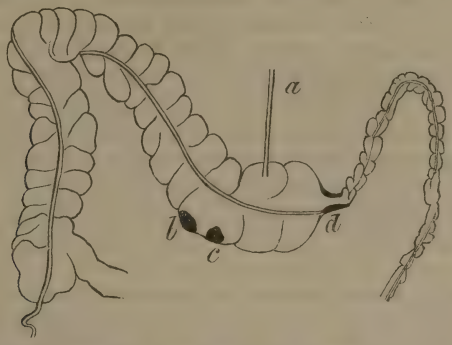
It was clear now that there was some impediment to the passage of the fæces along the course of the bowel at a point beyond that where it opened through the parietes. All hopes of closing the external aperture were, therefore, now abandoned, and attention was directed, after the healing of the wound which I had made, to mechanical means of restraining the escape of fæces. The yielding nature of the abdominal parietes renders it no easy matter to adjust an apparatus which should, on the one hand, make sufficient pressure to retain the fæces, and, on the other, avoid causing pain to the patient. That which ultimately answered best was a steel girdle fastened round the chest, from the point of which there projected downwards a curved steel spring bearing a conical pad of cork covered with leather, which fitted over the orifice, a piece of silk with oil skin being worn beneath to keep the compress clean. This succeeded so far that the patient was again able to move about and take active exercise with considerable freedom. Up to the summer of 1847, she continued very much in this condition, but it was observed that, whilst the discharge of fæces by the orifice could be more easily commanded, there was more pain in the bowels, and the least irregularity in their action caused much irritation. The points now chiefly kept in view, in the way of treatment, were to obviate occurrences of this kind, and to diminish the chances of solid matter passing along the bowel and escaping by the orifice, a circumstance which always occasioned great suffering. With these objects the diet was restricted to those kinds of food which, with high nutritive power, combined ready assimilation. Strong soups, plain butcher meat, fish, and game, were allowed; vegetables were forbidden, and, indeed, the patient soon came to learn, that even a small portion of potato, or of vegetable matters, which were apt in part to escape solution in the stomach, generally caused an increase of pain at the seat of disease. On one or two occasions about a spoonful of well-boiled spinach was allowed to be taken, with the avowed purpose of observing, from its green colour, the rate at which matters reached the opening into the bowel. It was observed that the green colouring matter never made its appearance till eight or ten hours after it was swallowed. Had it not been otherwise sufficiently evident, from the exemption from inanition which the patient enjoyed, that the bowel involved in the disease was far down the alimentary canal, this little physiological experiment would have sufficed to confirm the supposition. If it had been a portion of the small intestine which was involved, we might reasonably have expected the green vegetable matter to have made its appearance at the orifice in a shorter time. There never appeared to me to be any reason to doubt that the part of the bowel engaged was the transverse colon.

In the summer of 1847 the patient had gone to reside for some time on the west coast, and had there begun to complain of some new symptoms, which were aggravated and apparently brought to a head by her taking a walk of upwards of six miles. On her return to town I saw her, and found that an abscess had formed in the right groin, immediately above Poupart's ligament, and nearly in the situation of the inguinal canal. In the centre of the inflamed skin over the abscess, a circular piece of integument was distinguished from the rest, by its being of a blue colour, obviously on the point of ulcerating through; and on touching this with the finger, the feeling of air moving below the skin showed that this abscess communicated with the bowel, and that a fresh opening was likely soon to form there. It was obviously not a matter to interfere with

surgically; it was therefore merely covered with warm water dressing, and left to take its own course. After a few weeks the skin ulcerated, and a circular opening, half an inch in diameter, formed, through which purulent and feculent matters were discharged abundantly. This new opening obviously communicated with the intestine at a point higher up than the original preternatural orifice, for now that the fæcal matter found exit at the groin, little or none was discharged at the umbilical opening. The whole skin in the groin was very tender, and the least pressure on it caused pain. No attempt, therefore, was made by mechanical means to prevent the exit of fæcal matter, and the treatment was limited to occasional warm fomentations, and strict attention to keeping the parts clean. After the patient had continued in this state for about a year, a similar series of appearances began to manifest themselves in the left groin, but at a point much nearer to the symphisis pubis. An abscess could be felt there readily, which ultimately extended downwards on the front of the thigh. It contained flatus, and there was a circular piece of integument over it, blue, very thin, and ready to ulcerate. This third abscess was in direct communication with that in the other groin. By pressing gently upon it, pus and flatus could be made at once to escape from the opening there. It also obviously communicated, though less freely, with the original opening at the umbilicus, as a small quantity of matter was generally forced out there when pressure was made on the left groin. The skin in this third seat of disease never ulcerated through. Though often so much distended with matter and flatus, as to appear as if on the point of bursting, it was always relieved by their escape from the other groin. The patient, therefore, remained, so far as the local disease was concerned, in the state described above, till her death in February 1849. For a couple of months before that event, there were indications of tubercular deposit in the apex of the left lung. The pulmonary disease, however, made little progress, and she eventually sunk from gradual exhaustion, accompanied, during the last two weeks of her life, by very severe periodic headache and hysterical spasmodic attacks, which were partly subdued by large doses of morphia.

The autopsy was performed by Dr W. Gairdner, in presence of Dr Bennett and myself. The morbid appearances will be best understood by tracing the colon, to which organ I restrict my account of the dissection, as the rest of the body did not present any features of interest. The ascending portion of the colon was healthy, both in texture and position. Immediately, however, where it became, or rather should have become, the transverse colon, it was bent downwards upon itself, and, instead of lying across the abdomen, it passed obliquely downwards to the supra-pubic region, where it was firmly attached to the abdominal parietes. From this point it again turned obliquely upwards towards the left hypochondriac region, and then it passed downwards in the natural situation of the descending colon to the sigmoid flexure. This bending down of the transverse arch of the colon to the lower part of the abdomen, thus gave to the gut very nearly the form of a capital M; the upper angle on the left side, however, being considerably lower than that on the right. The connection of the bowel with the original external opening in the site of the umbilicus was first examined. Instead of finding the fistula leading directly backwards into the bowel, as would have been the case had the transverse colon occupied its natural situation, the external umbilical opening in the parietes was found to lead downwards to the portion of the bowel which passed upwards from the pubic region towards the left angle of the colon, by a narrow canal, of about the diameter of a small quill, and about three inches in length. This canal lay between the muscles and their peritoneal lining. Immediately to the left of the point where this fistula communicated with the gut, the bowel was thickened and hardened, its walls having assumed the form of a tough hard ring which admitted only the point of a finger, and beyond this part the colon was, as might have been expected, empty and contracted, but in no respect altered in texture. By reflecting the abdominal parietes downwards over the

pubis, and opening the posterior wall of the bowel, where it was attached in this region, the connection of the bowel with the fistula in the right groin, and with the abscess in the left, was ascertained. The fistula in the right groin passed directly into the bowel by an opening equal in size to that which existed in the abdominal parietes. Another opening in the bowel, of equal size, communicated, through the adherent abdominal walls on the left side of the mesial plane, with a large abscess which contained a mixture of pus and fæces. In the groin, above Poupart's ligament, this was covered merely by the skin; but it passed downwards beneath Poupart's ligament into the cellular tissue of the thigh, and a part of its posterior wall was formed by the horizontal ramus of the pubis, which was there denuded of periosteum. The two openings in the gut leading respectively to the right and left groins, and which were situated on the anterior wall of the bowel, were within half-an-inch of each other, and from the small portion of the intestine which lay between them, and which adhered firmly to the parietes of the abdomen, a bridle of mucous membrane extended like a cord backwards to the posterior wall of the bowel, forming a small arch, beneath which the finger could be passed, by pushing it into the opening in the right groin, passing it into the bowel, and outwards again into the abscess on the left side. With the exception of complete emaciation, and some small tubercles in the left lung, the other organs were healthy. The accompanying diagram will render the above description of the post-mortem appearances more intelligible.



a, Canal leading from colon to the orifice at the umbilicus.
b, Opening in the bowel discharging through the orifice in the right groin.
c, Opening in the bowel communicating with the abscess in the left groin.
d, Thickened and strictured portion of the bowel.

In speculating upon the course by which this case had arrived at the condition in which it was found at death, the following appears to be the probable manner in which these morbid alterations had been developed. The primary disease was undoubtedly connected with the blow which the patient had received. Probably the transverse colon was distended at the time of the accident, and thus was more directly injured by the contusion. The bowel had inflamed, become thickened and strictured, and at the same time had by its peritoneal coat acquired an attachment to the abdominal parietes. The formation of the abscess, after a lapse of some months, took place, I should be inclined to believe, from within outwards; the passage onwards of the contents of the bowel being impeded, and

these becoming more or less accumulated there, ulceration took place, and, as the bowel was adherent to the parietes, abscess formed and gradually made its way to the surface. The frequent occurrence of such abscesses in the neighbourhood of the cœcum, from impaction of foreign matters in it, and the subsequent occurrences in the present case, seem to warrant this conclusion. The progress of the morbid condition of the colon would probably be suspended during the time that the feculent matters found a ready exit through the preternatural opening; but as this gradually contracted, and as the stricture in the bowel probably contracted also, accumulation in the transverse colon would again occur. The horizontal portion of bowel, by the weight thus thrown upon it, was gradually dragged downwards. Its fistulous opening through the abdominal walls thus gradually became elongated, and from a short passage, leading directly into the bowel, it became extended into the quill-shaped canal described above. When the bowel had been doubled downwards as far as it would stretch, a similar set of phenomena had again occurred, the bowel had again become adherent, again ulcerated through, and formed the fresh abscess and subsequent opening in the right groin; another opening had formed under this, towards the left side, and was prevented from forming a third fæcal fistula only by the freedom with which the contents of the bowel and of the abscess found their exit at the right groin. The formation of the bridge of mucous membrane which extended from between these openings towards the inferior and posterior wall of the bowel was most probably due to one of the natural folds of the bowel being involved in the adherent portion, and its base being perforated by ulceration, thus leaving a cord-like portion stretching partially across the gut.

The foregoing case holds out no encouragement to operative interference under similar circumstances. Fistulous openings into the bowels, in a majority of instances, ultimately close spontaneously; but when they obstinately refuse, as in the present instance, to do so, there probably exists some mechanical impediment which prevents the fæces passing along the gut, and obliges them to issue by the preternatural opening. In fæcal fistula resulting from hernia, the obstructing cause, which is generally the septum formed by the double fold of the bowel, together with contraction of the lower orifice, may be removed by appropriate treatment; but in cases unconnected with hernia this is not the state of matters. The obstructing cause is either beyond reach, or not of a nature to admit of removal; and under such circumstances the surgeon must content himself with mechanical restraint of the fæces, attention to cleanliness, and regulation of the bowels by diet and medicine.

66, Frederick Street, Edinburgh,
July 1849.

ARTICLE III.—*Contributions to the Pathology of the Heart.* By HALLIDAY DOUGLAS, M.D., Fellow of the Royal College of Physicians, Physician to the Royal Infirmary, Lecturer on Clinical Medicine, Edinburgh.—(*Continued from p.* 816.)

ANEURISMS of the heart, especially certain forms of the lesion, illustrate very remarkably the influence of dilatation in causing the leading features, and the ultimate evils, of cardiac diseases. The application of the term aneurism is here restricted to a dilatation which is partial, and more or less intimately connected with a degenerated or transformed state of the walls of the heart.

I have found it impossible to compress the cases so much as is desirable; because, though, in this paper, only certain points in these cases are discussed, it appears necessary to give an accurate and detailed account of the facts, that they may be available in reference to other questions than those involved in the present inquiry.

I ought to mention that I was prevented being present at the dissections of the two succeeding cases (V. and VI.;) but, in both, the hearts were preserved for my inspection.

CASE V.—*Aneurism of the left Ventricle, true and sacculated; Dilatation of the Heart; Hypertrophy; Fibrous Transformation of the Muscular Substance; Death by Apnœa.*

Margaret Sheriff, æt. 30, married, was admitted into hospital on the 20th November 1847. She was labouring under most urgent dyspnœa and extreme general dropsy. Her lips were livid; the pulse was small and feeble; the abdomen was swelled and tender; urine scanty, density 1015, albuminous. She wore a tracheotomy tube.

History.—She stated that troublesome symptoms had arisen within a year; but she could not give a distinct account of their progress. Tracheotomy had been performed many years previously.

In the preceding January she was in hospital under the care of Dr George Paterson, to whom I am indebted for an account of the symptoms and physical signs which then existed.

At that time she suffered chiefly from dyspnœa, cough, and occasional palpitation. Urine was copious, of density 1015, and free of albumen; pulse was 84, small and weak.

She stated that, with the exception of slight catarrhal symptoms, which she ascribed to the state of the trachea, she had had good health till the preceding November (1846), when, in consequence, as she supposed, of exposure to the weather, aggravation of her chest symptoms, with pain, occurred. Slight swelling of the limbs occasionally existed, and scanty urine. The account of the physical signs, as then observed, will be incorporated with the statement of those noticed by myself in November. She remained only five days in hospital, and was dismissed by her own desire.

From that time she was subject to an often recurring dropsy, which yielded to ordinary diuretics. She was under the care of Dr Thomas Keith, who says that the urine was free of albumen; but he did not examine it within three months of her admission; and about that period the dropsy yielded less readily, and latterly resisted the diuretics.

Physical Signs.—These were ascertained with difficulty, from her distressed and exhausted state, and from the intensity of general bronchitis. They dif-

fered from those observed in January, only in the *force* of the heart's impulse, which had become much less; for it was now feeble compared with its extent, and with the extent of the præcordial space dull on percussion. The following was the state of the signs as observed in January, and corroborated in November :—

The præcordial space dull on percussion was extended. The impulse of the heart was diffused, originally of good strength, but it became feeble compared with the extent of the præcordial space. The radial pulse was small. A murmur accompanied the first sound, and was most distinct at the apex. The usual signs of general bronchitis existed, these latterly were intense.

Progress.—Gradual and progressive exhaustion followed, without any material change in the symptoms, and she died on the 26th.

Post-mortem Examination.—Dropsy was general. The larynx, trachea, and lungs presented no appearance which was unusual under the circumstances.

The heart was enlarged; it had an irregularly globular form, with two circumscribed tumours, the size of walnuts—one at the apex, the other an inch and a half to its left. The organ, with a small portion of adhering pericardium, weighed nearly 18 ounces. On laying open the left ventricle in the usual manner, by separating its left wall from the septum, its capacity was found to be greatly increased,—the increase being in a great measure due to a diffused, but partial dilatation or *true* aneurism of the left wall. The endocardium, especially of the dilated left wall, had an opaque pearly appearance.

The tumours of the apex proved to be two well-formed *sacculated* or *false* aneurisms. They communicated freely with the ventricle,—the one in the precise situation of the apex, by a wide orifice; the other to the left, by a narrow circular opening. The cavity of the latter was diminished by a dense coagulum in layers; and the rim of its orifice, and the neighbouring tissues, were dense and semi-cartilaginous. One lip of its orifice was formed by one of the reticulated muscular bundles which had undergone a partial fibro-cartilaginous transformation. The sac at the apex and the neighbouring parts had a more fibro-membranous structure. Both sacs, as well as the ventricle, contained loose coagula. Their lining was smooth and continuous with the endocardium. Their walls were about a line in thickness.

In the posterior bundle of fleshy columns, and at their base, there was an irregular digital depression or small aneurism; it had a smooth lining, which was continuous with the endocardium. The leaves of the mitral valve were not appreciably altered; the tendinous cords were slightly shorter than usual.

The muscular substance of the heart had undergone extensive transformation of structure. This was most marked around the orifices of the sacculated aneurisms, and in the dilated or aneurismal left wall. In the vicinity of the sacculated or so-called false aneurisms, the transformation was semi-cartilaginous. In the left wall of the ventricle the transformation was fibrous in a very marked degree; the muscular substance was superseded by this dense grey fibrous structure to the extent of nearly three inches; and the average thickness of the wall was not thereby lessened, as it equalled half an inch. The pericardial surface of this portion of the heart was marked by adhesion, and a ragged fibro-cellular patch; the corresponding part of the endocardium was pearly and smooth, almost fibrous in appearance, and the usual reticulated muscular bundles were wanting :—The reticulated muscular bundles ceased near the base, behind the outer leaf of the mitral valve, by an abrupt transverse line, which formed the upper boundary of the diffuse aneurismal dilatation of the left wall; and this line corresponded precisely with the abrupt line of demarcation which separated the transformed fibrous tissue from the natural muscular substance above. In the substance of this fibrous texture there existed, here and there, the remains of muscular fibre in minute portions, pale and disconnected.

The adhesion of the pericardium was anterior, and to the left of the anterior coronary artery; and it extended only partially to the prominent surfaces of the

aneurismal sacs at the apex. To the left of the adherent part, and corresponding to the transformed portion of the left wall of the heart, a patch of ragged fibro-cellular tissue existed on the cardiac surface of the pericardium.

The right ventricle was large and dilated; it extended to the apex of the heart; its walls were decidedly thicker than natural.

CASE VI.—*Sacculated Aneurism of the Left Ventricle; Dilatation of the Heart; Hypertrophy; Incompetence of the Mitral Valve; Fibrous Transformation of the Muscular Substance; Pleurisy; Death after Progressive Exhaustion, but unexpected.*

Anne Stewart, æt. 25, a plethoric woman, of dissolute habits, and broken constitution, was admitted on the 14th March, 1849. She suffered from extreme dyspnœa, cough, frequent palpitation, and uneasiness, with sense of sinking, referred to the scrobiculus cordis, and epigastrium. Her face was puffy and slightly congested; her lips were livid; the pulse was small, 129.

History.—The account she gave of her symptoms could scarcely be depended on. She had been liable to chest complaints, with palpitation on exertion, for two years. Tracheotomy had been performed five years previously: and the tube was worn for two years thereafter.

Physical Signs.—The heart's action was excited and violent; the impulse was more remarkable for its *extent* than its *force*. The præcordial space dull on percussion, was extended; but its precise limits were obscured by mammary development, &c. The sounds of the heart were free of murmur, over the upper part of the sternum. At the apex, a murmur existed which was subsequently ascertained, when the action of the heart moderated, to accompany the first sound.

Percussion of the chest elsewhere was resonant, except in the inferior right dorsum, where there was incomplete dulness.

The sounds of respiration were harsh, but feeble in the inferior right dorsum. Some wheezing and submucous rattle prevailed in both sides of the chest.

Progress and Treatment.—Mild laxative medicine was given; leeches were applied to the scrobiculus cordis, and she was confined to bed. The heart's action was immediately moderated; and she felt relieved. For about ten days she improved, slightly but decidedly. The urine, however, was obstinately scanty, of the density 1015, and albuminous. No dropsy had as yet appeared. The pulse was always small, but distinct and regular, usually 120 in the minute. Paroxysms of urgent dyspnœa now supervened; cough became troublesome; and she began to manifest a preference for resting on the right side; other signs of pleural effusion on the right side were also developed. She was blistered, with immediate relief; and the paroxysms did not recur for several days.

In the beginning of April, her complaints became rapidly more urgent. Dyspnœa, palpitation, sense of sinking with epigastric pain, occasional sickness and vomiting, occurred. Treatment afforded only temporary and slight relief. Increasing weakness became apparent; the pulse was steadily losing strength; and œdema made its appearance in the lumbar region and feet. From this time she lost ground. She had great general distress; and during the last ten days of life, she had unceasing, restless, wandering delirium, with great exhaustion; and the pulse was irregular. She expired, unexpectedly, on the 17th April, immediately after conversation with the nurse. She preserved a degree of intelligence, when roused and addressed, to the last.

Post-mortem Examination.—Dropsy existed, chiefly in the lower extremities.

Both lungs were partially adherent; and the right pleura contained above half a gallon of turbid serum, with lymph in loose flakes, and adherent membranous layers coating both pleural surfaces. This lung was compressed, not condensed.

The heart was large, and more or less globular; it weighed 14 oz. Exter-

nally, on the posterior surface of the left ventricle, close to the septum, there was a diffused oblong tumour, occupying the two upper thirds of the ventricle, and bounded beneath by a transverse sulcus, in which there was a small adhesion of the pericardium. The cavity of the left ventricle was dilated ; the external tumour depended on a circumscribed aneurism as large as a bantam egg. From its situation, the walls of this aneurism were formed partly by the septum posteriorly, and partly by the left wall of the ventricle. The posterior edge of the mitral valve and its tendinous cords stretched across the orifice, or communication of the aneurism with the ventricle; and this communication was large and free. The aneurism contained loose coagula, such as filled the ventricle. The sac projected chiefly at its lower part, and downwards ; its walls were uniformly thin,—about a line. Within the sac, on its upper part or roof, there was a patch half-an-inch in diameter, depressed, and forming, as it were, the commencement of a saccule within the aneurism. Its surface was coated with a grey friable adherent membrane ; its edges were sharp, and discoloured ; but loss of substance by ulceration could not be satisfactorily demonstrated.

The leaves of the mitral valve were opaque, not thickened, they were widely parted : and the tendinous cords were unusually short. The muscular substance of the heart was generally, but slightly and irregularly hypertrophied. Posteriorly, from the lower boundary of the aneurism to the apex, the muscular substance was incompletely transformed into a fibro-elastic like tissue. In this transformed tissue, there existed several minute lardaceous tumours ; and one of the intermuscular sulci was dilated into a commencing aneurism.

The right ventricle was dilated ; and extended to the apex of the heart. Its walls were uniformly, but only slightly, thickened. The fleshy columns and reticulated muscular bundles were hypertrophied.

The liver was congested, and had the nutmeg appearance.

The kidneys were congested ; they were little changed in their bulk ; but they were pale in their cortical portion ; the pyramids were of a dark red colour, with marked and abrupt line of demarcation from the cortical portion.

Remarks.—The facts of these cases suggest three distinct questions. The diagnosis of cardiac aneurisms is confessedly most uncertain ; and, the physical signs in both these cases having been observed with some care, we have to inquire whether any sign or series of signs existed, which may be regarded as distinctive of cardiac aneurisms. Secondly, in regard to the more immediate object of this memoir, these cases indicate the relative influence of true dilatation of the heart, and of the partial dilatation or aneurism in causing the secondary lesions on which the progress and result of the cases depended. In the third place, the lesions of the heart in these cases afford some information regarding the anatomy of this disease, and the mode of origin of cardiac aneurisms.

Before entering upon these questions, it will be well to observe the *forms* which the aneurisms assumed ; because the different forms are intimately related to differences in the physical signs, as well as in the secondary pathological effects. Cardiac aneurisms have been distinguished according as they do or do not cause external deformity of the heart ; but I cannot see any practical good arising from such a distinction. They have also been distinguished into acute and chronic ; and this distinction has, perhaps, a more sound basis ; but it involves the question of the inflammatory origin of these aneurisms, which is less directly related to the immediate object of this

memoir. Besides, the present series of cases indicates the necessity of distinguishing cardiac aneurisms according to differences which are physical rather than pathological. In the present cases, the aneurisms presented two distinct forms; and they were not more distinct in form than they were in their physical signs, and in their pathological effects. The one is circumscribed, limited, and more or less *sacculated*, with or without a narrow orifice, and was well exemplified by the aneurisms at the apex of the heart in case V. This form has been designated the *false* or false consecutive aneurism, both of which names are objectionable, as they involve a theory of which the evidence is not very complete. The other form, as exemplified in case VII., and also in the left wall of case V., is diffused, having no very defined separation from the ventricle, and forming, in a more correct sense, a partial dilatation of the cavity, or *true aneurism*. In the remarks which follow, I shall observe this distinction, which has an important bearing on the living pathology of the disease. The former is designated the *sacculated* aneurism; the latter the *true* aneurism of the heart. It must not, however, be supposed that cardiac aneurisms always present the one or other of these forms. Case VI. exemplifies the combination of the diffused and circumscribed form in such a way as to justify a third or intermediate class, which may be designated the *mixed* aneurism of the heart. This subdivision is of minor importance; because, evidently, aneurisms of this form will induce results differing from those of the two primary forms only in degree.

In neither of these cases did there exist any physical sign which is not familiarly known to arise in other forms of disease of the heart. In both cases, there existed a murmur with the first sound, which, however, was not to be distinguished from the murmur of mitral incompetence. In the woman Stewart (case VI.), from the appearances described, there can be little doubt, the mitral valve was incompetent, and gave rise to the murmur in question. In the woman Sheriff (case V.), the post-mortem appearances did not indicate incompetence of the mitral valve; and the endocardial murmur with the first sound at the apex, must be attributed to the aneurisms in that situation. The only peculiarity noticed in the murmur was its greater distinctness at the base of the heart than is usual, with murmurs loudest at the apex. This, however, does occasionally occur in murmurs from mitral incompetence; and a circumstance so trivial and uncertain is really unimportant.

It has been suggested, that under possible circumstances, a double "to and fro" murmur might occur in aneurism of the heart. I am not aware that a double murmur has ever been observed; it at no time existed in these cases; and the form of the sacs, as well as their narrow orifice, appeared fitted to create the double murmur, if its existence were possible:—A double "to and fro" murmur would perhaps assist in distinguishing these aneurisms, while it is impossible to distinguish the single bellows murmur, having its seat

in the orifice of an aneurism, from one having a very different source.

The other physical signs in these cases were identical with those which are known to characterise hypertrophy and dilatation,—the latter especially is known to preponderate from the feebleness of the cardiac impulse, the horizontal extent of the præcordial space dull on percussion, the softness of the radial pulse, and from the mutual relations of these signs to each other; and these signs must of necessity arise in cases, such as the present, in which the following conditions existed :—An extensive degeneration of the muscular substance of the heart; an unyielding and unwieldy state of the organ, arising out of the rigid and deformed state of its walls; and, above all, the increased capacity which must result even from partial dilatation.

These statements are not intended to imply that an endocardial murmur may not arise in connection with a cardiac aneurism, which is neither sacculated nor narrow in its orifice. But it does appear that the murmur arising in the sacculated aneurism is not to be distinguished from that arising from incompetence of the mitral valve; and that the signs of the true aneurism, or partial aneurism, are more or less identical with those of true dilatation. Besides, in both these cases, true dilatation did exist as a complication of the aneurisms; and its signs were of necessity present. Indeed, it is probable that the signs of cardiac diseases in such cases will become decided only when the degree of partial dilatation is great, or when true dilatation is superadded.

In reference to the second question proposed, the secondary effects of the heart-disease in these cases were referable to the dilatation. The sacculated aneurism appears to have been comparatively uninfluential; and the true aneurism, or partial dilatation, acted injuriously only in so far as it approached in degree to the true dilatation, causing lividity, dyspnœa, dropsy, &c., which are well known, and have been shown in the first section of this memoir (cases I. to IV.) to depend on an obstructed circulation from preponderating dilatation.

Let it be borne in mind that, in both these cases the partial dilatation was of such a form as materially to increase the capacity of the ventricle; and to produce physical effects altogether undistinguishable from those of true dilatation. In both these hearts also more or less general or true dilatation of this and the other cavities had actually taken place. It is reasonable, therefore, to infer that the embarrassed circulation and its usual effects, lividity, dropsy, dyspnœa, &c., which we have seen to arise from a preponderating dilatation, depended upon the same cause in these cases. On the other hand, the circumscribed sacs existed in such a form as to cause no material increase of the capacity of the ventricle in *one* of the cases only (case V.); and the effects of obstructed circulation which were more marked in this patient, depended on the greater

amount of the diffused partial dilatation of the ventricle, and on the degree of the true dilatation in that case. We cannot, therefore, attribute to these sacculated aneurisms, effects so formidable; which are, moreover, perfectly accounted for by the otherwise dilated heart.

We have observed that, when the effects of dilatation once fairly set in, the progress is uncontrollable, and the case not usually very lasting. In the woman Sheriff (case V.) these effects appear to have arisen above eleven months before the fatal event; and this must be regarded as an unusual length of time for a patient to survive the well-marked effects of dilatation. From the fact, however, that she remained only five days in hospital, when she was first admitted in January, apparently labouring under these effects of dilatation, it may be assumed that the circulation had not as yet become obstructed in a very great degree. It cannot be precisely determined what the apparent degree of dilatation was at that date; but, from Dr Keith, we have ample evidence that, long subsequently, and indeed within a comparatively short period of death, an unfavourable and rapid change had occurred in the symptoms attributable to the dilatation:—The dropsy increased, and was uninfluenced by treatment; diuretics failed of the effect they previously produced; and the urine became scanty and albuminous.[1] In the woman Stewart (case VI.), the apparently recent origin of urgent symptoms, and the rapid progress of the disease in the later stage, contrasted with the slow and gradual progress of Sheriff's case; and was probably, as in case II., in some measure due to the inflammation of, and effusion into, the right pleura. The conditions directly referable to dilatation of the heart, *e.g.*, dropsy, small pulse, &c., did not exist in the same extreme degree as in case V.; and, *probably*, as observed post-mortem, the dilatation had not attained the same disproportionate excess.[2] The symptoms of pleural effusion became prominent before dropsy, or other extreme effects of embarrassed cardiac action were very evident; and this state of the pleura, as seen in case II., no doubt aggravated the effects of the heart-disease;—prematurely inducing the fatal event.

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[1] By this statement, I do not commit myself to any opinion as to the connection between disease of the heart and the albuminuria, which so frequently accompanies its last stage. There is some difficulty in determining how far this state of the urine is connected with a disorganised state of the kidney, which I am disposed to believe usually exists.

[2] This statement is expressed thus cautiously, not so much from any doubt I have in making it, as from the known difficulty in comparing the relative disproportions of hypertrophy and dilatation in different cases. For we know that the embarrassment of the heart's action, arising from disproportionate dilatation, bears no stated relation to the bulk or weight of the organ; and that a smaller heart may be either less or more dilated in proportion than one of greater size.

In the account of such cases by other writers, the structural changes of the heart have been recorded with minuteness, and the anatomy of cardiac aneurisms has received greater attention than any other part of the subject. In these observations, therefore, which it is desirable to make short and practical, I shall confine myself to a short notice of the transformation of the muscular substance, the probable existence of endocardial ulceration, and the pericardial adhesion.

The chief interest connected with the transformed, fibrous state of the muscular substance, is involved in the investigation of its influence in causing the aneurismal state of the heart—true or sacculated. It has been well remarked by Dr Craigie, that it is difficult to say how far this transformed state of the muscular substance is a cause or an effect of the aneurisms. The facts of our cases throw an imperfect light on this question. They do not prove that the sacculated aneurisms were not preceded by a transformation of the tissues of the heart; but they show that transformation had taken place in that part of the heart where the true aneurism or partial dilatation existed, and that that transformation was antecedent to the formation of the true aneurism, because it existed in parts where dilatation had not as yet occurred; and in regard to the sacculated aneurisms, the saccules which existed in both cases indicate that they may occur without any so peculiar and complete transformation of the tissues as appeared to precede the true aneurisms. If these inferences from the facts of the cases are admitted, it may be stated, in general terms, that the occurrence of true aneurism is determined in a great measure by the transformed state of the tissues; and that the sacculated aneurisms, though they may be preceded by a similar transformation of the tissues, appear to have other causes, and may arise where no adequate transformation exists. These sacculated aneurisms, however, probably tend to induce absorption of the tissues in a greater degree than the true aneurisms do—causing the thin fibro-membranous sac walls. It has been already shown, that the effects of these aneurisms are very much in proportion to the degree in which they cause increased capacity of the heart; now, the small size of many of these sacculated aneurisms, even when very prominent, will probably render their duration so much longer than that of the true aneurisms, as to give time for more advanced changes in the tissues involved in their formation. The fibro-cartilaginous state of the parts in the vicinity of these sacs is a further evidence of this.

Ulceration has been supposed to determine, in some degree, the seat and development of sacculated aneurisms; but there is a want of facts substantiating this; and in neither of the preceding cases was there any good reason to believe that ulceration of the endocardium had existed. Several of the rudimentary saccules were still at a stage of their formation, in which the effects of ulceration would have been apparent if it had ever existed. Their surface, however, was

continuous with the endocardium; and in some of them the margins of the orifice were formed by muscular bundles, little altered, and having no appearance of ulceration. In the interior of the aneurismal sac, in the case Stewart (VI.), there was a patch which, from the sloughy state of its surface, and from its sharp, abrupt edges, with discoloration, almost justified the opinion that it was an ulcer, but this was doubtful. The loss of substance was not apparent; but this portion of the sac had an additional depression, as it were, of a commencing dilatation. The only inference which these facts admit of is, that the sacculated aneurism occurs without antecedent ulceration.

Pericardial adhesion existed in both these cases. It was very limited in the case Stewart (VI.); and it was seated at the posterior surface of the heart. In the case Sheriff (V.) it existed anteriorly, and was extensive.

I am disposed to think that undue importance has been attached to adhesion of the pericardium, from the supposition that it tends to prevent rupture of the sac and hemorrhage. It is said to have existed chiefly in cases in which the sacs formed prominent tumours; and that it is due to the pressure of the opposed pericardial surfaces by the tumour. There is an insufficiency in this explanation; and the cases before us not only cast doubt upon it, but appear to afford data for a more rational notion of the nature and relations of this morbid state.

Observe, in the first instance, that the adhesion was neither limited to the surface of the tumours, nor did it involve these in any remarkable degree. Secondly, The part of the pericardium which was most adherent, especially in case V., or had the roughened surface of a fibro-cellular formation, corresponded precisely with that part of the muscular substance which had undergone the fibrous transformation; and the opacity of the corresponding part of the endocardium, as well as the destruction of the reticulated fleshy bundles, showed that the altered action in which the transformation of the muscular tissue originated, had extended to and involved the whole thickness of the heart's walls, and resulted in the adhesion of the opposed pericardial surfaces.

It cannot be denied, that adhesion might possibly arise from pressure of the aneurismal tumour. In both these cases we see considerable tumours without this effect, and the adhesions of the sacs which did exist were contiguous to that which corresponded to the transformed tissues, and involved neither the most *prominent* nor the *thinnest* part of the sacs.

In the case Stewart (VI.) there was a marked disproportion between the extent of the transformation of the muscular substance and the limited adhesion. The surface of the sac was devoid of all adhesion; and the transformation of the tissue, though extensive, was not by any means complete, and had not attained nearly the degree

which existed in case V. This, perhaps, explains the as yet unaffected state of the pericardium; certainly, where it did adhere in the sulcus beneath the sac, the transformation of tissue was more complete than elsewhere.

CASE VII.—*Diffused or True Aneurism of the left Ventricle; Dilatation of the Heart; Hypertrophy; Fibrous Transformation of the Muscular Substance; Hydrothorax; Chronic Pneumonia; Death by Syncope.*

Mrs Richards, ætat. 66, a delicate-looking woman, was admitted on the 5th June 1847.

Dyspnœa was not urgent, but she was unable to rest continuously in the horizontal posture; and she had cough and palpitation. She died suddenly a few hours after admission, before any careful examination of the case had been made.

History.—This could not be ascertained, farther than that she had been liable to palpitation for a period of years.

Post-mortem Examination.—The volume of flesh was good. There was general dropsy, and the right pleura contained a gallon of serum. The lower lobe of the left lung was condensed in the second stage of pneumonia.

The heart was rounded in its external form, chiefly from a diffused bulging of the left wall; the cavities were filled with loose dark-coloured coagula. The weight of the organ was thirteen ounces, and it was dilated in all its cavities. The capacity of the left ventricle was more especially increased by the diffused aneurismal dilatation of the left wall, the muscular substance of which was almost completely transformed into fibrous tissue, presenting a mere trace of muscular fibre. This wall of the left ventricle was, in consequence, rigid and fibro-membranous to the extent of three inches; and the ventricle did not collapse when laid open. The dilated fibro-membranous wall was little more than one line in thickness.

Remarks.—Great variety has been observed in the age of individuals affected with cardiac aneurisms; and it is probable that the disease will be found to assume different forms at different periods of life. In this case the atrophy of the dilated part, and the removal of the muscular substance, was, perhaps, more remarkable than the transformation of the tissues, though it is possible that transformation may have preceded the atrophy of the left wall. Still the existing state of the heart contrasts remarkably with the fleshy fibrous transformation in the two preceding cases, both of whom were in comparatively early life.

The mode of death in this case, by syncope, is not unusual in cases where the heart is dilated; and there can be no doubt that the capacity of the left ventricle, and its embarrassed action, had much to do with this accident, though, as in case II., the state of the lungs and pleura was probably not without its influence.

The next case was not under my own observation, having occurred in the practice of Mr Benjamin Bell, who kindly favoured me with the details, from which the following account has been drawn up.

CASE VIII.—*True Aneurism of the Left Ventricle of the Heart; Dilatation; Hypertrophy; Fibrous Transformation of the Muscular Substance; Pulmonary Apoplexy; Death unexpected.*[1]

Mr ——, æt. 65, a professional gentleman, of full body, but without plethora, temperate, and of active habits, had a severe attack of acute pneumonia in the right lung in December 1844 (four and a-half years before death), and he was re-attacked by the same disease in the following summer, 1845. He was largely bled for both attacks. The lung appeared to recover its healthy action; but subsequently, more or less breathlessness on ascending even a slight acclivity existed; and, though his mind continued active as ever, he found himself unfit for the exercise he had been accustomed to. This may be regarded as the beginning of Mr ——'s ill health.

Subsequently, the urine occasionally became scanty; but this was readily counteracted by ordinary diuretic remedies. He was subject to uneasy feelings in the chest—sometimes of the right side, sometimes in the cardiac region—which, however, subsided after leeching, a small blister, or a belladonna plaster.

About four months previous to death, the breathlessness, and, as he himself called it, "the disability on locomotion," became so much increased, that he was unable to walk a few yards, even on level ground, without stopping to take breath. He referred the painful or disabling sensation to a point near the situation of the gall-bladder.

The chest had been repeatedly examined in the course of the preceding four years; but no sign indicating a morbid state of the heart was observed until latterly, when the impulse of the heart became forcible and extended, and an indistinct murmur with the first sound was occasionally audible at the seat of the apex, and below the left costal margin.

About six weeks before death, the lower extremities became œdematous; urine was at the same time scanty, and diuretics exerted little or no influence. For one or two days the urine was observed to be albuminous; but this afterwards disappeared. About ten days previous to death the anasarca suddenly subsided, without either purging or diuresis; and then hæmoptysis occurred, which never left him. At this time the dyspnœa was much aggravated. To the last he was conscious, and to a certain degree intelligent when roused; but he readily lapsed into a state of dull, wandering lethargy. Shortly before death he was in his usual state, extremely feeble and exhausted; but at the last his death was instantaneous, and unexpected by his family.

Post-mortem Examination, thirty hours after death.—The surface generally was slightly yellow. The volume of flesh was considerable; with more than the usual amount of fat under the integuments.

The heart was large and globular, weighed eighteen ounces, and was distended in all its cavities by loose dark-coloured clots. In the apex of the left ventricle, there was found a fibrinous coagulum, firm, grey, and adherent; it was contained in a partial dilatation of the ventricle, which caused a pouch-like addition to the cavity. The ventricle was immensely increased in its general capacity, partly by this sac, and partly by general or true dilatation. The widest diameter of the sac was at its orifice; its lining was continuous with the endocardium, and had one or two patches of yellow atheromatous deposit; the reticulated muscular bundles were mostly wanting in its interior. The wall of the sac was formed in part by the septum of the ventricles, and by the left wall, at the apex; it was attenuated and fibro-membranous, without a trace

[1] This expression requires explanation. It is intended to convey the idea of death anticipated as perhaps not very distant, but nevertheless occurring at an unlooked-for moment. This unexpected death is distinguished from sudden death, inasmuch as in the latter a fatal issue was less immediately anticipated; and it is equally distinct from death after gradual exhaustion.

of muscular tissue. A small but firm adhesion of the pericardium existed; surrounded by a fleshy cellular patch, on the surface of the tumour, where the wall of the sac was thinnest. The walls of the ventricle elsewhere were thickened; in the anterior part, their muscular substance had undergone a very imperfect transformation into fibrous tissue; and the fleshy columns of the ventricle were similarly, but more completely, transformed. The endocardium, especially towards the apex, was opaque and thickened. The valves were normal, except the aortic, which, as well as the aorta and coronary arteries, were altered by a calcareous and atheromatous deposit. The right ventricle was dilated; the reticulated muscles hypertrophied, and the walls slightly thickened. The auricles were large.

The lungs were partially emphysematous anteriorly. In all the lobes there were masses of condensation from hemorrhagic effusion,—some of considerable size.

The liver was small and pale. The kidneys normal.

Remarks.—The name and known accuracy of Mr Benjamin Bell, is a guarantee for the correctness of the narrative of this case; and the length of time that the patient was under observation, gives a value to the *history* which cannot be claimed for cases observed only during the later stages. I shall, therefore, endeavour to indicate the relation between the different forms of heart-disease which existed, and the morbid phenomena noticed at different stages.

The case illustrates, very strikingly, the influence of dilatation; for this was the only lesion of the heart competent to explain the symptoms; and the dilatation was of two kinds—it was originally partial or *aneurismal;* latterly, *true* or general dilatation also existed. It appears probable that the slight degree, and gradual progress, of the symptoms in the first years, was due to the *former;* while the extreme obstruction to the circulation, and the rapid progress of the symptoms during the last few months, were due to the *latter.* The state of the parts proves that the aneurism was of old standing; that it must of itself have increased the capacity of the ventricle, and must otherwise have embarrassed the action of the heart;—causing effects similar to those of true dilatation, but less in degree, just because the dilatation was less or partial. The fact that the lesion involved the effects of dilatation from so early a date, is, I believe, intimately connected with the steadily unfavourable progress of the case.

The proof that the true dilatation was of more recent origin, is less direct. A very decided change in, or aggravation of, the symptoms occurred within the last six months; and the only lesion accounting for this, was the great dilatation of the left ventricle. This increased capacity was not due to the aneurism of the apex alone, but also to true dilatation of the whole ventricle; and the aneurism presented no appearance of having undergone any change or advance such as would explain this rapid aggravation of the symptoms. While, therefore, the old standing aneurism was the apparent and sufficient cause of the pre-existing slighter symptoms, this true dilatation was probably connected with the development of the later symptoms of a more obstructed circulation; though the date of its origin cannot possibly be determined, nor its precise influence on the progress and

issue of the case distinguished from that of the aneurism. Nevertheless, we see secondary pathological effects and symptoms in the case which an uncomplicated partial dilatation scarcely explains, and which are identical with those we have seen to accompany *simple* preponderating dilatation (cases I. and II.). The dilatation of the ventricles, especially the left, was the leading feature of this case; and, as in other cases, it co-existed with an instantaneous and more or less unexpected death, a distended state of the heart by recent loose clots, and pulmonary hemorrhage.

The hypertrophy of the right ventricle was, perhaps, a cause of the slow progress of this case in the early stages. But this question will be considered in commenting upon other cases.

The wall of the aneurismal sac, as in case VII., was attenuated and membranous, but to a less extent. This fact is worthy of remark in connection with the age of the patients, but is quite insufficient to establish any necessary relation between this state and advanced life. Moreover, the muscular substance of the heart was elsewhere in progress of the same fibrous transformation, as in cases V. and VI.

The state of the pericardium throws some light on the probable cause and mode of the adhesions in these aneurisms.

The tumour caused by the aneurism, on the external surface of the heart, had no very manifest prominence distinct from the globular and dilated state of the organ generally; the adhesion of the pericardium existed at the apex, but no such protuberance of the tumour existed here as to justify the idea that increased pressure had existed; it is probable, therefore, that some other cause than pressure had to do with the formation of this adhesion. Besides, the band of adhesion was in itself small and insignificant; but it was surrounded, on the surface of the tumour, by a considerable cellular fleshy patch; and the sac wall was most attenuated at, and near to, this situation. It appears, then, first, that the thinness of the sac wall, just as is observed in other hollow viscera, had to do with the exudation on the pericardial surface; and, second, that the exudation may result merely in the formation of a layer or patch externally; while adhesion depends upon the accidental circumstances of contiguity, &c. It is manifest, that the formation of an organised layer externally, by thickening the attenuated walls, will have the effect of delaying rupture of the viscus; and this hindrance will be promoted by the adhesion of the opposed surface, should that *accidentally* occur.

The entire failure of the diuretics, in the late stages, is an almost invariable occurrence, and was well seen in the progress of case V. It is probably intimately connected with, or an effect of, dilatation.

The sudden disappearance of dropsy shortly before death is not unfrequently observed. The practical lesson which the circumstance teaches is, that other secondary morbid conditions, such as the he-

morrhagic condensation of the lung in the present case, having been established, the removal of dropsy, viewed singly, does not materially modify the prognosis.

ARTICLE IV.—*Notes on Scarlatina.* By PATRICK NEWBIGGING, M.D., F.R.S.E.,

(*Read before the Medico-Chirurgical Society of Edinburgh, 20th June* 1849.)

FOR several months past scarlet fever has prevailed as an epidemic throughout Scotland. It has not been confined to any particular class of the community; but has affected all ranks with great severity; and, although it has chiefly attacked young persons, adults have not been exempt.

Having had the opportunity of witnessing many cases of this disease, and that, too, in an excellent field for observation, viz., whilst it prevailed among the inmates of John Watson's Institution, I have considered that it might not be uninteresting to arrange some notes, however brief, of a few of the phenomena of scarlatina as they exhibited themselves in the cases at that seminary.

The present notice may be additionally instructive, when viewed in connection with the previous report of the epidemic in 1835, at the time the medical superintendence of the Institution was under the judicious direction of my respected predecessor, Dr Johnson.

To my professional brethren unacquainted with Edinburgh, I may state, that John Watson's Institution is a large building in the immediate vicinity, occupying an elevated and airy situation, and presenting great facilities for drainage, which, I have reason to believe, have been made fully available. It is dedicated to the education of children of both sexes. The number at present is 120, which, including the other inmates, make in all a household of 136. The internal arrangements of the house, as well as the domestic management, are excellent, and there is every convenience for the accommodation and treatment of the sick; a large room at the further extremity of the dormitory gallery—a bath room, with nurses' accommodation—and other conveniences, being set apart for this purpose.

The first case of scarlet fever which occurred in the Institution, was that of a girl on the 6th September. She was immediately removed to the sick room, and all communication, in so far as was possible in such an establishment, was interdicted. Cases, however, continued to present themselves upon the 7th, 9th, 17th, 19th, 20th, 21st, 23d, and 28th September, 1st, 3d, 4th, 7th, 8th, 12th, 15th, 16th, 17th, and 20th October. No new case occurred after this date. The number in all was twenty-five. I have to record one fatal case —that of a delicate boy. In consequence of an enlarged condition of the mesenteric glands, producing slight ascites, he had been formerly sent to his friends in the country to recruit. For some time, however, he had been in good health, and it was thought advisable

that he should return to the Institution, and although still a delicate child, he continued apparently well, as far as regarded his former ailments. Upon the 28th September he was attacked with symptoms of scarlet fever, and died during the night of the 1st October. The case from the commencement presented a most unfavourable appearance. There was great restlessness, an anxious expression of the countenance, and injected conjunctiva; the pulse was extremely rapid, but soft and readily compressed; the skin hot and dry, and of a dusky-red appearance, with irregular patches of a purplish hue; the tongue was dry, with a thick brown fur; and the membranes of the mouth and fauces dark-coloured, and somewhat swollen; the external glands of the throat and neck were enlarged, and pressure upon them caused uneasiness. He had much nausea, and the bowels were disposed to be loose; the urine was of normal specific gravity, slightly acid, and, on exposure to heat, exhibited an albuminous haze. On the following day, or about sixteen hours from my first visit, the throat was much affected, having a white aphthous crust upon it, emitting a fœtid odour, and presenting all the appearances of the Cynanche Maligna; deglutition was greatly impeded; the pulse was extremely rapid, small, and compressible; the skin presented the same appearances, only in an aggravated degree, as upon my previous visit; there was great prostration of strength. A strong solution of nitrate of silver was applied to the throat, carbonate of ammonia was administered, and he had a tea-spoonful of wine occasionally. As the extremities were becoming cold, external warmth was assiduously maintained. No alleviation, however, of these formidable symptoms took place. The boy passed gradually into coma, and died early on the morning of the fourth day.

In the other cases the prominent symptoms were bright efflorescence, with great heat of skin, painful and aphthous condition of the throat, rapid pulse, and peculiar unmistakeable appearance of the tongue.

A few of the cases had rigors, nausea, and violent headache at the commencement; but, in the greater number, these symptoms were not excessive, and after the first forty-eight hours, I felt but little anxiety as to the result. The treatment I adopted, consisted, in the first instance, in the liberal employment of antimony, preceded by an aperient, and this was again repeated, and generally with marked benefit, towards the disappearance of the eruption. Tepid sponging was much employed, and proved most agreeable to the feelings of the patients, by promoting, I have reason to believe, the action of the skin. I had not recourse to the warm bath so much, and as I myself had an opportunity of witnessing, so justly lauded, in a former prevalence of the epidemic, by Mr William Wood and Mr Sidey, as the symptoms did not, in my opinion, appear to require it. Neither did I employ blood-letting, so highly recommended, in former attacks of scarlet fever, by Drs Dewar, Craigie, and others, not considering

such a proceeding necessary in any of the cases which, during this epidemic, have come under my consideration, either in my private practice, or amongst the children at Watson's Institution. The diet was carefully regulated, and nothing containing animal food was allowed during a period of at least ten or fifteen days.

Great advantage was derived from the early and frequent application of lunar caustic in solution, or in the solid form, to the throat, and I did not limit its use simply to the aphthous state, but employed it in almost all my cases, and feel satisfied that it was a valuable auxiliary to the general treatment.

Water, acidulated with nitric acid, was found very agreeable, and, I have no doubt, was of considerable benefit by acting as a convenient gargle, especially in the case of very young children, where there is great difficulty in making any local application. Notwithstanding the confinement of the children in John Watson's Institution to the sick-room, under the strict superintendence of a nurse to guard against every exposure to cold, errors in diet, &c., five cases of anasarca occurred, and it was observed (and the same remark has been made by others) that this took place in the instance of children who had passed through a milder form of the disease, in which the eruption and other symptoms had been by no means prominent; thus showing that those affected with the simpler form of the complaint are even more liable than the more severe to be followed by attacks of this nature. Such cases generally occurred from fourteen to twenty days after the attack of fever, or on the 10th or 12th day from the stage of convalescence. In three of these I deemed it advisable to apply leeches over the region of the kidney, and in all to resume the use of antimony in combination with colchicum. Strict confinement to bed, a low diet, and the administration of an active purgative, were found to be effective remedies. Eventually these dropsical cases made perfect recoveries.

During the entire progress of this epidemic, I almost daily examined the condition of the urine in the acute eruptive stage, as well as after convalescence was declared, being under the impression that the state of that secretion and anasarca were not always necessarily connected, and I thought this a fitting opportunity for making some observations on this point. I found that the specific gravity of the urine did not vary in the different cases, not even in those which were dropsical, but averaged 1·017 to 1·025. On exposure to heat there was almost invariably a slight opalescent appearance. In the five dropsical cases, the urine was secreted in smaller quantity, and was more albuminous, but its specific gravity in no case fell below 1·017,—a depression not sufficient to indicate any serious organic change.

In this state a gradual improvement took place, under the treatment indicated above, and great benefit was derived after the antimonial course was concluded, from the use of the nitrate and carbonate of potass in the form of a diuretic.

The alleged connection between albuminous urine and degeneration of the kidney in this disease, is a subject which has excited considerable interest, and this association of cause and effect received an apparently strong corroboration from the result of Dr Hamilton's observations on scarlet fever, as it occurred in his practice in and around Falkirk.[1]

In the inference drawn from these observations as to the cause of dropsy, I for some time concurred. My original opinion, however, became modified by certain observations made in my practice generally, and my faith in its accuracy has undergone a still more material abatement in consequence of the investigations I have had an opportunity of prosecuting at Watson's Institution.

These have led me to consider the anasarcous condition, which arises during the progress of the convalescence from scarlet fever, to be, in very many cases, dependent on mere functional derangements, amenable to treatment, and leaving the health uninjured as regards organic alteration.

Confirmatory of this view, I may mention a fact, to which I have already slightly alluded, that in an early stage of the disease, even at the time when the eruption was just declining, having instituted an examination of the urine, I ascertained that, in every case, the albuminous haze was distinctly present, and remarkably so during the progress of the desquamation. That this appearance occurred without alteration, either in the specific gravity, except to a very small extent, or in the quantity, of the secretion; and that it continued from a period varying from four or five to ten days, gradually becoming less defined, and finally disappearing altogether.

This occurred in cases in which the urine maintained its normal standard as to quantity, where the patients were well, and certainly unaffected by any dropsical symptoms.

I have been encouraged to lay these observations before my professional friends, in consequence of the corroboration they seem to have received by the careful investigations made by Dr James Begbie, and published in the "Monthly Journal" for January. His remark corresponds with my own experience, that "if careful examination of the urine be instituted, albumen in small amount will be found to exist in every case of scarlet fever."

It is worthy of remark that the quantity of urine voided at the period of desquamation is even greater than in health, although it may contain albumen, and this is interesting, as, in dropsy occurring, it may be subsequently in the same case, the amount is greatly diminished, sometimes altogether suppressed, so that any diminution of this secretion, without apparent increase of albumen, may be almost certainly considered as the forerunner of anasarca.

These observations, although corresponding with those of Martin

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[1] Edinburgh Medical and Surgical Journal.

Solon, who ascertained the presence of albumen in *twenty-two* out of *twenty-three* cases, are, I am aware, irreconcileable with those of some other authors, more especially with Philippe, who states that, although the succeeding dropsy was very common in the mild epidemic which he witnessed, yet, in *sixty* cases of the disease, in which he tested the urine for albumen, employing for this purpose both heat and nitric acid, no trace of that principle was discovered.

Dr Scott Alison remarks, in his "Clinical Observations,"[1] that he has observed dropsy to ensue more frequently after a severe attack of scarlatina, than when the symptoms were of a more moderate character. Also, that the urine in some cases, according to his analysis, was of specific gravity 1·005, while in others it was as high as 1·030; and he is of opinion that the kidney becomes organically affected at an early period of the disease. From these deductions, it is almost unnecessary to remark, that my own investigations, as well as those of others, differ essentially. I am greatly disposed to believe that, in many instances, the albumen is eliminated at the kidney—not owing to any disorganisation of that viscus, but that the blood, in all eruptive diseases, undergoes certain changes, and that in scarlatina this is exhibited by the presence of albumen in the urine; and that, in cases where the kidney is organically affected, as, doubtless, it is occasionally, that this arises from a specific inflammation of that organ.

I am sensible that these observations are partial and incomplete, and had I again an opportunity of investigating a series of cases similarly circumstanced, I would avail myself of more minute analysis; at the same time I feel disposed to draw the following conclusions from what I have observed:—

1st. That in very many cases albumen appears in the urine at an early period in scarlatina, without other alteration of this secretion, or affection of the system generally.

2nd. The diminution in the quantity of this fluid may be considered as a certain forerunner of anasarca.

3rd. That the treatment should be strictly antiphlogistic, the diet being for some time carefully regulated, so as to exclude much azotised food.

4th. That the patient, however mild his symptoms, should be confined to bed for several days after the eruption has declined.

Scarlet fever having prevailed in this institution to so considerable an extent, and the cases having occurred in close succession, notwithstanding a system of separation, as complete as was possible, amongst inmates residing under the same roof, I felt desirous to try the effect of belladonna as a prophylactic against this disease. It was

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[1] London Journal of Medicine, No. III., March 1849.

an opportunity such as rarely occurs for the investigation of the alleged virtue of this drug on a large scale.

Having ascertained the number of children unaffected with scarlet fever, or who were uncertain as to ever having had it—making, in all, sixty-nine—I directed that belladonna should be administered to them, in the proportion of one-sixth to one-fourth of a grain twice a-day, according to the age of each child—the first dose being given before breakfast, and the last dose at bed-time. This plan was adopted on the 16th October. Three new cases occurred between that and the 20th. After that date no child was affected, nor has there been any instance of scarlet fever since that period in the institution. The use of this preventive was continued during upwards of five weeks.

It may, perhaps, be asked why the belladonna was not administered at an earlier stage of the epidemic—indeed, on the first occurrence of a case of scarlatina? My answer to this apposite question, I confess, is not perfectly satisfactory, viz., want of faith, and hence confidence, in the prophylactic properties of belladonna, and this though aware of the report made at the Orphan Hospital of Langendorf, in Prussia, in a family of 160 individuals, where belladonna having been administered immediately on the occurrence of the epidemic, two only took the disease.

I should now consider it my duty to lose no time in making use of this medicine on the first appearance of this disease, and I would strongly recommend the same plan of practice to those of the profession who are connected with similar educational institutions, with the view, not merely of attempting to ward off a malady so uncertain in its progress, and occasionally so fatal in its termination, but also with the object of accumulating information on a point of such paramount importance to the public health.

The opinion I have adopted on this point has been greatly strengthened by a similarly beneficial result, produced some time afterwards in another case. I was requested to visit a young gentleman at a large educational seminary. I found him labouring under scarlet fever, with profuse eruption, an aphthous, and very painful condition of the throat, accompanied by all the usual symptoms exhibited in the acute stage of a smart attack of this disease. I caused my patient to be removed, a few hours after first seeing him, to the house of a relative, and placed his brother, who continued to reside in the seminary, upon belladonna. This treatment was adopted in the other members of the family, consisting of nineteen, who had not previously been affected with scarlet fever. No other case occurred.

I am well aware, and can fully appreciate, how much, at times, the practical physician may be disappointed, under apparently similar circumstances, as to the efficacy of an acknowledged, and it may be favourite, remedy; and doubtless it may fail, and we may be unable to explain the reason of its want of success; but these two instances of the apparent efficacy of belladonna, administered under

circumstances in which a continuance of the disease might have been expected, weigh greatly with me in advocating a fair trial, at least, of this remedy.

29, Heriot Row, Edinburgh.

ARTICLE V.—*Contributions to the Pathology of the Heart and Arteries.— On Dissecting Aneurisms.* By THOMAS BEVILL PEACOCK, M.D., Physician to the Royal Free Hospital, London.

IN the year 1843, I published in the "Edinburgh Medical and Surgical Journal," a paper on the dissecting aneurism, in which I detailed the particulars of two cases not previously published, and described the preparations of three others, two of which I had found in the Museum of the College of Surgeons of Edinburgh, and the third in the Hunterian Collection of the College of Surgeons of England. I also entered into an analysis of the whole of the nineteen cases of which I was then able to obtain information, so as to illustrate the general characters of this form of aneurismal affection. Since this period, several interesting examples of this disease have been published; but, with little exception, they have only tended to confirm and extend the views I then expressed.

The so-called "dissecting aneurism"[1] is a form of primitive false aneurism, which originates in a laceration, or perhaps occasionally in an erosion, of the inner and part of the middle coat of the aorta, or one of the larger arteries, while the external and another portion of the middle tunic remain entire; the blood, therefore, which escapes from the artery is forced between the layers of the middle coat, separating them for a greater or less extent, so as to produce a sac following the course of the vessel. The cases of this affection which are on record may be arranged into three classes, according to the period which elapsed between the occurrence of the rupture of the internal coats, and the death of the patient.

1. In the first or incipient stage there is found a more or less extensive laceration of the internal, and of a part of the middle coat, with a slight extravasation of blood, or ecchymosis, between the laminæ of the middle tunic immediately adjacent to it. The cases in which the disease has been found in this very early stage are few in number. Instances are, however, related by Morgagni,[2] Nicholl,[3] and Hodgson.[4]

2. In the second stage, in addition to the laceration of the internal tunics of the artery, the blood is found to have penetrated be-

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[1] This term is stated by Rokitansky to have been introduced by English writers; but, so far as I am aware, it was first employed by Laennec.

[2] De Sedibus et Causis Morborum, xxvii. § 29.

[3] Phil. Trans., 1762.

[4] Diseases of the Arteries and Veins, p. 63.

tween the layers of the middle coat to a varied extent, so as to produce a newly-formed canal, occupying more or less of the circumference of the vessel, and which, at its distal extremity, not unfrequently re-opens into the original vessel, or one of its branches. In this category by far the largest proportion of cases of dissecting aneurism may be arranged. Such are those related by Laennec,[1] Lobstein,[2] Burns,[3] Smith,[4] M'Lauchlan,[5] Guthrie,[6] Elliotson,[7] Goddard,[8] Tessier,[9] Nivet,[10] &c., together with the two first cases described by myself in the paper before referred to. More recently similar instances have been reported by Drs Todd,[11] Thompson,[12] Kirkpatrick,[13] Lees,[14] Macdonald;[15] by Messrs Pirrie[16] and Farr;[17] and by Rokitansky;[18] and, indeed, of thirty-eight well characterised examples of this affection, the particulars of which I have collated from various sources, thirty are of this description.

3. In the third or complete form of the disease there is not only a separation of the laminæ of the middle coat of the artery, extending along its course for a considerable distance from the seat of the internal ruptures, and at its distal extremity occasionally re-opening into the original vessel; but the aperture by which the blood escapes from the original trunk is found smooth and regular, and the newly-formed canal is lined by a membrane bearing a very close resemblance to the ordinary internal coat of an artery. In these cases, also, the arteries distributed to the adjacent parts, some communicate with the original vessel, others with the newly-formed canal; so that, did we not possess the means of tracing the disease through all its various stages, there might be considerable difficulty in deciding whether such a condition were to be regarded as an original malformation of the ves-

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[1] Traité de l'Auscultation Mediate. Second edition, 1826. Tome second, p. 696.

[2] Rapports sur les Travaux Executés à l'Amphithéatre d'Anatomie de Strasbourg, 1805.

[3] Diseases of the Heart, &c., 1809, p. 231.

[4] Dublin Journal of Medical Science, vol. ix. 1826, p. 426.

[5] Glasgow Medical Journal, N.S. vol. i. Plates of Museum of Army Medical Department at Chatham, 4th fasc.

[6] Lectures on Diseases and Injuries of Arteries, 1830, p. 43.

[7] Lumleyan Lectures on Diseases of the Heart, p. 34.

[8] American Journal of Medical Science, quoted in London Medical Gazette, vol. xxiii. p. 668.

[9] Gazette Médicale de Paris, vol. x., 1842, p. 524, quoted from the Giornale delle Scienze Mediche.

[10] Nivet, Bulletins de la Societé Anatomique, 1836, p. 295.

[11] Med. Chir. Trans., 1844, vol. xliv. p. 301.

[12] London Medical Gazette, 1843, vol. xxxvii. p. 584.

[13] Dublin Journal of Medical Science, 1843, vol. xxiv. p. 283.

[14] Ibid, 1844, vol. xxv. p. 170.

[15] Ibid, 1846, vol. xxvi. p. 454.

[16] London and Edinburgh Monthly Journal, N.S., vol. 2, p. 363.

[17] London Medical Gazette, 1847, vol. xl. p. 1116.

[18] Rokitansky—Handbuch der Pathologischen Anatomie.

sel or as the result of disease. The dissecting aneurism very rarely attains this stage, though instances have been related by Shekelton,[1] Pennock,[2] and Henderson;[3] and very recently a case has been described by Bouillaud,[4] as an anomalous form of malformation of the aorta, which I am inclined to regard as having been a dissecting aneurism of this description.

The primary rupture of the internal tunics, in cases of dissecting aneurism, is most generally situated in the immediate neighbourhood of the origin of the aorta, or in some part of its ascending portion. Of thirty-five cases which I have analysed in reference to this point, in twenty-four the internal coats had given way in this situation; in two the ruptures occurred near the origin of the arteria innominata; in five at the transverse arch, or commencement of the descending aorta; in two in the course of the descending aorta; in one at the lower part of the abdominal aorta; and in one at the division of that vessel into the primitive iliac arteries. Of the twenty-four cases in which the coats had given way near the origin of the aorta, in fifteen the separation of the coats constituting the aneurismal sac, was limited to portions of the ascending aorta; in seven it extended a greater or less distance along the abdominal aorta; in one it reached to the common iliac arteries; and in another to the popliteal arteries. In one of the two cases in which the primary lacerations were situated near the origin of the arteria innominata, the separation of the coats extended from the commencement of the aorta to the point of departure of the left subclavian artery; in the other from the same point to the abdominal aorta. Of the five cases in which the ruptures occurred at the transverse arch, or near the origin of the left subclavian artery, the separation extended upwards to near the heart, and below along the descending aorta, in three cases; and was limited to the descending and abdominal aorta in the other two. In one of the two cases in which the coats were torn in the course of the abdominal aorta, the separation extended from the origin of the left subclavian artery above, into the primitive iliacs below; and in the other, the sac occupied portions of the descending and abdominal aorta. In a case in which the rupture occurred in the lower part of the abdominal aorta, the sac extended to the common iliac arteries on each side; and in the last case, in which the coats had given way at the bifurcation of the aorta, the separation was continued into the left external iliac artery.

Writers on this form of aneurism have very generally described the sac as situated between the external and middle coats of the

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[1] Dublin Hospital Reports, vol. iii. p. 201.

[2] American Journal of Medical Sciences, quoted in London Medical Gazette, vol. xxiii. p. 663.

[3] London and Edinburgh Monthly Journal, 1843, vol. iii. p. 613.

[4] Archives Générales de Médecine, 4me serie, tome xv. p. 248.

artery. This opinion is, however, most probably incorrect. Drs Pennock and Goddard, in the cases which fell under their notice, found that the primary ruptures had involved the internal and a part of the middle tunics, and that the sac was formed by the separation of the laminæ of the latter. In three cases which I had examined in the recent state, before the publication of my former paper, the sac was found to be situated between the layers of the middle tunic. I also ascertained, by experiments[1] on the aorta of the human subject, that the layers of the middle coat, when the internal and a part of the middle tunics are lacerated, admit of extensive separation by fluid forcibly injected into the vessel; while, when the whole of the middle coat is divided, and fluid is thrown into the interspace between the middle and external tunics, it readily escapes by transudation through the latter. I was, therefore, led to conclude, that the sac of the dissecting aneurism was, most probably, always situated between the laminæ of the middle tunic—an inference which has since been confirmed by various published cases. I may further remark, that, having recently had the opportunity of examining two specimens of the disease, previously described as having the sac situated between the external and middle coats, I found that its true situation was between the laminæ of the latter; a distinct though thin layer of the middle tunic being in each instance attached to the separated external coat, so as to assist in forming the outer boundary of the sac.

The result of cases of dissecting aneurism depends materially on the seat of the internal ruptures. When these are situated in the immediate vicinity of the heart, death generally rapidly ensues from the rupture of the external wall of the sac, and the consequent escape of blood into the cavity of the pericardium. Of twenty-four cases of this description, all but two so terminated almost immediately; while of the two exceptional cases, one, related by Mr Burns, proved rapidly fatal, from the rupture of the right auricle, occasioned by the over-distension of that cavity, resulting from the suddenly induced obstruction to the flow of blood in the aorta. In the other case life was prolonged for a period, probably of seven or eight years; and the patient ultimately sank from the secondary disease of the heart, superinduced by the aortic disease. In two other instances not here enumerated, in which dissecting aneurisms existed in the first or incipient stage, death also occurred from rupture of the cavities of the heart; in one of these, that of George the Second, recorded by Dr Nicholl, the right ventricle having been the seat of rupture; while in the other, related by Mr Hodgson, the left ventricle gave way at its apex. Where the rupture of the internal tunics occurs at a greater distance from the

[1] Of which an account is published in the Edinburgh and London Monthly Journal, 1843.

origin of the aorta—as near the arteria innominata, at the transverse arch, or in the vicinity of the left subclavian artery—the result is generally very similar to that which obtains when the primary lacerations are situated nearer to the heart. Of six cases of this description, one proved fatal by the rupture of the whole of the coats of the arteria innominata; in one the sac burst into the pulmonary artery; in one into the left pleural cavity; and in two into the pericardium. In the sixth case life was prolonged, probably for nearly nine months, and was ultimately arrested by the secondary disease of the heart, resulting from the aortic obstruction. In this case an entirely new canal had been formed in the coats of the aorta, and extended from near the origin of the left subclavian to the left common iliac artery, with which vessel it again communicated. In the other instance, in which the patient survived some years, the sac commenced near the origin of the aorta, and terminated in a *cul de sac* at the common iliac arteries. In both cases the sacs were lined by a newly-formed membrane, bearing a very close resemblance to the ordinary serous lining of an artery. When situated in the descending and abdominal aorta, the dissecting aneurisms are generally of limited extent, and do not differ in their result from the more ordinary forms of aneurism.

The occurrence of the ruptures of the internal coats of the artery forming the first step in the development of a dissecting aneurism, must be ascribed to a diseased condition of the tunics, by which they are rendered easily lacerable. In a large proportion of the published cases of this form of disease, the aorta is stated to have been dilated, and in some its calibre was very greatly increased, while, at the same time, its coats were most generally atheromatous, and irregularly thickened and thinned in places. The existence of these conditions is not, however, necessary to the production of the ruptures. In some instances the tunics of the artery are stated to have been entirely healthy, or only slightly diseased; and in others, though there existed much atheromatous deposit in other parts of the vessel, the immediate seat of the rupture was entirely free from obvious morbid change. The lacerations, in such cases, must, therefore, have resulted either from the excited action of the heart dependent on some undue exertion, or have depended upon an acute change in the arterial tunics, by which they were rendered incapable of resisting the ordinary amount of pressure. Whatever be, however, the state of the internal coats, there is no doubt that a morbid condition of the heart very powerfully predisposes to their rupture; and it will be found that hypertrophy, with or without dilatation of the left ventricle, and the evidences of old inflammation of the pericardium and endocardium, are almost constant concomitants of the disease. From the greater frequency of dissecting aneurisms in persons at advanced periods of life, than in middle age, when the circumscribed forms of aneurism most frequently occur, it would also appear that

the frangibility and looseness of the arterial tunics in elderly persons predispose to the disease. In females, also, the dissecting aneurism is somewhat more frequent than in males, the reverse of which obtains with the circumscribed aneurismal tumours; and this is probably due to the greater delicacy of the coats of the arteries in the sex.[1]

Rokitansky, in a recent article on this subject, has referred the production of the ruptures and separation to acute inflammation involving either the external coat alone, or the whole thickness of the arterial tunics. In the former case the external is separated from the middle coat, and the rupture of the middle and internal tunics ensues secondarily. In the other the internal rupture is the primary change. The former mode of origin does not appear to have obtained in any of the cases which have fallen under my own notice. From the experiments before referred to, it also follows that, if such a process act as an occasional cause of the disease, the inflammation, instead of being limited to the external, must involve also a portion of the middle coat; and it seems scarcely probable that the tunics so diseased should be able to resist the pressure of the extravasated blood sufficiently long to allow of the formation of a dissecting aneurism. After much consideration of the subject, and a careful examination of eight or ten recent specimens, I am still inclined to ascribe the production of the dissecting aneurisms to disease, either of an acute or chronic nature, chiefly limited to the internal and a portion of the middle tunics of the artery. To this I refer the rupture of the internal, and a part of the middle coat, and the ready separation of the laminæ of the latter, while the external and outer layer of the middle coat being little, if at all, diseased, and, therefore, possessing sufficient power to resist the pressure of the column of blood flowing from the artery, the outer boundary of the sac. In the circumscribed aneurismal tumours, the intimate adhesion of all the tunics together, and the extension of disease throughout the whole thickness of the coats, form the earliest and most constant morbid changes which can be detected; and it is the presence or absence of this intimate adhesion which seems to determine whether, on the occurrence of the internal ruptures, the aneurism shall assume the dissecting or the circumscribed form. In

[1] The following table presents the statistics of this form of aneurismal disease as respects age and sex:—

20 to 30 years of age,	2 cases.
30 to 40 ...	1 ...
40 to 50 ...	5 ...
50 to 60 ...	10 ...
60 to 70 ...	3 ...
70 to 80 ...	2 ...
84 & 88 ...	2 ...

Mean age, 56.

Males,	12 cases.
Females,	13 ...

some instances, however, a peculiar separability of the tunics has been noticed in the recent state; and while I believe that, provided there be no adhesion of the tunics, the occurrence of the internal rupture will necessarily be followed by some amount of dissection of the laminæ of the middle coat, I do not doubt that this is generally much facilitated, either by an original or acquired looseness of the texture of that tunic, or by its being the seat of acute inflammatory action.

On the occurrence of the rupture of the internal tunics of the artery, which leads to the formation of a dissecting aneurism, the patient is usually suddenly seized with pain in the region of the heart, and extending thence to the shoulder, and down the course of the spine; the breathing becomes difficult, and there is a sense of constriction across the chest; the pulse at the wrist is feeble, faltering, and irregular or imperceptible; the skin is cool, and bathed in perspiration, and there is a feeling of impending dissolution. These symptoms may either follow some undue bodily exertion or mental excitement, or they may supervene without any obvious exciting cause having been in operation; and, in some cases, the patient has been so seized on awaking during the night. In the great majority of cases, as the external wall of the aneurismal sac gives way very shortly after the primary rupture has occurred, these symptoms are soon followed by death. This occurred in thirteen out of twenty-four cases in which the aneurism was situated near the heart; and in four out of seven cases, when the sac commenced at the transverse arch. Should, however, the sac possess sufficient power to resist the pressure of the extravasated blood, either from the thickness of its outer wall, or from the blood being poured out with little force, the symptoms may subside more or less completely, till a recurrence of the syncope, indicating the external rupture, is rapidly followed by death. This interval, in the comparatively small number of cases in which it occurs, is usually of very short duration;—in five cases it extended to various periods not exceeding twenty-four hours; in one case to two days; in two cases to three days; in two cases to four days; in one to ten days; and, as before stated, in one to nine months, and in another to eight or nine years. When the interval is of short duration, the circulation is generally very imperfectly restored, and the symptoms never entirely subside; the pulse continues feeble or irregular, there is still more or less dyspnœa, and a sense of constriction across the chest, and pain in the præcordia or spine, are complained of. The pain appears not unfrequently to be of a peculiar rending or tearing character, and may probably be caused by the forcible separation of the coats of the aorta by the column of blood escaping through the internal ruptures.

The cerebral functions are generally disturbed, and there is insensibility or delirium. In the case described by Dr Todd, hemiplegia occurred, and was ascribed to the defective vascular supply

of the brain, consequent on the compression of the right carotid artery by the blood extravasated in its coats, occasioning softening of the cerebral substance. Suppression of urine is also a frequent symptom in these cases, and there is usually severe pain in the loins.

These symptoms, it will be seen, do not possess any features sufficient to characterise this disease from some other forms of suddenly induced lesion of the heart and aorta; and as the cases in which life has been sufficiently prolonged to allow the physical signs to be carefully observed, have been very few in number, we cannot at present, with any certainty, lay down the indications by which the disease might be detected during life. When dissecting aneurisms have been found in connection with the abdominal aorta, the symptoms induced have been so slight or indecisive, that the disease has in no instance been suspected during life.

The following case, which fell under my notice during the course of the last year, will illustrate several points referred to in the previous brief summary of the general features of the dissecting aneurisms:—

Aortic Valvular Disease; Early Dissecting Aneurism; with Rupture of the Aorta into the Sac of the Pericardium.

Thomas Campion, aged 44, by trade a painter and upholsterer, was admitted as an out-patient of the City of London Hospital for Diseases of the Chest, on the 30th of June 1848. He had been of intemperate habits at an earlier period of his life, but had never suffered from any form of rheumatic affection. About twelve years before he had received a blow on the left side of the chest, and had since that time been subject to occasional palpitation of the heart. For a year and a-half before the time of his application at the hospital, he had suffered more severely from palpitation, and he had been seriously ill since the previous December. He was at that time much involved in pecuniary difficulties, and while out one morning on important business, he was suddenly seized with extreme breathlessness, palpitation, pain in the region of the heart, and syncope, and could with difficulty be got home. From this period all his symptoms became aggravated; he was never able to lie down in bed, and was subject to frequent paroxysms of urgent dyspnœa. The attacks usually occurred once a week, at about one o'clock in the morning; and, after continuing for half an hour or more, gradually subsided. He was, however, at all times subject to difficulty of breathing and palpitation, and these symptoms were aggravated by any exertion, and especially by ascending stairs. Latterly he had been subject to a hard cough, usually with scanty expectoration of pale-coloured mucus, but which had been occasionally sanguineous. On examination at the time of his admission at the hospital, the cardiac dull space was found to commence above at the level of the nipple, and to extend much beyond its natural limits. A loud double murmur was heard over a large portion of the front of the chest, and was roughest in character, and most intense, between the sternum and left nipple. The pulse was synchronous at the wrists; it was small and feeble, and not of the peculiar regurgitant character. He was at this time seen by Drs Jeaffreson and Bentley, and subsequently went into the country. While there he was suddenly summoned to town, and, while on his way from the village at which he was residing to the railway station, he became much excited, fearing he should be too late for the train. This proved to be the case, and he was compelled to wait for another; soon after starting by which he experienced a peculiar sensation, which commenced in the lower part of the abdomen, gradually rose to the præcordia, and then be-

came an agonising pain in that situation. When he reached home he was much collapsed, his extremities were cool, the urine was entirely suppressed, and he still suffered from the pain in the region of the heart. I saw him early in the evening, he was then perfectly intelligent, and the collapse had in some degree subsided. He could only obtain ease by kneeling upon the sofa resting on his arms, and with his head hanging over the end: his face was pale and anxious, his hands and feet cold, and he complained of agonising pain in the region of the heart, extending thence to the shoulder, and down the spine from the occiput. His pulse was regular and equal in the two wrists, and somewhat vibratory, though not distinctly regurgitant in its character. The suppression of urine continued. On percussion, there was found decided dulness over the whole of the sternum, along its right side, and over a large extent of the left side of the chest, beneath the level of the nipple. A loud double murmur was extensively heard over the front and left side of the chest, and was most distinct at the upper third of the sternum, and on each side of that bone. The murmur which accompanied the first sound of the heart, though still audible beneath the left nipple, was less marked there than at the upper part of the sternum—but the murmur with the second sound, though of a softer note, was most distinct towards the left side of the lower part of the sternum. He had been directed to apply sinapisms, and had taken diffusible stimulants, before I saw him. These he was directed to continue, and to have the space between the shoulders and the region of the heart rubbed with an embrocation consisting of the compound camphor liniment, and the strong tincture of aconite. From these means he experienced some relief, but about two o'clock on the following morning, after having been more quiet, he suddenly raised himself, clasped his arms round the neck of his wife, and expired.

On examination, the lungs were found of large size, sparingly crepitant, and much engorged. The pericardium was enormously expanded, and when opened, was found to contain a very large quantity of pale serum, together with a thick and solid coagulum, which enveloped the heart, especially in its right side. On exposing the heart, a transverse slit, about an inch in length, was seen on the pericardiac covering of the commencement of the aorta, immediately above the base of the right ventricle; and it was evident that the blood effused into the sac of the pericardium had escaped by this passage. The heart was of very large size, weighing not less than 20 oz. The attached pericardium displayed some nodular concretions of old lymph on the anterior surface of the left ventricle, and was opaque and thick on the right. The right ventricle was dilated, but the walls retained their natural thickness. The cavity of the left ventricle was also very much enlarged, and its walls hypertrophied, while the columnæ carneæ not being proportionately increased in size, appeared relatively small. The auricles were natural. The folds of the mitral valve were somewhat rigid and opaque. The aortic valves were much thickened at their free edges, and the right and left crescents were adherent at their angles of attachment, so as to leave an aperture by which fluid slowly regurgitated from the aorta into the left ventricle.

The ascending aorta was much dilated, and its coats were thin and in places atheromatous. On the right side of the vessel, at a point one inch above the aorta orifice, there was an irregular slit, about three-fourths of an inch in length, extending obliquely across the artery, and which penetrated nearly the whole thickness of its proper coats, so as to leave only the corresponding covering of pericardium entire. Around this fissure blood was extensively extravasated into the meshes of the subserous cellular tissue, and thence by the giving way of the pericardium at the point before described, it had escaped into the sac of that membrane.

At the posterior part of the root of the arteria innominata, immediately beyond its origin, there existed a second rupture. This laceration, which, unlike the other, extended through the internal, and only a portion of the middle, coat of the vessel, was about one-third of an inch in length. From it the blood

had escaped between the laminæ of the middle coat, and had effected their separation along the posterior part of the arch of the aorta, for a distance of between three and four inches, forming a sac which terminated about one inch and a-half behind the left subclavian artery. The dissecting aneurism thus formed, occupied fully one-half the circumference of the aorta, and was obviously situated in the laminæ of the middle coat,—a distinct though thin layer of that tunic being reflected along with the external coat to form the outer boundary of the sac. The cavity of the aneurism terminated in a culde-sac which had no communication with the aorta, and which was filled by a firm clot of blood. The descending aorta was small in capacity, and its coats were atheromatous. The liver was large, indurated, and engorged.

The laceration of the arterial tunics which had first taken place in this case, and to which the production of the symptoms which supervened about eighteen hours before death must be ascribed, was doubtless that near the origin of the arteria innominata, and in connection with the dissecting aneurism. The obstruction to the free flow of blood through the aorta at that point, resulting from the compression of the natural canal by the blood extravasated in the newly-formed sac, was also probably the cause of the second laceration of the arterial coats, in the vicinity of the heart. This second rupture must necessarily have been almost immediately followed by death—the laceration having involved nearly the whole of the proper coats of the vessel, so that the covering of the pericardium must have been too feeble to resist, even for a very short period, the pressure of the blood effused beneath it.

ARTICLE VI.— *On Scorbutus at York Factory.* By WILLIAM SMELLIE, M.D., Edin.; and H. H. B. Co.'s Service.

THE settlement of York Factory in Hudson's Bay has long been remarkable for an annual outbreak of epidemic scurvy during the summer and first of the autumnal months, while other posts on the sea-coast are exempt. As the diet of the different establishments is nearly alike, the occurrence of scurvy at York Factory is a fact of peculiar interest in the investigation of the causes of this disease.

As topography and climate have often been thought to influence the development of scorbutus, I shall premise, first, a short account of the topography and climate of the three localities to which my observations refer. York Factory is situated on the north bank of Hayes' River, about five miles from its mouth; the country around on every side, as far as the eye can reach, is one level pine-swamp, in which the water is in a great measure stagnant during the open season of the year; the thaw seldom penetrates more than three or four feet; the banks of the river show an alluvial deposit of fifteen to twenty feet in depth, consisting of rich loam and blue clay of an offensive odour, scantily interspersed with granite and trap boulders; there is no kind of rock *in situ* to be found within many miles. Frost

continues from the middle of October to the middle of May, without any rise of the thermometer above the freezing point; in the winter months the cold reaches the intensity of 48°. The heat of the summer portion of the year varies from 35° to 90° Fahr.; and raw fogs from the sea frequently prevail. The other localities to which I will allude are Fort Churchill and Severn House. I have not visited either; but the former has, I believe, the advantage of a rocky site, and the country around to some distance is dry; the latter is, in some respects, similar to York Factory; at all, the climate is much the same, except that, at Churchill, it assumes a slighter degree of severity.

Scorbutus is unknown in Churchill and Severn House, while in York Factory it is very common, but only among Europeans. I have not seen a single case of scurvy in an individual born in the country. The number of persons at York Factory is about fifty, at Churchill about twenty, and at Severn about ten; while the number of cases of scorbutus at York amounts annually to at least one-fifth of the whole establishment; and this season, namely, from October 1847 to September 1848, the number amounts to nearly one-half. The dietaries of the three settlements differ very little from each other; and, as regards their scorbutic tendency, very little, if at all, as far as reliance can be placed on the theories proposed by late inquirers into the causes of this disease. River water is used for culinary purposes at all three; it can always be had pure; and is without trace of any vegetable matter, even at York Factory, where the river flows for a hundred miles above its mouth through a boggy pine-swamp country. The water of this river contains a considerable proportion of lime; and is sometimes slightly brackish during the summer. In some instances cases of scurvy have been ascribed, by the patients themselves, to bad water, and not without some show of reason.

Scurvy at this place is now, I believe, a much milder disease than it was many years ago, when deaths from it were not uncommon. There has been no fatal case for many years; a fact owing, I think, solely to the introduction of lime juice as a remedy. That others may judge of the quality of the diet which is used, I state the rations issued during one year to the men employed at York Factory. These vary very little in different years, and only in the proportions of the different articles, not in their nature. From June 1847 to June 1848, which is a fair average, the rations were, exclusive of tea and sugar, which the men purchase in considerable quantities as a luxury:—35 days, prepared barley; 9 days, Indian corn meal; 11 days, oatmeal; 10 days, round white peas; 38 days, salted pork; 7 days, salted beef; 95 days, fresh fish; 44 days, fresh fowl (ducks, geese, and partridges); 49 days, salted fowl (ducks, geese, and partridges); 12 days, fresh venison; 14 days, dried meat (buffalo meat or venison); 42 days, pemican—in all, 366 days; which gives, in one year, 65 days, farinaceous rations; 94 days,

salt provisions: 56 days, meat preserved by drying; and 151 days, fresh provisions. There is, in addition, a daily allowance of one pound of flour for each man. Kitchen vegetables are unknown; but wild berries of the currant, bramble, and whortleberry species, are plentiful during the autumn, and are generally indulged in. As already stated, scurvy makes its appearance during the summer and early in autumn, and disappears toward the month of September; a few cases happen sometimes in the months of January, February, and March, but these generally occur in individuals who have arrived from England the preceding autumn, after a sea voyage of eight or nine weeks,—leading to the conclusion, that the predisposing cause of scorbutus is the want of a due proportion of succulent vegetable matter in the aliment. In the officers' mess, in which are to be found salads, and a few potatoes and other kitchen vegetables for a short period of the year, I have not yet seen a case of scurvy.

But why are Fort Churchill and Severn House exempted from the disease, seeing that the diet of these establishments is equally deficient in vegetables? The theory of Dr Garrod (see this Journal for January 1848), which supposes the chief cause of scorbutus to be a deficiency of potash in the diet, and that a supply of this alkali in the form of its salts will be found an unfailing antiscorbutic, appears very plausible at first, when we consider that the pure citric acid is inert in scurvy; but the preceding table of the York Factory rations shows that the great proportion of the daily food consumed is not wanting in that alkali; and it may likewise be necessary to state, that these rations do not succeed each other in the order given in the table, but are alternated one with the other during the year. As to potash of itself proving antiscorbutic, I may observe that I had a patient, a few months ago, who took scurvy while he was taking daily nine grains of hydriodate of potash for a rheumatic affection of the joints. Neither are the rations deficient in nutritive azotised matters, the want of which is supposed by Professor Christison to be the cause of this disease.

Dr Anderson (see the Number of this Journal for September 1847), says:—"Food appropriate to man must consist of three parts—the nitrogenous, to nourish: the non-nitrogenous, to produce the extra heat required; and a third element to aid in the assimilation of these. This element is furnished by the various usually more or less acid juices contained in vegetables and fruits. In favourable circumstances man may dispense with them, and live in tolerable health, but withal obnoxious to disease; and if he be weakened, as by confinement or depression, or previous disease, or if the nourishment in the food be scanty, scurvy comes on." This is a guarded and correct statement of the case; but Dr A. has gone too far in ascribing the antiscorbutic property of milk to the lactic acid developed during its digestion. Other vegetable acids found during the changes which vegetable

matter undergoes, have not the same effect; why should the acid produced in badly fermented bread not be equally efficacious as an antiscorbutic? The individuals in this establishment, who use chiefly sour bread, do not on that account enjoy immunity from scurvy.

It appears, then, that "in favourable circumstances, man may dispense with the use of succulent vegetables or fruits, and live in tolerable health,"—as is evident from the condition of two of these settlements on the coast; but the nature of these favourable circumstances still affords matter for inquiry. The water appears in some degree to predispose to scurvy; because, when the disease comes on early in the season, before the frost relaxes, it invariably shows itself first among those engaged in procuring timber, at the distance of some miles, where they lodge in a hut from October till April, and where the water is more of a swampy and stagnant nature than the river water used at the factory. This is the only point in which their circumstances differ, as their rations are entirely the same, and their accommodation comfortable, and in some respects more salubrious than at the factory—their dwelling in the woods being less crowded and more cleanly.

The situation of York Factory is proved to be a very insalubrious one; but the particular cause of this I am unable to determine. It is certainly a swampy district, and were it not fortunate in having eight months of the twelve of continued frost, would probably become a perfect lazar-house; but the state of the system induced by residence here, is not that commonly resulting from marsh miasmata; and it prevails as much during the frosty season, when several feet of ice must be bored to reach water, as when the swamps are open. During the first twelvemonths of a residence at this place after arrival from Europe, with some few exceptions, a person becomes stouter and more healthy in his aspect; but after that period, he gradually assumes a sallow and cachectic appearance, with a predisposition to dyspeptic disorders, and bronchitic affections. Very few Europeans can maintain tolerable health for a longer period than five years; and, indeed, I would consider three years' continued residence to be as long as was safe for any individual of ordinary robustness. There are some persons, however, who have resided here for a great number of years consecutively, and have maintained tolerable health; some have even become corpulent; but all are sallow and cadaverous in their complexion, and time is soon traced in their features. It is very remarkable how much a year's residence in the interior improves the complexion, and apparently renews the youth; and those who have for any time resided in the interior, are apt to suffer more from a return to the sea-coast than even individuals fresh from the eastern world.

This state of the system induced by residence at York Factory, does not obtain at Fort Churchill and Severn House, the latter of which is similarly circumstanced as to diet, topography, and climate; and the former differs only in respect to its topography. With these

facts, then, before us, it still seems difficult to determine why scorbutus should prevail at York Factory, and be unknown at the other two places; and all that has been lately written on the causes of this disease, does not lead to a solution of the enigma. Dr Anderson's view, that "when a man is weakened by confinement, depression, or previous disease," he becomes more predisposed to suffer from scorbutus,—"when he is denied a sufficiency of succulent vegetable matter, or any other of the three things necessary to health,"—comes nearest to an explanation; but why, then, should individuals fresh from Europe, and during their first year's residence in this region, when in other respects they appear to improve in health and robustness, be the first affected with the disease?

FORT YORK, HUDSON'S BAY,
September 1848.

ARTICLE VII.—*On Anormal Nutrition in Articular Cartilages.* By P. REDFERN, M.D., London, Lecturer on Anatomy and Physiology, and on Histology, at the University and King's College, Aberdeen. (*Continued from p.* 990).

OBSERVATION V.—*Extensive Disease of the Bones of the Left Tarsus; Complete Destruction of some of the Articular Cartilages of the Foot, and Partial Disease of all the Rest; Amputation; Recovery.*

Alexander Johnston, æt. 41, farm-labourer, admitted into the Aberdeen Royal Infirmary, under Dr Keith, on March 20, 1849. Is considerably emaciated—complexion and irides light. States that he has always enjoyed good health, and lived regularly. About twenty years ago, a horse trod upon his left foot, and rendered him lame for some days, but he continued his work and was soon well. Two years ago, he noticed pain on the inner side of the left foot, which he ascribed to a sprain, and as it went off in a few days, he took little notice of it, and continued his work. About twelve months ago, he first felt pain on the outer side of the left tarsus, limited to a space about the size of a shilling; the pain being at times dull, at others, lancinating, and not regularly worse in the night than day;—he continued at his work for a month, when he gave it up, and since then he has never resumed it. He says the pained part was somewhat swollen; it was lanced by a surgeon, and blood alone escaped from the wound. It was again lanced, five months after the commencement, and pus then escaped:—at this time the pain was constant, but he could walk about. In December of the same year, an opening was made into a swelling which had formed on the inner side, and he says ten or twelve ounces of pus escaped.

On admission, the foot was considerably swollen, and there were two fistulous openings on the inner side, and several on the outer side, of the tarsus. Amputation was performed in the lower third of the leg, on March 31st, whilst the patient was under the influence of chloroform. Erysipelas took place in the stump two or three days afterwards—leeches were applied—suppuration occurred, and openings were made. At present (April 10) he is going on well, and his health has improved considerably.

Examination of the Foot.—The soft tissues are matted together, and the ligaments are softened and reduced to a pulpy mass, or altogether removed.

Astragalus.—The articular surfaces for the tibia and fibula are covered with cartilage, which is apparently healthy, except at the edges of the upper and

external surfaces, where it is reddened and abraded. The surfaces for the os calcis and scaphoid show parts covered with cartilage, which appears healthy —others with cartilage which is riddled with openings, exposing the bone; or, with patches of diseased cartilage here and there,—and others, which present a rough and bare surface of bone. The bone is soft and spongy, and its head is the part most diseased.

Os Calcis—The surfaces for the astragalus are similar to those to which they are opposed, but the parts from which the cartilage has been removed are covered with a soft and closely adhering gelatinous mass. The surface for the cuboid presents an opening, as large as the point of the little finger, leading to a large cavity in the anterior part of the bone, containing a reddish gelatinous mass, in which are spicula of bone. The posterior part of the bone is dense, and appears healthy.

Cuboid.—The posterior surface is covered with the soft pulpy mass, and it presents an opening, which is of the size of a small pea, and leads into the softened and spongy substance of the bone. The cartilage of the anterior surface is entire in the centre, but reddened and abraded at the edges. A few patches remain on the internal surfaces, which are separated from the scaphoid and external cuneiform bones by the gelatinous mass.

The scaphoid bone is rough and bare on its non-articular surfaces, and the articular ones in front are covered with cartilage, which is abraded at the ridges separating them,—the surface behind presents no cartilage, at two reddish and irregular spots, where it is covered with a soft and vascular membrane. The bone appears but little changed.

The cuneiform bones are all bare, soft, and spongy, below and in front. Their articular surfaces are covered with cartilage, which appears healthy.

The metatarsal bones appear healthy at their tarsal extremities, except the first and second, which are carious below and behind. The shafts are healthy, and the heads are covered with cartilage, which is very thin at the plantar edge of the surfaces in all the bones, whilst the head of the first is denuded of cartilage in several parts, especially at the lower edge. The cartilages of the phalanges appear healthy.

Microscopic Examination.—The soft-reddish mass, covering the bones, is composed of fibres, fusiform cells, and plastic cells, with a few irregular, rounded, and nucleated cells up to $\frac{1}{1700}$th of an inch in diameter (Fig. XXIX.); or, of a molecular mass, containing a few shrivelled granular cells, and granules of oil. The soft mass in the interior of the diseased bones is granular, and contains primitive filaments and plastic cells (Bennett)—(Fig. XXXI.) When acetic acid is added to the fibres, it partially dissolves them and leaves a granular mass. It causes contraction of the plastic cells, and shows their granules more plainly—(Fig. XXX.)

Fig. XXIX. Fig. XXX. Fig XXXI.

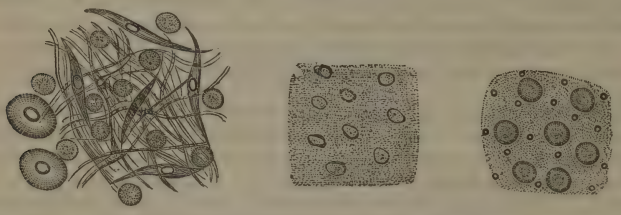


Fig. XXIX.—*Fibres, with fusiform, plastic, and nucleated cells in the soft mass covering the diseased bones.*
Fig. XXX.—*The fibres and plastic cells after the action of acetic acid.*
Fig. XXXI.—*Molecules, fatty granules, and plastic cells, from the interior of the diseased bones.*

The cartilage on the surface of the astragalus for the tibia is healthy, except at the edges: all the other cartilages are affected with disease of similar charac-

ters on all the surfaces. The cartilages of the phalanges are all diseased, though they appear healthy to the naked eye. In many places a distinct but thin membrane may be removed from the surface of the cartilage. This consists of horizontal or vertical fibres, mixed up with corpuscles discharged from cells, and in all stages of elongation. The hyaline substance on the surface, or in the deeper parts, may be seen in different preparations to become striated, and then split into fibres, the arrangement of which is horizontal or vertical, according to the original direction of the cells of the tissue. In every part, this change in the hyaline substance is associated with the enlargement of the cells, and the discharge of their corpuscles. Both changes are shown in Figs. XXXII. and XXXIII., taken from sections of the cartilage on the heads of the metatarsal bones.

Fig. XXXII.

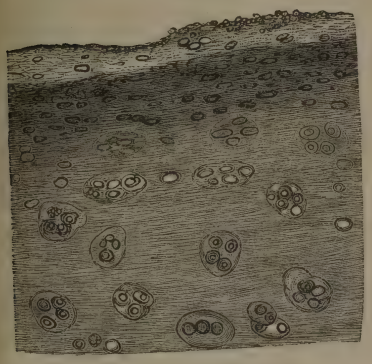
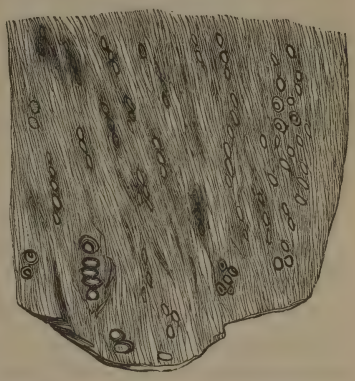


Fig. XXXIII.



Figs. XXXII. and XXXIII.—*Vertical sections of cartilage from the heads of the metatarsal bones, showing the changes in the cells and hyaline substance in the superficial and deep parts.*

Sections, taken from the tarsal end of the fifth metatarsal bone, show the superficial cells to be granular, and very much enlarged, many being $\frac{1}{250}$th, and a few $\frac{1}{140}$th of an inch long, resembling those in Fig. XVIII. The deeper cells measure on an average $\frac{1}{350}$th by $\frac{1}{400}$th of an inch. They are filled with corpuscles, closely packed together, and a few contain cells as well.

The posterior surface of the third cuneiform bone is so much diseased, that its cartilage is almost as loose as if stretched across a cavity, and yet the deep cells are but little changed in many places; in others, they are enlarged, rounded, and filled with corpuscles or cells. The superficial cells are very much elongated, and many contain large fat globules and granules—(Fig. XXXIV.); in some places the cells have opened on the surface, and this has a fibrous and nucleated covering, which presents nipple-like processes filling the cavities of the cells which have burst.

The cartilage on the anterior surface of the cuboid bone has its deep cells enlarged, rounded, and full of cells or corpuscles, the superficial cells being absent in many places, and their place supplied by a layer of fibres, $\frac{1}{80}$th of an inch thick, these fibres being arranged horizontally, and mixed up with nuclei, measuring $\frac{1}{1770}$th by $\frac{1}{7100}$th of an inch—(Fig. XXXV.) Where the superficial cells remain, they are elongated and granular, or they are seen opening on the surface. In one place alone are the deep cells finely granular and distended, as if about to burst, whilst the superficial ones are but little changed, and have no membrane covering them.

The other articular surfaces were carefully examined, and similar changes were found in their cartilages. Notwithstanding the bones of the second

row of the tarsus are so spongy as to cut like cheese, the disease in the cartilage appears to have gone on to the greatest extent on the surface.

Fig. XXXIV. Fig. XXXV.

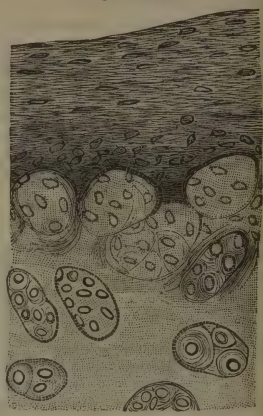


Fig. XXXIV.—*Vertical section of loose cartilage on the posterior surface of the third cuneiform, bone, showing the superficial cells enlarged, and containing fat globules and granules.*

Fig. XXXV.—*Vertical section of the cartilage on the anterior surface of the cuboid bone, showing the enlargement and bursting of the deep cells, and the fibro-nucleated membrane, with its nipple-like processes on the surface.*

Remarks.—There seems good reason to believe that this disease commenced in the bones of the second tarsal row,—that it involved the cartilages and the other bones in its progress,—and that it was of long standing. The changes in the structure of the cartilage were of a similar character to those described in the foregoing observations, whilst the occasional conversion of the nuclei into fatty granules and globules was perfectly shown. It is remarkable that so many of the cartilages should have been attacked on their free surface, whilst they covered bones so seriously diseased. The formation of the fibro-nucleated covering of the diseased cartilage was shown to take place by an actual conversion of the texture of the cartilage into it, whilst it was as evident that the membrane was not present until the changes, which are essential for ulceration or rapid absorption, had actually taken place; and, therefore, that the membrane was rather the result than the cause of these processes. Not the slightest attempt at anchylosis had taken place, for the disease continued extending in the bones after the destruction of their articular surfaces and cartilages, and was obviously of such a nature that it could not have been cured without the occurrence of a very material change in the nutrition of the diseased parts.

OBSERVATION VI.—*Scrofulous Disease of the Elbow-Joint; Ulceration and complete removal of large portions of the Articular Cartilages; Partial Anchylosis; Erysipelas; Amputation; Recovery.*

Wm. Howitt, æt. 23, farm-labourer, admitted into the Aberdeen Royal Infirmary, under Dr Keith, on March 19, 1849, for disease of the elbow-joint.

Complexion light—irides blue—has always enjoyed good health. States that his brother had sores about his ankle for some time, when sixteen or eighteen years of age, but that he is now quite well.

He says that he never injured his elbow, but, three years ago, he noticed it become stiff and painful. At this time, the surgeon to whom he applied lanced it behind, and evacuated some blood mixed with pus ;—he carried his arm in a sling for eight or nine weeks, when the elbow got so much better that he resumed his work, and continued it until a week previous to his admission, except at four periods, each of three or four days duration. On each of these occasions, the elbow was lanced, and pus and blood escaped ;—the first time, the opening was made in front, and twelve months after the commencement of the disease,—the other openings were made at the sides of the elbow. During the whole time the elbow was painful on pressure, there were occasional lancinating pains in it, and for some time each morning it was very stiff. The pain was never so severe as to keep him from holding a plough, or doing other farming work ; nor did it affect his appetite until the week previous to admission, when he was obliged to discontinue his work.

On admission, the left elbow is found much swollen, rounded, and pulpy to the feel ;—the skin is white, and presents five fistulous openings at different parts of the joint, but only one of these is now discharging. To the diminution of discharge he attributes the increase of pain, for he has noticed that the pain increased when the discharge diminished, and the reverse. On the evening of the third day after admission, he felt sick and generally unwell, and on the following day, the elbow was affected with erysipelas, which speedily extended to the shoulder. A blister was applied on the outside of the upper part of the arm, and the erysipelas at once stopped, and did not return. Suppuration, however, took place some days afterwards, and two or three ounces of pus were evacuated by an incision in the lower part of the arm ;—the abscess healed rapidly. Amputation was performed under the influence of chloroform on the 14th of April. The hemorrhage was troublesome at the time, and recurred, though a great number of ligatures had been applied. After the operation, he went on very well, and on the 10th of April the wound had healed by the first intention, except at a few small spots. He was dismissed cured on May 14, 1849.

Examination of the Limb.—The elbow is much swollen and rounded,—the numerous sinuses communicate with the joint on both sides. On cutting through the soft tissues, and especially the parts immediately under the skin, they are found infiltrated with a gelatinous substance ;—the ends of the bones adhere, and at last yield with a noise as of some part giving way ;—this is found to be the posterior part of the external condyle of the humerus, which adhered to the ulna, and has torn off a portion of bone from it. The trochlear surface of the humerus is covered with cartilage, both in front and behind,—the other parts are destitute of it, and are covered by a spongy fibrinous mass of a reddish colour, the mass and the bone below it being highly vascular. The ulna presents similar appearances. It has no cartilage except on the ridge separating the two surfaces of the olecranon and on the smaller sigmoid cavity ;—in most other parts, the bone is rough and bare, and, in the remaining ones, covered by a firm fibrinous and reddish mass. The radius has diseased cartilage on the surface around its head, and on the adjacent part of its humeral surface ; the other part of this surface is bare, or covered with a dense fibrous mass which has particles of bone adhering to it that have been torn from the humerus.

Microscopic Examination.—The fibrous substance is the same in every part, and consists of an indistinct granular mass, or of fully formed fibrous tissue with waved fibres, amongst which are patches of cartilage cells, which are very transparent, slightly granular, and evidently the discharged contents of larger cells. —(Fig. XXXVI.) On adding acetic acid to these structures, the fibres are rendered transparent, or converted into a granular mass, and the whole tissue is found studded with beautiful and elongated nuclei.—(Fig. XXXVII.)

Where the cartilage appears to be wholly absent and replaced by a fibrous mass, its corpuscles are yet to be found ; in some places, mixed up abundantly

Fig. XXXVI.

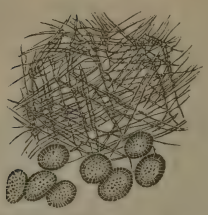


Fig. XXXVII.

Fig. XXXVI.—*Fibrous tissue and scattered cells from the soft mass on the articular surfaces.*
Fig. XXXVII.—*The same fibrous tissue, after the action of acetic acid.*

with a hyaline and softened mass ; in others, amongst fibres, granules, fusiform cells, and spicula of bone. The last-named elements constitute the soft, reddish, and vascular mass found on many parts uniting the opposed bones. The texture of the bone at such places is exceedingly soft, and has no defined surface, but its cells appear to be opened up, and spicula are found projecting into the soft mass, so that it is scarcely possible to obtain any portion of the fibrous mass from the surface without spicula. The vessels appear to spring from the bone. They render the mass excessively vascular, but wherever the cartilage cells remain in a hyaline substance, no vessels ramify amongst them, though the part is of a brownish-red colour, as in case No. I., where there were no blood-vessels except at a great distance. In some parts, the fusiform cells are very numerous, and amongst them are nucleated cells of different sizes, rounded, irregular, or caudate, and blood corpuscles.

Sections from the thickest portions of the cartilage of the humerus, ulna, and radius, show cells near to the bone, of nearly healthy character ; but in the greater number of places, even these cells are enlarged, rounded, and contain corpuscles, of which the mean size is $\frac{1}{5400}$th by $\frac{1}{2300}$th of an inch, the cells themselves measuring $\frac{1}{230}$th by $\frac{1}{400}$th of an inch. On approaching the surface, the enlarged cells are seen to burst and discharge their corpuscles, which are found in a finely granular gelatinous mass. Many of the corpuscles are seen considerably elongated, and others become gradually lost in the fibrous tissue found in many parts on the surface. In a few places the hyaline substance is split into bands and fibres on the surface, as was more perfectly seen in case IV.—(Fig. XXXVIII.)

Fig. XXXVIII.

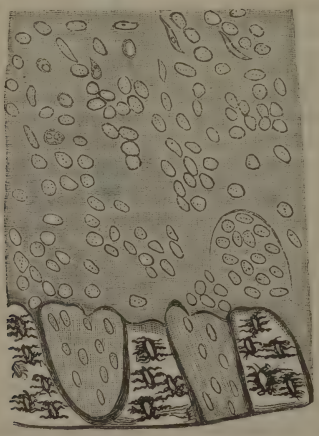


Fig. XXXVIII.—*Vertical section of the cartilage of the humerus, showing the changes in the cells and the elongation of the corpuscles.*

Remarks.—This was a chronic disease in a scrofulous constitution, and it destroyed the whole of the tissues in the interior of the joint, with but little suffering. The affection of the cartilages had gone on to the removal of the whole superficial layer of cells, whilst the deeper ones were shown to be undergoing the usual changes, but to have suffered to a less extent, so that there can be little doubt that the disease commenced on the free surface. The discharged corpuscles of the cells were seen lying on the surface in hyaline substance, or among a mass of fibres; they were considerably elongated, and at many parts appeared to have become transformed into fibres. When the fibrous tissue was acted upon by acetic acid, a number of nuclei were seen, which appeared to be the remains of the elongated corpuscles. These bodies could not have been inactive in the formation of the fibrous tissue, and they may have been concerned in this process in one or both of two ways—viz., by elongating into fibres, or by causing the hyaline substance to split into fibres, as in the cases before related, and as very evidently took place, in some parts, in the present instance. The fusiform cells appeared to have grown in the usual way in the blastema, formed of newly exuded matters and the disintegrated substance of the diseased cartilage. The fibrous tissue, found where the cartilage had been destroyed, was becoming perfectly organised; whilst the texture of the bone was opened out, and spicula were shooting into the fibrous mass for the purpose of consolidating the union between the bones. It is a matter of extreme interest and importance to ascertain in what way the new bone in anchylosis is really formed; and in connection with the subject, I may refer to the elevations of the surface of the bone, in Observation IV., which appeared to have taken place at those parts from which the cartilage had been removed by former ulceration, or a chronic process of the same character.

Observation VII.—*Cancer of the Liver, Brain, Kidney, and of every other Organ except the Heart; Cancerous Deposits in the Right Psoas Muscle, on the transverse processes of the Lumbar Vertebræ, and on the Right Ilium and Femur; Disease of the Cartilages of all the articulating processes of the Lumbar and several of the Dorsal Vertebræ—of the Fibro-Cartilage of the Right Sacra-Iliac Synchondrosis—of the Cartilages of the Right Hip and Knee-joint, —of both Elbow-joints, and of the Carpal Articulations;—Tubercle and Cicatrices at the Apices of both Lungs;—Granular Degeneration of both Kidneys; Epileptic Convulsions; Partial Paralysis; Death.*

This was the case of a widow, æt. 48, who had a son before marriage, but no children since that time. She was stout and healthy up to January 1849, and gained her livelihood by carrying large baskets of fish to the country, and by selling dulse (Rhodomenia Palmata), which she herself pulled, standing in a considerable depth of water. When she went to the country, she was generally away from home for two or three days at a time; but she lived regularly, and was never known to have taken spirits of any kind. In January she began to complain of pains in the right hypochondriac, epigastric, and in the lumbar regions, with œdema of the lower extremities. Her complexion became sallow,—the symptoms above-named continued to increase in intensity,—she had

a distinct epileptic fit on April 18th, and another imperfect one on the 26th, in which she died. A complete account of her symptoms, and of the characters of the cancerous and other disease, will be given in another paper.

In the post-mortem examination, made in the dissecting-room, corresponding portions of the ventral and dorsal aspects of the right ilium, half-an-inch distant from the anterior superior spinous process, were found uncovered by periosteum, over a space as large as a half-crown piece. On these spots there were cancerous deposits as large as the halves of a large walnut, by the sides of which there were numerous small nodules on the bone; whilst, under the deposits, the bone was rough but presented a plane surface. Behind the trochanter major of the right femur, a similar deposit was found, lying also on rough bone, not covered by periosteum. In all these parts, the deposits of cancerous matter appeared to have taken place in the substance of the periosteum, or between it and the bone, and their histological characters were those of the larger masses found in the different organs.

In the substance of the right psoas muscle is another extensive and elongated deposit, lying upon the transverse processes, and the sides of the bodies of the second and third lumbar vertebræ, which are not covered by periosteum at the parts named. The right articulating processes of the second, third, and fourth lumbar vertebræ are considerably enlarged, but their texture is dense, with the exception of the upper articulating process of the second, which is infiltrated with the yellowish deposit, whilst its transverse process is nearly destroyed at its middle, and broke off during the examination.

The cartilage on every articular process of the lumbar vertebræ, and those of most of the dorsal ones, is diseased. The right articular surfaces of the 2d, 3d, and 4th lumbar have scarcely any cartilage left upon them, the bone being bare and rough, or covered by reddish velvetty patches of diseased cartilage. The cartilage of the other processes appears affected most seriously at the edge of every surface, where it is reddish and velvetty or flocculent. The cartilages on the articular facettes of the bodies and transverse processes of the dorsal vertebræ are similarly diseased, but to a less extent as we proceed upwards in the column.

Microscopic Examination shows the superficial cells absent from most parts, and granular where they still remain. In many places the cells are as much enlarged and rounded as in Obs. I. and IV. They contain a number of other cells, each of which has several bright granules in its interior. In some places the cells have burst and discharged their contained cells on the surface or into the tissue. The hyaline substance, in the velvetty parts, presents a mass of fibres and bands free on the surface.

The bones separate very easily at the right sacro-iliac synchondrosis, and the fibro-cartilage joining them contains very large elongated cells, crowded with others which are granular; or they contain small patches of granules, indicating the original position of their contained cells or nuclei. Most of such cells lie in clusters, and are placed vertically to the surfaces of the bones. They lie in an indistinctly fibrous intercellular substance, which is highly transparent. —(Fig. XXXIX.)

On the head of the right femur, and at the bottom of the acetabulum, near the attachment of the round ligament, the cartilage is fibrous, velvetty, and reddened. On being examined, its hyaline substance is found converted into bands and fibres, whilst the cells are very much enlarged and crowded with corpuscles or cells, which are granular.

Right Knee-Joint.—The cartilage of the patella is velvetty and rough in the centre, but denser and smooth near the circumference; that of the femur, at the inner edge of the junction of the inner condyle with the trochlea, is depressed and velvetty; but apparently healthy in other parts. The cartilage of the external glenoid cavity of the tibia is soft and velvetty in the centre, but dense and apparently more healthy at the circumference.

Microscopic Examination shows that the velvetty parts of these cartilages

Fig. XXXIX.

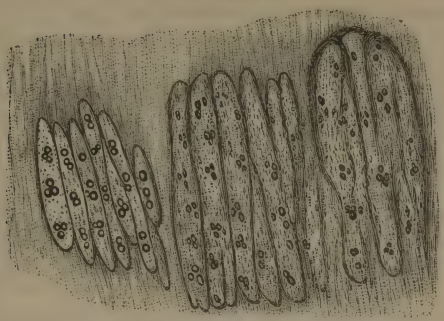


Fig. XXXIX.—*Portions of fibro-cartilage from the right sacro-iliac synchondrosis.*

have healthy cells and hyaline substance in the deep parts; at the surface, the hyaline substance is split into bands and fibres in the usual way, whilst all the cells contain granules, collected together into small patches, which occupy the normal position of the nuclei. These granules are found in all the cells, whether belonging to the superficial or deep layers. In many places, all the cells of the superficial layers are completely removed.

Upper Extremities.—The cartilage, on the ridge separating the small head of the humerus from the surface for the ulna, presents, in both limbs, a linear excavation running from before backwards. The cartilage on the larger sigmoid cavity of both ulnæ is divided into two parts by a thin band of membrane, which passes across at the junction of the olecranon with the shaft, and is the only substance which covers the bone at that part. On examining the structure of the cartilage near the excavations on the humeri, it is found converted into fibrous tissue on the surface, whilst a thin layer of cartilage is applied to the surface of the bone. The membranous band on the ulnæ is of a fibro-nucleated character, similar to that covering the bone in the depressions on the femur in Obs. IV.

Several patches of cartilage on the bones of the carpus present similar diseased appearances to those before described in the other surfaces. In the joints of which no mention has been made, the cartilages are healthy.

Remarks.—Owing to the co-existence of the disease of the cartilages, in this case, with the general cancerous affection, it is of great importance to ascertain whether the former was produced by the latter affection. The characters of the disease in the cartilages were not affected by the cancerous disease, for they were precisely those which are found in all similar states of disease without cancer. There was no deposit of cancerous matter in the cartilages, nor any degeneration of them other than is usual, especially in old persons. And yet the greater extent of disease on the surfaces of the lumbar than of the dorsal vertebræ appears connected with the great amount of cancerous affection of the abdomen, and it was probably the result of the increased activity of the circulation, which we may suppose to be necessary for the deposit of large masses of cancer, tubercle, or other exudations. It is to be noticed, that on all the articulating processes of the vertebræ, the disease commenced at the edges of the cartilages, whilst it was found in the centre of the surfaces alone, on the right patella and tibia.

OBSERVATION VIII.—*Gelatinous Degeneration of the Tissues of the Tarsus and Ankle; commencing disease of the Cartilages; Amputation; Recovery.*

George Prosser, æt. 9, a schoolboy, admitted into the Aberdeen Royal Infirmary, under Dr Keith, on April 24, 1849. He is remarkably intelligent—complexion light—irides grey. His father is a stout healthy man; but his mother is of a scrofulous diathesis.

He suffered from an affection of the chest, with pleuritic effusion, about a year ago, but recovered tolerably well. A short time before Christmas 1848, he complained of pain in the region of the right ankle, but no swelling or other positive mark of disease was noticed until about a month afterwards, when the ankle was evidently swollen. It was still painful, especially on moving. For three months after the commencement of the disease, he continued to walk to school as before; but, at the end of this time, he could not wear his shoe; and before admission, the pain got so much worse as to prevent him from walking.

On admission, considerable swelling exists about the right ankle-joint. He has pain in it equally bad during the day as in the night. This frequently occurs in paroxysms without any evident cause, and is so severe as to make him cry aloud. It is also produced by any movement of the joint. He cannot walk, and he was carried to the hospital.

An opening was made, three weeks after his admission, on the outside of the ankle-joint, and a small quantity of pus escaped. This relieved the pain very considerably; the wound did not heal, and fungous granulations protruded shortly after from an ulcer of the size of a half-crown piece, occupying the position of the opening. The patient became emaciated, hectic came on, and amputation in the lower third of the leg was performed on June 9th. The stump healed rapidly, and his general health improved from the day of the operation.

Examination of the Foot.—Considerable swelling exists in the region of the tarsus, extending back as far as the ankle. In front of the external malleolus is an ulcer of the size of a half-crown piece, with fungous granulations protruding. The skin on the inner side is healthy, but appears thinned. The tissues surrounding the ankle and tarsal articulations are converted into a reddish-brown gelatinous mass, mostly at the parts close to the bones; but this change does not extend in any considerable amount to the tarso-metatarsal articulation. There are no effusions in any of the articulations; and the cartilages, on a superficial inspection, appear healthy. When examined more minutely, those of the ankle-joint, and of the articular surfaces of the first row of tarsal bones, appear slightly reddened in some places, and at their edges are covered, for an extent of a line or more, by a highly vascular and pulpy membrane (synovial), which is less distinctly seen on the scaphoid and cuboid bones, and does not exist on those further in front, at least in the same vascular and pulpy state. All the bones of the tarsus are softened, and cut like cheese.

Microscopic Examination.—The pulpy membrane is full of loops of capillaries, amongst which are numerous fusiform corpuscles and plastic cells, or granular ones, precisely like the latter. The pulpy mass outside the joints has a similar structure. The cartilages are all affected in the same manner. In the most diseased parts, the superficial cells are rounded and enlarged to two or three times their usual size. They contain dark granules, and are displaced so much from their horizontal direction, that many lie obliquely or vertically in the finely granular hyaline mass, which separates them very considerably from each other. The deeper cells of these parts are granular, but not perceptibly changed in size or position.

In the parts least diseased, the cells of the surface are broader than usual, but scarcely as long as healthy cells;—they are separated from each other, and contain no evident nucleus, but appear finely mottled. The central parts of most of the surfaces are nearly healthy.

Remarks.—This was a case in which there were many symptoms which are usually believed to indicate the existence of serious diseases in the articular cartilages, and yet the affection of these textures was so slight, as to be found only after a very careful examination. There were the usual appearances produced by the gelatinous disease of the synovial membrane, and the texture of the bones of the tarsus was opened out, so that they were all very much softened. The affection of the cartilages had evidently commenced at the circumference of their free surfaces, for all the deep parts were quite healthy. That the disease in the cartilages should commence at the parts just named is not surprising, when it is considered that, in adult articular cartilage, these parts are very freely supplied with blood, being covered with a distinct reflection of the synovial membrane, and the *circulus articuli vasculosus* of Dr William Hunter. The vessels on the surface of the cartilage, at the parts named, become distended with blood; an anormal nutrition is going on in all the parts surrounding the joint, and this process may be expected to extend to the cartilages, as it was found to have done in the present instance. If articular cartilages are chiefly nourished by the vessels of the bone, as is believed by Mr Toynbee, it is very singular that they are not more frequently attacked by disease on their attached surface, especially in cases where there is extensive disease in the bones.

(*To be continued.*)

Part Second.

REVIEWS.

Lectures on the Causes and Treatment of Ulcers of the Lower Extremities. By GEORGE CRITCHETT, Esq., F.R.C.S., Assistant Surgeon to the London Hospital, Lecturer on Practical Anatomy, &c. &c. London, 1849.

THIS treatise has for its object the recommendation of an improved method of affording mechanical support, by strapping from the toes upwards, in the treatment of most ulcers of the lower extremity, more especially of the indolent sore, and where there is a varicose state of the veins.

In the common method, with which Baynton's name is connected, the limb is bandaged up to near the sore, the straps are then applied from a little below to a short distance above it, and with moderate

tightness, and over this the bandaging is completed. The patient may then either walk about or keep in bed, according to circumstances, and the strapping is renewed usually every second or third day. Mr Baynton's method has been known since 1799, and our author informs us that the improvement upon it, now under consideration, was recommended in 1828 by Mr John Scott, whose name is associated with a well-known method of applying pressure and insuring rest in chronic swellings, more especially of the knee-joint. But, as his method of strapping for ulcers is mentioned only briefly in the treatise on the joints, and has not been brought forward, at least prominently, in subsequent writings, it has never attained the position and notoriety which Mr Critchett desires to ensure it.

The straps are applied so as to envelope the whole limb, from the roots of the toes upwards to beyond the ulcer. The straps are about two inches in breadth, and from twelve to eighteen inches in length, so that their ends cross for some distance, and each is overlapped by the next, in the usual manner. Care is taken to cover every part well, and to apply the straps with uniform tightness; and over all this an eight-yard bandage is carefully applied.

It is easy to see that by strapping thus applied a firmer and less easily loosened support is given to the parts below the ulcer than by the bandage only, and that therefore greater pressure may be applied over it without danger of partially strangulating the limb, as when, in following Baynton's method, the circular straps are applied too tightly. But the question occurs, is it necessary or advisable to have recourse to this more troublesome and expensive method, as a general practice? Mr Critchett has much fault to find with Baynton's: the strapping is apt to strangulate the limb; the bandage does not give uniform and efficient support; and the plan is not to be depended upon, unless the patient is confined to bed. In proof of this, he quotes from various eminent writers expressions of caution regarding its use or abuse; but these are not stronger than some used by Mr Critchett himself, when giving caution concerning the improper use or occasional inapplicability of his own adopted plan, of which we could easily quote several examples.

In addition to the advantage of Scott's plan, as stated above, our author informs us, that the cure by it was much more rapid and permanent than by any of the other means in use, and that his patients could follow their usual occupations with comfort; and that, from the latter circumstance, the expense (which amounts to about sixpence each dressing) becomes a minor consideration. He has known strapping well applied in this way remain unmoved for many weeks, and even months, but it usually becomes loose in two or three days, from the absorption caused by its pressure, and in the majority of cases he renewed it twice a week.

The applicability of the "universal strapping" to the various

kinds of ulcers, and in the different stages of each, is discussed at great length; and it appears to us, that the classification of ulcers here put forward has reference chiefly to the relation they bear to his favourite method of cure. His acute ulcers do not bear strapping of any kind. The sub-acute do not first agree even with short straps crossing the sore without encircling the limb—a plan which he finds useful when the other may not be required or cannot be borne; and when the universal strapping is begun, it must be applied somewhat loosely. But it is in the indolent sore that the plan is so advantageous, or rather, it would seem, indispensable, for it would appear that all others are deemed quite unequal to the task—even Mr Syme's speedy and simple method by blistering, although it is admitted that it attacks the root of the evil, and that it was tried with marked benefit. He does not agree with most systematic writers in setting aside the varicose ulcer as a distinct one, on the ground that any kind of ulcer may exist on a varicose limb; but his varicose ulcer is a new one, as, singularly enough, he has not yet met in surgical writings with a correct description of its appearances. But it is in this kind of ulcer—where the limb is in that puffy and spongy state, which is occasioned by the varicose condition extending from the larger to even the minute veins—that the universal strapping effects, as it is said, the most signal triumph. It cannot be applied too tightly, all pain at once ceases, and the cure is rather accelerated by the patient moving about. It is recommended to continue the strapping in such cases for two or three months after the ulcer has healed; and in some cases, it was found desirable to continue it constantly, as, in severe cases of varicose enlargement, he reckons the universal strapping, when skilfully applied, much preferable to any known means. Irritable ulcers were found at first to resent the strapping, but Mr Critchett now finds that for this kind of sore the straps should be applied very tightly, with in fact, as much tightness as our muscular power will permit. Under this high-pressure system the agonising pain ceased, and the sore at once began to progress favourably towards a rapid cure.

In reading the observations on specific sores, we were chiefly struck with the view taken of the treatment of the "menstrual ulcer." Objection is made to the doctrine, that the attention should be first directed to the uterus. Mr Critchett says, "Heal the ulcer, and the uterine function will be speedily restored to health and regularity;" and again, "The ulcer being healed, the uterus spontaneously resumes its healthy and regular function—at least such has been my experience."

We think that we have now noticed all that is worthy of record, either from its novelty or interest, in this work; and we are not satisfied with the author for having obliged us, for this, to search through 120 pages. We imagine that, in less than six lectures, the pupils at the London Hospital might have been presented with a

more appropriate view of the subject of ulcers; for instance, one modelled after that which may be found in much smaller compass in one of the recent text-books of the "Young Surgery" school, against which Mr Critchett seems inclined to enter the lists. Nor, surely, is that of a series of lectures intended for the student—and in which there is little more than one good idea woven up with a lengthened disquisition, destitute alike of other novelty and of completeness—an appropriate form in which to come before the profession, on a subject with which their patience has even very lately been severely tried. We think that our author is too anxious to discover the inefficiency of other plans of treatment, and the endeavour to make out a strong case against these, in no way strengthens his own. Thus we do not think that a well applied bandage necessarily, to use the author's words, cuts in at one place and gives way at another, or that our rollers are usually so perverse as to become loose where pressure is most needed, and to get tight just where it is injurious. Indolent ulcers are common enough everywhere; and we do not recollect any case at a large hospital, during a series of years, which did not heal satisfactorily enough when either Baynton's or Mr Syme's method was fairly tried. In the former, we found the well-applied bandage serve the purpose better than it seems to have done in the London Hospital; and, with a moderate starching, our simple roller might be found quite as efficient and permanent as the universal strapping. It may be conceived, too, with what surprise we (in Edinburgh) read, under the head of sub-acute ulcers, of the "water-dressing," that "I can scarcely recal a single case of importance, out of many in which I have fully tried this remedy, that I have been able to bring to a successful termination by this application alone." Verily, our author should come to Edinburgh, and see how simple ulcers get well under "Young Surgery's" water-dressing, which he uses, not as a "remedy," but merely as a means of keeping the sore clean and moist, so as not to prevent nature, aided by posture and the simple bandage, from working her own cure.

We have been led to make these remarks simply because our author makes far too much of his favourite plan as compared with others. We really think it likely that the careful application of strapping from the toes upwards will be found a most efficient means of affording uniform support in any case where considerable circular pressure is required above; and the same practice might be applied, in certain circumstances, to the upper extremity. The practice has long been in general use at the London Hospital, where Mr Critchett, with an ample field for experience, has for ten years devoted especial attention to the treatment of ulcers; and the profession are now indebted to him for reviving and bringing under their notice a plan by which the cure of certain cases may be accelerated.

Practical Observations on the Diseases of the Uterus. By ROBERT LEE, M.D., F.R.S., Fellow of the Royal College of Physicians, London, &c. Parts I. and II. With Nine Coloured Plates from Original Drawings by Mr Perry. Folio. London, 1849. Pp. 20.

THE work before us is in two parts; one of which was published in 1846, the other during the present year. It consists of a series of coloured illustrations of uterine disease, executed on stone in a rather peculiar, but generally very effective, style, which is all the more pleasing and true from the absence of that glaring and overcharged colouring so frequent in pathological drawings. Two sections of letter-press are added, in which many cases and observations are detailed from Dr Lee's personal experience, and interspersed with practical remarks, which show the able and experienced physician, and give considerable interest to the details.

Section I. treats of the symptoms and morbid changes produced by malignant disease of the uterus. This we regard as, in every point of view, the least satisfactory portion of the work. The illustrative drawings cannot for a moment be compared with the beautiful illustrations of cancer in Cruveilhier's large work; and we are confident that neither the drawings nor the descriptions will be found to bear out the statement of the author, "that the fungoid tumour of the uterus, or cauliflower excrescence, scirrhus, carcinoma, and corroding ulcer, are merely different forms of the same malignant disease." For example, Plate IV., Fig. I., gives a tolerably distinct example of the corroding ulcer: a disease not preceded or accompanied by any peculiar morbid deposit, and which we have been accustomed to consider as a non-malignant, though certainly very dangerous ulceration, bearing much more resemblance to ordinary phagedæna than to cancer, and, under favourable circumstances, amenable to local treatment. We are not aware to which of the numerous cases described, the plate refers; but in case 38, which bears the nearest resemblance to it, we find nothing to alter our opinion, and indeed everything to confirm it, from the absence of any morbid deposit in the immediate neighbourhood of the ulcer. In the investigation of a point of so much importance, we should have expected, not, certainly, a greater *number* of cases, but far more careful analysis of them, and more pains taken in their description, to convey precise ideas as to the distinguishing characters of malignant disease, in all the various forms which, according to the author, it assumes in the uterus. Failing this, we cannot, with all our respect for Dr Lee, allow ourselves to be carried away by the unsupported assertion of his opinion; but must continue to believe, that he has included under one name diseases very distinct in their essential nature, origin, and progress.

In regard to the symptoms and treatment of malignant diseases

of the uterus, we find little, as might be expected, either encouraging or novel. We may remark, however, that the author's judgment, that the destruction of the os uteri by caustic or other means, is not "justifiable in any case," becomes a dangerous rule of practice when applied to corroding ulcer or cauliflower excrescence, in both of which affections, whether malignant or not, there are now numerous examples of life prolonged and health restored, by excision or cauterization of the affected parts.

We pass with pleasure to the second section, which treats of fibrous tumours and polypus of the uterus, and contains, we think, both sounder doctrine and better description than the former. The account of the fibrous tumour is admirable, and would have been all that could be desired, had something been added to our knowledge of the minute structure and source of these growths. On these points Dr Lee is silent; but all that investigation by the unaided eye can do is here faithfully given. In common with most other pathologists, Dr Lee states, that "the greater number of uterine polypi are fibrous tumours, which have been formed under the lining membrane, and a stratum of muscular tissue," and project into the cavity, carrying before them an envelope of mucous membrane and more or less of muscle. The thickness of the pedicle of the polypus, and the length of time required for its formation, are proportionate to the amount of muscular substance forming its envelope. The drawings are admirably executed, and are well adapted for illustrating these views. The cases and descriptions are also very clear and satisfactory. The other varieties of uterine polypi are well described and figured. The most important and frequent is that composed of cellular tissue and mucous membrane, and similar in structure to the nasal polypus. Other common varieties are the enlargement of the Nabothean glands, and of the penniform rugæ of the cervix uteri.

Dr Lee narrates, at some length, the history of opinion in regard to uterine polypi, showing that up to the present century they were practically confounded by pathologists with malignant growths. We think he scarcely gives an adequate view of the investigations of the more recent pathologists on this subject. We should have expected some account of the splendid labours of Cruveilhier, whose plates are all but complete in reference to this subject, and the accurate descriptions of Andral and Rokitansky might have been referred to with much advantage to the reader;—being, as they are in most points, an important confirmation of the views laid down by the author.

In the removal of uterine polypi, Dr Lee recommends the use of the forceps for small, and the double canula and ligature for large, polypi; and the cases contain many interesting practical details.

We feel bound to remark, that, in the execution of the drawings, the second part is very greatly superior to the first, and, in truth, forms a very beautiful specimen of pathological lithographic draw-

ing. We have no hesitation in pronouncing this work a most acceptable addition to the pathological literature of this country; and although we have not been able to agree with Dr Lee in all his opinions, we have great pleasure in expressing our sense of the value of his present labours, and in assuring him that we shall ever have real satisfaction in finding him adding to his well-earned reputation as a practical and pathological writer. Meantime, we regret to observe, by the publication of a title page with the present part, that it is not the author's intention to proceed further with these illustrations of the large and important subject of the Diseases of the Uterus.

Observations on the Pathology of Croup; with Remarks on its Treatment by Topical Medications. By Horace Green, A.M., M.D., &c. &c. New York, 1849. 12mo. Pp. 115.

We do not think that this work deserves either the ungenerous depreciation which it has received from a leading review in the country of its birth, or the somewhat high estimate with which it has been favoured by some of our British contemporaries. It is simply an attempt to introduce into the treatment of infantile croup the practice of applying remedial agents directly to the larynx itself, by means of a sponge and bent whalebone probang introduced beyond the glottis; a practice certainly differing in a very material point from that of MM. Trousseau and Belloc, who content themselves with applying the sponge to the lips of the glottis, and expressing the fluid into the larynx. Dr Green employed this method first in the adult, and published, some years ago, a work strongly advocating its employment in ordinary laryngittis. Its application to the case of children is a bold step, but one which does not seem to have been taken unadvisedly, or without due precaution on Dr Green's part. There can be little doubt of the practicability of the operation; and to its safety and efficacy in laryngeal affections the author pledges himself, on the strength of numerous trials both in the adult and the infant. We must say that we think the method deserves a cautious trial; and should it be found to bear out the testimony in its favour borne by this and the former volume of Dr Green, we do not see on what principle he can be deprived of the credit which attaches to all persevering endeavours to introduce new and useful modes of practice, especially as he has not depreciated the received methods, but strongly advocated their conjunction with his own.

Having, however, made this admission, we must say that there are some points in this work, as well as the former, which undoubtedly go far to justify the suspicion with which they have been received in some quarters. Small as this volume is, it is one-half too

long. The author treats of the pathology of the disease at a length quite out of proportion to the importance of what he has to say; and contrives, in doing so, to make many blunders, arising from a love of innovation, combined with a too implicit reliance upon old authorities. This circumstance is, we fear, not calculated to inspire confidence in the author's powers or opportunities of observation. Thus, when he asserts that croup is a *specific* disease, because it is characterised by an inflammation attended with an *exudation of plastic lymph*, he is not aware that he is giving a character common to all inflammations of a certain degree of intensity; and in maintaining the croupous exudation to be an altered secretion, not of the membrane, but of the muciparous glands, he overlooks the fact that the structures in question are the essential constituents of the membrane, and enter into all its morbid processes without exception. In fact, his whole pathology is in the highest degree confused, and cannot be said to have added one fact to our previous knowledge. The work is likewise disfigured by literary errors of a very gross description.

The practical part of the work refers both to the local and general treatment of croup. We have already given the author credit for the attention which he appears to bestow on the latter, amid his enthusiasm for his own particular plan. There is nothing in his general treatment which calls for notice, except his rejection of tracheotomy, on the very absurd ground, that death in croup does not take place by asphyxia, as "three-eighths of the canal are always open, constituting a space quite sufficient for the transmission of all the air necessary for the maintenance of the process of respiration."

The topical remedy recommended by the author, is the solution of the nitrate of silver in the proportion of two scruples to a drachm of the salt in an ounce of distilled water. Its application directly to the laryngeal mucous membrane in disease is usually followed by a copious expectoration of mucus, which is rapidly secreted, and affords great and immediate relief by its discharge.

The instrument used by the author is merely a piece of whalebone bent to the proper curve, and covered with a fine round sponge, from one-third to half-an-inch in diameter.

The efficiency and safety of the remedy is thus adverted to:—

"Ordinarily I have applied in croup a solution, composed of from two scruples to a drachm of the salt, dissolved in one ounce of distilled water. A remedy of this strength I have applied freely to the fauces, pharynx, and into the larynx of young children, in a large number of cases during the last eight years, and in no single instance have I observed any indications of the danger of suffocation from its employment. On the contrary, I have repeatedly observed, and have once before remarked, that much less bronchial irritation is produced by the application of the nitrate of silver into the larynges of young children who are suffering from croup, than when it is introduced into those of adults who are affected by chronic disease of the larynx.

"In cauterising the cavity of the larynx in the above disease in adults, I

have advised on a former occasion, that the aperture of the glottis should not be passed until after the parts in the faucial and pharyngeal region had been prepared by having the solution applied for a few times to the pillars of the fauces, the epiglottis, and about the opening of the glottis. Proceeding in this manner, it has been shown that the instrument may then be passed into the larynx without producing half the amount of that irritation which its introduction below the epiglottis would have awakened without these preparatory steps.

"Happily, it is not necessary to take these precautionary measures, before employing the topical remedy, in the treatment of croup in children;—for, as we have seen, applications of the argentine solution of a proper strength may be employed without apprehension in these cases; and these applications should be made promptly to the tonsillary and pharyngeal regions, whenever the symptoms present indicate the commencement of the exudative inflammation in the mucous membrane of these parts."—Pp. 81-83.

In proof of the efficacy of the treatment, we must refer the reader to the author's cases, which, if faithfully detailed, certainly present strong evidence in its favour.

We close our notice with the following extract, in regard to the rules for the application and repetition of the remedy:—

"An application may first be made to the tonsils, and about the opening of the glottis. After a delay of from fifteen minutes to an hour, the operation may be repeated, and the sponge, wet with the solution, should then be passed into the glottis. The cauterisations may be repeated once in two, four, or six hours, according to the effect produced, and the intensity of the disease.

"When the symptoms indicate that the disease has extended into the tracheal divisions, or when the affection is complicated with inflammation of the bronchi, the applications should be repeated more frequently, in order that some of the solution may find its way over the mucous surface of the larynx and trachea into the bronchial divisions."—P. 90.

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVIII.

MEETING XVI.—WEDNESDAY, *July* 18, 1849.—JAMES SYME, Esq., President, in the Chair.

SCIRRHOUS DISEASE OF THE PERITONEUM.

Dr W. T. Gairdner showed some specimens of cancerous disease of the peritoneum, from the body of a patient who died in one of Dr Alison's wards in the Infirmary. The symptoms during life had been jaundice, and pain in abdomen; there were also signs of fluid effusion. Two pounds of clotted blood were found in the peritoneal cavity, but the source of hemorrhage could not be detected, even when the great vessels were injected with water. Masses of decolourised blood were found in the abdominal cavity, and adhering to the intestines.

The peritoneum enclosing the lesser curvature of the stomach was indurated and thickened, the disease extending nearly up to the cardia. The mesentery was also thickened close to the intestines, and the appendices of the colon enlarged and hardened. Small cancerous tubercles were seen adhering to many parts of the peritoneum. Characteristic cancer-cells were detected, with some difficulty, on examining the tubercles from the pelvic peritoneum. The cancerous disease was confined entirely to the peritoneum ; the mesenteric glands were not affected. Drs Christison and Newbigging had recently had under their care a somewhat similar case, in which the cancerous disease commenced in the serous membrane, and proved fatal by perforation of the stomach.

Mr Syme had several years ago been requested to tap a patient for ascites. Two days after the operation the patient died, and, on dissection, as in the case detailed by Dr Gairdner, a quantity of blood was found in the peritoneal cavity, and the serous membrane was seen studded with masses of encephaloid matter. The hemorrhage had not proceeded from any vessel wounded during the operation.

DISSECTING ANEURISM OF THE AORTA.

Dr W. T. Gairdner produced a specimen of dissecting aneurism of the aorta. The patient was stated by Mr Sidey, who saw him before death, to have been a man of fifty-four years of age, of intemperate habits, and subject to stomach complaints. Soon after a debauch he had a sudden fit of pain in the stomach, and was ordered to take some castor oil and laudanum. Pain and nausea continued, and when he was seen by Mr Sidey, the pulse was 76, full and soft, the action of the heart natural, but its sounds indistinct. He died suddenly a few hours afterwards.

On opening the body, the pericardium was found distended with about a pound and a-half of blood. A transverse slit was found in the aorta a short distance above its valves, extending almost round the vessel, and involving the inner coat, and inner layers of the middle coat. Blood had been extravasated in large quantity between the layers of the middle coat, constituting a dissecting aneurism, which had finally given way into the pericardium. There was little or no atheromatous degeneration of the coats of the artery. Aneurisms of this kind were rare ; and Dr Gairdner believed that only four specimens of the kind were to be met with in the museums of Edinburgh. There was reason to think that they were sometimes the consequence of external violence, particularly where, as in this case, the coats of the vessels were not materially diseased ; and, although in the case of Mr Sidey's patient, there was no evidence of a fall or blow, yet as he was known to have been intoxicated when his complaints began, there was reason to suspect that he might have suffered some external injury without being aware of it. Dr Gairdner referred, in corroboration of his views, to the case of dissecting aneurism published by Dr Peacock ; and to other cases recorded by Dr John Gairdner, Dr John Davy, and Casper of Berlin, in which external violence had caused ruptures of the heart or great vessels, in healthy subjects, and without any commensurate injury of the external parietes.

The President made some observations upon the effect of external violence in inducing aneurism in subjects whose arteries were not in a healthy condition. In the case of the late Mr Liston, it was believed that the aneurism of the aorta was connected with a blow from some part of the rigging of a yacht, received a few months before his death.

Mr Struthers adverted to the frequency of extravasation in the injection of subjects for the dissecting room.

Mr Goodsir believed that the arteries, if in a healthy condition, could sustain a very heavy pressure from within without rupture ; indeed one of their most important functions was to withstand such pressure. When the vessels of a fresh subject gave way beneath the force of the injecting syringe, it was usually found that the rupture was connected with atheromatous deposit. These ruptures were notoriously most frequent in the parts of the arterial system

most subject to atheroma; as, for instance, in the ascending aorta, and in the arteries of the abdominal extremities.

Dr W. T. Gairdner believed, with Mr Goodsir, that the arterial tunics were capable of withstanding an enormous statical pressure applied from within; but they might yield to a great and sudden collision, induced by violent exertion or by a fall.

Mr Goodsir mentioned, that the sudden death of the horse, which sometimes occurred when his strength was over-tasked, had been found to depend upon a transverse rupture of the aorta similar to that exhibited by Dr Gairdner's preparation.

The President had heard, that the hare, when hunted, sometimes died suddenly in consequence of rupture of the left ventricle of the heart, and that a preparation illustrating this fact might be seen in the museum of the Royal Medical Society.

RESTORATION OF THE UNDER LIP BY OPERATION.

Mr Syme exhibited a patient for whom he had recently constructed a new under lip, by the process described by him in the March (1847) number of the "Monthly Journal." Dr James Duncan had removed the old lip on account of disease. The patient, an elderly man, was subsequently annoyed not only by the frightful deformity which resulted, but by the pressure upon the ears and teeth, caused by a bandage which he was obliged to wear. The result of the operation for the formation of a new lip had been most satisfactory; there was hardly a trace of deformity, and the patient could use his mouth with freedom, and articulate distinctly. This was the second case of the kind on which Mr Syme had operated, and he believed that, if a third presented itself, he could still slightly improve upon the operation, by making the lip a little fuller.

IRREGULAR DISTRIBUTION OF THE ARTERY OF THE BULB.

Mr Spence exhibited a dissection of the pelvis of a boy, aged six, in which there was an irregularity in the distribution of the artery of the bulb on the left side. The left internal pudic was smaller than usual, and after giving off the inferior hemorrhoidal and superficial perineal branches, terminated by dividing into the artery of the crus and dorsal artery of the penis, but sent no branch to the bulb or to any part of the corpus spongiosum. The artery of the bulb arose from the anterior division of the internal iliac artery, passed along the side of the bladder, and on the upper surface of the prostate, above the ilio-vesical reflexion of the pelvic fascia. On arriving at the pubic arch, it pierced the fold of the fascia forming the anterior true ligament of the bladder, immediately to the left of the mesial line. It then altered its direction, and passed obliquely downwards between the two laminæ of the triangular ligament, towards the bulb; in this part of its course, it rested at first in immediate contact with the deep layer of the triangular ligament and perinæal surface of the prostate; crossed the course of the membranous portion of the urethra obliquely, until it passed posterior to it or lower in the perineum, lying in close contact with it; and then again bending forwards, it entered the bulb. The size of the vessel was nearly equal to that of the left pudic trunk in the same subject. Mr Spence considered this irregularity of interest, both on account of its rarity, and also on account of its surgical importance. It was the only case of the kind he had ever met with in his own dissections of the pelvis (amounting now to upwards of 180), and the only approach to it he had ever seen, was in a preparation preserved in the University museum, and described in Dr Monro's work on the pelvis. As regarded its relation to the incisions in the lateral operation of lithotomy, Mr S. remarked, that in the first part of its course, as in the more common variety of the dorsalis penis, or supplemental pudic, it lay above the ilio-vesical reflexion of the pelvic fascia, and therefore would at that point run no risk of injury if the prostate were divided obliquely

outwards and downwards so as to leave the reflected fascia entire; but in the perineal part of its course, from its close relation to the apex of the prostate, and the oblique direction in which it crossed and lay upon the membranous portion of the urethra, Mr Spence thought it could hardly have escaped division in the course of the operation of lithotomy.

PERFORATING ULCER OF THE STOMACH.

Dr Macaulay exhibited a specimen of perforating ulcer of the stomach. The patient from whom it was taken was a servant girl; while lifting some coals, she had been suddenly seized with pain in the abdomen, followed by vomiting, collapse, and all the usual symptoms of perforation. She died in sixteen hours after the attack of acute pain. In the stomach an ulcer was seen nearly cicatrised, and corresponding to a puckering of the peritoneal surface, another cicatrix probably marked the former existence of another ulcer; a third ulcer had caused fatal peritonitis by perforating the serous coat of the stomach.

ENLARGEMENT OF THE THYMUS GLAND, AND DISEASED SPLEEN, CONNECTED WITH EXTREME ANÆMIA.

Dr W. T. Gairdner showed a drawing of a very remarkable degree of enlargement of the thymus gland in an adult. The case was that of a man aged twenty, who was seized with abdominal pains some weeks before his death, and had been considerably depleted in consequence of profuse hemorrhage from leech-bites applied to the seat of the pain, as well as by some degree of hæmoptysis. Before death he became extremely anemic, and had some degree of anasarca, with *tinnitus aurium*, and imperfect vision. He did not appear, from the account of his relations, to have suffered from any long-standing affection.

The spleen was enlarged (weighed 25 oz.), adherent to the diaphragm, and contained light-coloured masses of softish exudation. The thymus gland was so much enlarged as to fill the entire mediastinum, resting on the diaphragm below, and passing into the neck above. The gland was eleven inches long, and, on an average, three or four inches broad; the microscope showed that the essential glandular elements were present in large quantity. There were numerous ecchymosed spots in the stomach and other internal organs, but none on the external surface.

This patient had some degree of the prominence of the eye-ball noticed by Dr Begbie in his late observations on the connection of anæmia with enlargement of the thyroid gland; and keeping in view Dr Begbie's case, as well as those lately alluded to by Dr Addison in the Medico-Chirurgical Society of London, in which a relation was pointed out between anæmia and disease of the supra-renal capsules, Dr Gairdner had examined these organs in the present case; but both the thyroid and supra-renal glands were normal. Dr G. had not been able to find any case similar to the present on record, nor had he ever witnessed any such enlargement of the thymus gland in an adult; but he mentioned that, during the late cholera epidemic, he had repeatedly observed the organ to be above the average development in the subjects of that disease.

ENCYSTED TUMOUR ENCLOSING BLOOD.

Dr Dunsmure exhibited an encysted tumour, the size of an orange, which he had lately excised from a weaver, in the surgical wards of the Royal Infirmary. The man suffered considerable inconvenience from the tumour, which was situate over the right *tuber ischii*, and had been present for upwards of two years. Latterly he had caused a hole to be cut in the bench on which he sat, and thus obtained relief from the pressure. In removing the cyst, the bursa covering the tuberosity was cut through; it adhered firmly to the upper surface of the cyst, from which, however, it was quite distinct.

The cyst was composed of condensed areolar tissue, of the density of fibro-cartilage. A careful examination of its internal surface with the aid of the

microscope, failed to discover any distinct epithelial lining. The contents consisted of a fluid, containing numerous blood-corpuscles and flocculi of partially coloured lymph, which adhered to the wall of the cyst, and had all the characters of the fibrine of the blood.

The President delivered a short address, and the Society then adjourned till November.

OBSERVATIONS ON THE EPIDEMIC CHOLERA OF 1848-9,

CHIEFLY AS IT PREVAILED IN THE 13TH MEDICAL DISTRICT OF THE CITY OF GLASGOW PARISH. BY DR JAMES MAXWELL ADAMS, SURGEON; MEMBER OF THE FACULTY OF PHYSICIANS AND SURGEONS, GLASGOW.

(*Continued from* p. 1020.)

THE subjoined tables contain the results, and an analytical view of the most important facts connected with all the cases of choleraic disease which came under my observation.

There are various points intended to be illustrated in these tables, and to several of these I will refer briefly in explanation, restricting my comments to those cases which I have termed cholera. I have first to remark that the mortality in private and in parochial practice was much alike. With reference to age, it will be seen that the mortality under ten years was 55½ per cent.; between ten and thirty it was 12½ per cent. only; between thirty and fifty it was 30 per cent; and above fifty years it was nearly 42 per cent. The number of females affected with cholera was absolutely greater than males, but the relative mortality was less than that of the males, being 46 per cent., and that of females 39 per cent.

By far the most severe cases were those in which there was no premonitory diarrhœa, by which I mean, that within a less period than five or six hours from the first loose stool, the evacuation lost all appearance of bile or feculent matter, and either vomiting or spasms supervened. The mortality in these cases was 50 per cent. Where premonitory bilious diarrhœa existed for five or six hours prior to the occurrence of serous stools, and of vomiting or spasms, the cases were of milder character, and the mortality was 31·3 per cent.

The previous general health and habits had, as might, *a priori*, have been expected, an important influence on the chances of recovery. Children under ten years cannot be said to have acquired habits; their early age is also of even more consequence than their previous general health. I have therefore entered their cases in the column headed "not known."

A section of the table shows the number, &c., of cases occurring in infected houses. I have entered the first case which happened in any tenement in the column headed "non-infected;" when a second occurred in the same building or house it is entered in the respective columns headed "infected."

In farther illustration of this last point, I have with considerable labour prepared a sufficiently accurate return of the number of houses, and the extent of population, comprised in those tenements in which any cases of choleraic disease occurred, and which came under my treatment in parochial practice. I have also ascertained the number of additional cases which occurred in the same tenements, and which either received no treatment, or did not come under my cognisance at the time. I exclude some isolated cases which happened in buildings inhabited by a mixed or superior class of population, and in which certain information could not be obtained.

Number of tenements in which choleraic disease appeared, .	45
Number of houses contained within the above tenements, . .	426
Population of the above houses, ...	2035

{ Of the houses above referred to, the disease was confined to . . 107
The population of the houses to which the disease was confined, was 483
The number of individuals actually affected was 213

From this it appears that in those buildings in which choleraic disease occurred, the number who were affected with illness amounted to ten per cent. of the entire population of the tenements, while the individuals who resided within the same houses with the sick, were affected in the proportion of forty-four per cent.

Taken in the mass, the circumstances and habits of these individuals were alike, and they all inhabited similar localities, and may therefore be considered to have been equally exposed to a general atmospheric or epidemic influence. It appears difficult in this view to account for the comparative immunity enjoyed by one section, and the large extent to which those individuals were affected who resided within the same house, or, as may be said, within the same four walls, which enclosed the sick. It certainly shows that the disease, when it does effect a lodgment, has a tendency to become intensely localised. But a large allowance must be made for the increased predisposition caused in the inmates of an apartment containing a sick person, and proceeding from inability to secure the customary attendance and comforts—want of their usual rest—depression of mind—apprehension, &c. &c.

This leads me to the subject of contagion of cholera, which, however, I have neither space nor inclination to review, and yet cannot pass altogether without notice. There certainly did occur under my observation a number of circumstances and of individual cases, to some of which I have already alluded, tending strongly to show that the affection is propagated by actual contact, and I think that *where the epidemic influence is present*, contact with the sick may prove a possible cause of determining an attack in an individual otherwise predisposed. But I am much more satisfied that the cases which I saw in the early period of the epidemic, and the large proportion of those occurring during its prevalence, could not be accounted for on the principle of contagion unless by stretching the arguments for that doctrine far beyond the limits of legitimate deduction.

With regard to the efficacy of preventive measures in the way of cleansing, &c., I can say little, having had imperfect opportunities for judging. Too many agents were concerned in these operations, clashing interests were affected, and views diametrically opposite as to the manner and extent in which the instructions of the Board of Health should be carried out, were held by the principal parties who directed these operations. The partial cleansing which was effected in the district under my charge, was carried on after the disease had attained its acme, and was followed by no marked results, as for every instance of exemption enjoyed by the inmates of a house which had undergone cleansing, there could, I have no doubt, be furnished a counter case. In only one instance was the experiment of cleansing, as a preventive, fairly tested, and I think the circumstance worthy of notice. At No. 15 College Street, there is a back tenement of four stories, having a building of equal height in front at a distance of about nine feet, a considerable part of the ground-story of this back tenement forms a common dungstead, and it is built up at the back by another tenement of nearly equal height, and with an interval of *only ten or twelve inches*. This latter building forms a part of the "Broad Close," viz., Nos. 4 and 6, and contains nearly the same number of houses and of inhabitants as the back tenement of 15 College Street, and the houses are tenanted by the same class of inmates. The entrance to both buildings is from different streets, and the inhabitants have no communication with each other, save in wordy warfare from the back windows. The building in 15 College Street is, therefore, as unfavourably situated, with regard to light and ventilation, as can well be imagined. The two tenements thus closely approximated, have hitherto been nurseries of disease. During the last six months of 1847, almost every house had two or more cases of typhus. I anticipated, therefore, a considerable amount of disease in this locality, and from the outset of the recent cholera

epidemic, I directed special attention to these buildings, and caused a house-to-house visitation to be made once or twice daily by my assistants. From first to last there occurred in 15 College Street, only two cases of choleraic disease, while in Nos. 4 and 6 Broad Close, there occurred fifteen cases, of which three proved fatal. It may be difficult to account for this unusual contrast, unless from the circumstance that the relative condition of the two tenements became altered a few months prior to the commencement of the epidemic. No. 15 College Street passed into the hands of a factor who, within a few months, caused all the houses and lobbies of the tenement to be whitewashed thoroughly several times, and by constant inspection enforced habits of cleanliness on the tenants. In Nos. 4 and 6 Broad Close, matters remained in their usual dirty condition.

A new element in the treatment of cholera consists in the house-to-house visitation, first suggested twenty years ago by Dr Kirk of Greenock, and adopted during the late epidemic under the orders of the General Board of Health. In the city parish of Glasgow it was only very partially carried out, and to that extent was productive of beneficial results. In reference to the effects in the district of which I had the superintendence, I may make a few extracts from a letter which I addressed to Dr Sutherland on the subject :—

"As regards the principle itself of the system of house-to-house visitation, I am satisfied that it is good, and that in no other way can a large proportion of cases, and especially of the incipient stages of cholera, be brought under notice or under medical treatment. * * * *

"Cases of cholera have been discovered which were fully developed in the symptoms, or were collapsed, or were actually dead, which had not undergone medical treatment, and which would not otherwise have been brought under medical notice ; and numerous cases of disorders of the general health, believed to be premonitory of cholera, have been brought under effective medical treatment.[1] * * * * * *

"I place little or no value on house-visitation, so far as relates to the more successful treatment of cholera. The chief value of the system I believe to be in the moral agency which is brought to bear on the ignorant and unreflecting—in the stimulating and teaching of the people—in the continuous iteration of advice and warning as to the subject of individual health, and the means of avoiding and preparing for the common danger. After this, which is the main result to be looked for, I place the very satisfactory control which medical aid, timeously applied, possesses in tying down, as it were, at a certain stage, the large majority of those cases of disease which prevail during a cholera epidemic, and which, however slight, must all, in some measure, be regarded as modified or undeveloped results of the general choleraic influence pervading the locality. On the whole, I am satisfied that, through the agency of house-visitation, many lives have been saved, and much aggravated disease and suffering obviated."

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[1] From careful inquiries I have ascertained, that a considerable number of cases of cholera occurred, which from first to last did not receive any medical treatment. In other cases the patients had received only a casual visit from some neighbouring practitioner. In one instance I entered a deserted house, and found, in a back apartment, the corpse of a female, only a few hours dead from cholera. She had not been seen by any medical man, and the other inmates, being filled with apprehension, had fled the house and abandoned her during life. In several instances the doors of houses were secured against the intrusion of myself and assistants, from an impression, prevalent among the poor, that the medical men were hired by the parochial boards with sinister intentions regarding the poor, and that they received a remuneration of so much for every *bona fide* death from cholera which they could report as having occurred in their hands. In the same houses which furnished the majority of cases treated by me there occurred twenty deaths, of which cases none were seen by me during life ; and at least in nine of these there was received no medical treatment whatever ; while most of the others were only partially brought under treatment.

No. II.

Table of Cases of Choleraic Disease, including Cases in Private Practice.

Divisions of Table.	Classification of Cases of Choleraic Disease.	Total Cases.	Total Males.	Total Females.	Under 10 Years.		10 & under 20		20 & under 30		30 & under 40		40 & under 50		50 & under 60		60 & under 70		70 & under 80	
					M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
A. Cases treated at the Houses of the Patients.	1. *a.* Bilious purging	76	35	41	7	7	6	5	11	11	6	10	3	4	1	4	1	...	...	...
	b. Bilious purging with vomiting or cramps	40	18	22	4	2	2	2	4	2	6	4	...	7	1	4	1	1	...	...
	c. Bilious purging, vomiting, and cramps	14	5	9	...	...	1	...	2	3	2	4	...	2	...	...	...	...	...	...
	2. Vomiting and cramps	10	4	6	...	...	...	...	...	3	2	2	...	...	1	...	1	1	...	...
	3. Serous purging	20	8	12	1	...	1	3	1	3	3	4	...	2	2	...	...	...	...	...
	4. *a.* Serous purging with vomiting	28	18	10	9	3	...	1	3	1	2	3	2	2	1	...	...	...	1	...
	b. Serous purging, vomiting, and cramps	65	23	42	2	2	2	3	3	11	6	12	5	9	3	2	2	3	...	...
	Total cases	253	111	142	23	14	12	14	25	33	26	40	10	26	9	10	5	5	1	...
	Deaths from class 6	7	6	1	6	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	Do. ... 7	27	13	14	2	1	...	1	...	2	3	6	5	2	2	...	1	2	...	...
B. Analysis of Class 4 treated at Home.	With premonitory bilious diarrhœa	69	29	40	8	4	2	2	4	9	6	12	5	9	2	2	2	2	...	...
	With no premonitory bilious diarrhœa	24	12	12	3	1	...	2	2	3	2	3	2	2	2	...	...	1	1	...
	Deaths with premonitory bilious diarrhœa	22	11	11	5	2	...	1	...	1	1	4	4	2	...	...	1	1	...	...
	Do. no do.	12	8	4	3	...	...	...	...	1	2	2	1	...	2	...	...	1	...	...
	Deaths in stage of collapse	19	8	11	3	1	...	...	...	2	2	5	2	1	1	...	...	2	...	...
	Do. after reaction	15	11	4	5	1	...	1	...	...	1	1	3	1	1	...	1	...	...	...
	Total cases	93	41	52	11	5	2	4	6	12	8	15	7	11	4	2	2	3	1	...
	Total deaths	34	19	15	8	2	...	1	...	2	3	6	5	2	2	...	1	2	...	...
C. Cases sent to Hospital.	Cases of cholera sent to hospital	17	6	11	...	...	1	2	2	5	1	1	...	1	1	2	...	...	1	...
	Died in hospital	13	4	9	...	...	...	2	1	3	1	1	...	1	1	2	...	...	1	...

No. III.

Table of Cases of Choleraic Disease, including Cases in Private Practice.

Divisions of Table.	Classification of Cases of Choleraic Disease.	Total Cases.	General Health.				General Habits.				Locality.						Stage of disease on admission.			Had previously.		Prominent Symptoms.			
			Good.	Bad.	Average.	Not known.	Temperate.	Intemperate.	Average.	Not known.	Infected House.	Infected Land.	Now infected Locality.	Ground Story.	Middle Story.	Top Story.	Active.	Collapse.	Reaction.	Premonitory diarrhœa.	No premonitory diarrhœa	Lividity of Surface.	Urine Suppressed.	Collapse.	Secondary Fever.
A. Cases treated at the houses of the patients.	1. *a.* Bilious purging,	76	28	8	33	7	30	15	6	25	29	22	25	17	39	20	76	..	..	..	..	..	..	1	7
	b. Bilious purging with vomiting or cramps,	40	9	13	10	8	14	11	5	10	12	17	11	10	18	12	37	1	2	..	..	..	2	9	8
	c. Bilious purging, vomiting, and cramps,	14	6	3	4	1	5	4	1	4	5	6	3	7	3	4	11	..	3	..	..	..	4	11	3
	2. Vomiting and cramps,	10	5	1	3	1	4	2	3	1	3	2	5	5	3	2	6	..	4	..	..	..	2	7	2
	3. Serous purging,	20	4	3	11	2	9	3	4	4	10	5	5	4	15	1	17	2	1	16	4	..	2	7	2
	4. *a.* Serous purging with vomiting,	28	9	4	3	12	11	3	3	11	10	8	10	7	11	10	19	5	4	22	6	6	18	28	7
	b. Serous Purging, vomiting, and cramps,	65	14	14	33	4	19	28	14	4	23	19	23	17	21	27	26	29	10	47	18	35	49	63	14
	Total cases,	253	75	46	97	35	92	66	35	59	92	79	82	67	110	76	192	37	24	85	28	41	77	126	43
	Deaths from class 4. *a.*	7	..	..	..	7	..	..	..	7	4	2	1	..	3	4	2	2	3	4	3	1	7	7	3
	Do. 4. *b.*	27	4	9	11	3	6	12	6	3	11	6	10	7	11	9	5	21	1	18	9	22	25	27	4
B. Analysis of class 4, treated at home.	With premonitory bilious diarrhœa,	69	15	15	27	12	22	22	14	11	22	22	25	19	22	28	38	24	7	69	..	33	49	67	18
	With no premonitory bilious diarrhœa,	24	8	3	9	4	8	9	3	4	11	5	8	5	10	9	7	10	7	..	24	10	17	24	3
	Deaths with premonitory bilious diarrhœa,	22	4	6	5	7	4	6	4	8	9	6	7	4	9	9	6	14	2	22	..	16	21	32	4
	Do. no do.,	12	..	3	6	3	1	6	2	3	5	2	5	3	5	4	..	10	2	..	12	8	11	12	3
	Deaths in stage of collapse,	19	3	6	6	4	3	8	4	4	7	4	8	5	8	6	4	15	..	12	7	6	19	19	..
	Do. after reaction,	15	1	3	5	6	2	4	2	7	7	4	4	2	6	7	2	9	4	10	5	8	13	15	7
	Total cases,	93	23	18	36	16	30	31	17	15	33	27	33	24	32	37	45	34	14	69	24	41	66	91	21
	Total deaths,	34	4	9	11	10	5	12	6	11	14	8	12	7	14	13	6	24	4	22	12	24	32	34	7
	Mortality per cent.,	36.5	17	50	30 5	62.5	16.6	38.7	35	73	42	29.6	36	29	43.7	35	13	70.5	28.5	31.8	50	58.5	48	37	33
C. Cases sent to hospital.	Cases of cholera sent to hospital,	17	10	4	1	2	6	6	2	3	5	4	8	4	7	6	..	..	..	10	7	..	..	..	..
	Died in hospital,	13	6	4	1	2	4	5	2	2	4	3	6	4	5	4	..	..	..	7	6	..	..	..	..

No. IV.

Table of Fatal Cases of Cholera, exclusive of Cases sent to Hospital.

Age.	Total Deaths.	Males.	Females.	Hours.						Days.																	
				6 to 12		12 to 18		18 to 24		1		2		3		4		5		6		7		8		above 9	
				M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F
Under 10 years	10	8	2	1	...	...	...	...	...	...	...	1	...	...	1	2	...	...	...	2	1	1	...	...	...	1	...
10 and under 20 years	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...
20 ... 30 years	2	...	2	...	...	...	...	...	...	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
30 ... 40 years	9	3	6	...	...	1	3	...	...	1	...	...	...	1	...	...	...	2	...	...	...	...	...	...	1	...	...
40 ... 50 years	7	5	2	...	...	...	...	...	...	1	...	1	...	...	...	1	1	...	...	1	...	...	...	1	...	...	...
50 ... 60 years	2	2	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	1	...
Above 60 years	3	1	2	...	...	...	2	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...
Total	34	18	15	1		6		1		3		3		3		5		4		3		2		1		2	

DEATH OF SIR CHARLES SCUDAMORE.

We have to announce the death of Sir Charles Scudamore, on the 4th instant, from the effects of chronic disease of the kidneys and heart. The following particulars in regard to this eminent physician have been published in the "Medical Times." He was born at Wye, in Kent, in 1779, and received his early education at Wye College, under the Rev. Philip Parsons, and there distinguished himself by his assiduity in the study of the Classics, English Literature, and the Belles Lettres. He afterwards commenced his medical studies, under the tuition of his father, a highly-esteemed medical practitioner at Wye, and an elegant scholar. Sir Charles's energies were, in due time, transferred to a more extensive field; and the area of the Borough Hospitals being the chosen theatre, and Cline, Saunders, Babington, Cooper, and other distinguished men, his teachers, his thirst for medical knowledge increased, and his acquirements became enlarged. After remaining three years at Guy's and St Thomas's Hospitals, where he gained the esteem of his teachers, he established himself at Highgate as a surgeon, which step he considered only as a preliminary one to the higher views on which, from the commencement of his medical studies, his mind had been fixed. Remaining a few years at Highgate, he fulfilled his desire of acquiring professional experience, and, proceeding to Edinburgh, devoted himself to his studies with intense ardour, and became noticed by the most eminent men there. At Glasgow, he took his degree, and wrote an elaborate Thesis on Gout, which he dedicated to Dr Babington, of London, and Dr Brown, the celebrated professor of Moral Philosophy in Edinburgh. Dr Scudamore then returned to London to practise as a physician, residing first in Holles-street, and shortly after in Wimpole-street, where he remained until his death. Soon after coming to London, he published an Analysis of the Mineral Waters of Tunbridge Wells, in which he was assisted by his friend, Mr John George Children; also, the first edition of his work on Gout, which he dedicated to Dr Matthew Baillie.

Among numerous works on different diseases, published by Sir Charles, may be mentioned, "A Treatise on the Nature and Cure of Rheumatism and Tic Douloureux;" "An Essay on the Blood," containing many original observations, and displaying much physiological and scientific research; "Observations on the Stethoscope and Percussion," published after studying the subject in Paris under Laennec; "An Analysis of all the important Mineral Springs in England;" "A Treatise on the Mineral Waters and Tepid Springs of Buxton;" in which last works he was assisted by Mr Garden (the well-known operative chemist of Oxford Street, London); and two short treatises, containing the result of

his extensive experience in gout, and on the use and abuse of colchicum in the treatment of that disease. His later works were on the use of the inhalation of iodine and conium in phthisis, and an account of a medical visit to Graefenberg, made in the year 1843, for the purpose of investigating the merits of the water-cure treatment. The last contribution to medical science published by him, in 1847, was a work on pulmonary consumption.

DEATH OF DR JOHN REID.

THE death of this eminent physiologist, on the 30th of July last, from the effects of the painful disease under which he so long laboured, has produced a general feeling of very deep regret throughout the medical circles of this country, but more especially in Scotland, where he was known as a most amiable and gifted man to a large circle of personal acquaintances. We have been unable, owing to unavoidable circumstances, to present our readers with a memoir this month, but it shall be given in our next number. We do not usually consider the pages of the "Monthly Journal" as open to poetical effusions, but we are nevertheless induced to insert the following lines, which we have received from a valued medical friend and correspondent, from the feeling that, in our pages, they will meet the eyes of a very large number of those who most deeply sympathise in the melancholy event to which they refer.

DEATH has at length released thee,
Thou brave and patient one!
The unutterable pangs are past,
And all thy work is done.

Thou wert a Daily Lesson
Of Courage, Hope, and Faith;
We wondered at thee living,
And envy thee thy death.

Thou hast gone up to Heaven
All glad and painless now,
The long-worn look of anguish
Has left thy noble brow.

Thou wert so meek and reverent,
So resolute of will,
So bold to bear the uttermost,
And yet so calm and still,—

We think of thee with sorrow,
Thy sad untimely end;
We speak of thee with pity,
Our sore-tried suffering friend.

We cheat ourselves with idle words,
We are the poor ones here;
Sorrow and Sin and Suffering still
Surround our steps with fear.

Our life is yet before us,—
The bitter cup of woe,
How deep it is, which each must drink,
No one of us doth know.

The Shadow of the Valley,
Whose gateway is the tomb,
Spreads backwards over all of us
Its curtain-cloud of gloom.

Some stand but at the inlet,
And some have passed within,
O'er all the shadow hourly creeps,
And we move further in.

Thou art beyond the shadow;
Why should we weep for thee?
That thou from Care, and Pain, and
Art set for ever free. [Death,

Well may we cease to sorrow;
Or, if we weep at all,
Not for thy fate, but for our own,
Our bitter tears should fall.

'Twere better still to follow on
The path that thou hast trod,
The path thy Saviour trod before,
That led thee up to God.

G. W.

DEATH OF ASTON KEY, ESQ., FROM CHOLERA.

WE have to announce the death of Mr Aston Key, Senior Surgeon to Guy's Hospital. Mr Key was seized with cholera at his residence, St Helen's Place, Bishopsgate, on Wednesday morning the 22d inst., at half-past eleven; and died

on Thursday at half-past seven A.M. He was in his usual good health on Wednesday morning; shortly after breakfast he appeared unwell; but, notwithstanding his indisposition, he saw and prescribed for his patients as usual, and in the evening was suffering severely from an attack of the prevailing epidemic, which terminated fatally on the following morning. Mr Charles Aston Key was well known to the profession as a scientific surgeon and successful practitioner. He was admitted a member of the Royal College of Surgeons on the 5th of January 1821, and on the 30th of July 1845, was elected by the "Fellows" a member of the Council of the College; he also held the appointment of Senior Surgeon to Guy's Hospital, and Surgeon in Ordinary to His Royal Highness Prince Albert. Mr Key was the author of several contributions to the advancement of surgical science.

APPOINTMENTS.

Dr Walshe has been appointed Professor of Practice of Medicine; Dr Parkes, of Clinical Medicine; Dr William Jenner, of Pathological Anatomy; and Dr W. B. Carpenter, of Medical Jurisprudence, in University College, London.

Dr Alexander Fleming has been appointed Professor of Materia Medica in Queen's College, Cork.

THE "MARCH OF INTELLECT" IN SURGERY.

It has at length been discovered in France that there is such a thing as a benign fungus of the testicle, an affection, it appears, "vaguement decrite par quelque chirurgiens de la Grande Bretagne" (!) but fully described, and illustrated with most incomprehensible drawings by M. Jarjavay in the "Gazette des Hôpitaux." It must be satisfactory to our readers to know that the first case of this affection recognized in France, was under M. Bonnet of Lyons, but since this, several other specimens have been procured, as a foundation for the very elaborate memoir of M. Jarjavay, in which he pronounces with great confidence upon all the methods of treatment, and comes at length to the conclusion that the true methods are cauterization or ablation if the fungus be small, and castration if it be large! We need not shock the nerves of our Edinburgh readers by going any further. How many mutilations of the last-mentioned kind may have been practised among our neighbours since Mr Lawrence's discovery rendered them totally unnecessary, it would be difficult to surmise. But the above paragraph must be read with a very edifying horror by those who are aware that, according to Mr Syme's method of reduction of the fungus, the removal of even the smallest portion of the gland, is, in most cases, unjustifiable.

TO THE EDITORS AND PUBLISHERS OF OUR EXCHANGE JOURNALS.

We have particularly to request that our Exchange Journals be in future addressed to the Editors of the "Monthly Journal of the Medical Sciences," care of Messrs Sutherland and Knox, 23, George Street, Edinburgh.

German Journals should be forwarded to the house of Hartmann in Leipsic, care of Messrs Williams and Norgate, Henrietta Street, Covent Garden, London.

French and Italian Journals should be forwarded to the care of J. B. Baillière, Rue de l'Ecole de Médecine, Paris.

American Journals should be forwarded to Mr Wiley's American Library Agency, Paternoster Row, London.

Attention to these directions will spare the Editors of the "Monthly Journal" much inconvenience, as well as the frequent loss of journals incorrectly addressed to them.

THE

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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On Typhoid and Typhus Fevers,—An Attempt to Determine the Question of their Identity or Non-Identity, by an Analysis of the Symptoms, and of the Appearances found after Death in Sixty-six Fatal Cases of Continued Fever, observed at the London Fever Hospital from January* 1847 *to February* 1849. By W. JENNER, M.D., Lond., Licentiate of the Royal College of Physicians, Professor of Pathological Anatomy in University College, London.—(*Continued from p.* 963.)

PHARYNX AND LARYNX.

I HAVE conjoined the appearances found after death in these two organs, because both were in numerous instances diseased in the same subject; and because the relation which the pharyngeal bears to the laryngeal affection is important in a practical as well as in a theoretically pathological view.

The term inflammation has been used, because the appearances found after death, and the signs and symptoms observed during life, so closely resemble, if they are not identical with those described by that term, that I cannot follow Rokitanski, and call these phenomena "diffused congestive typhous processes," especially as that author offers no proof that these local changes are peculiar to typhoid fever.

Typhoid Fever.—The larynx and pharynx were examined in fifteen of the twenty-three cases—they were both healthy in six, *i.e.*, more than one-third, or exactly in the proportion of 40 per cent.

Ulceration of the pharynx was found in four cases; in one of the

four there was an ulcer seated on the posterior aspect of the velum pendulum palati and uvula, by which the mucous membrane covering the latter, and a small part of the former was completely destroyed; the breadth of this ulcer was four lines; its edges were bright red, and not elevated; its floor, formed of submucous cellular tissue, was dusky red; the ulceration was quite superficial. In the same case there was a similar but smaller ulcer on the hard palate, and the arytæno-epiglottidean folds were considerably thickened, the chordæ vocales swollen, the mucous membrane of the larynx generally congested, and covered with muco-purulent fluid. Erysipelas, trifling in extent, had affected this patient a few days before death. In another of the four cases there were two or three small ulcers on the posterior wall of the pharynx. In the third an oval ulcer, half an inch in width, partially divided into two by a strip of apparently healthy mucous membrane, was found in the pharynx, just at its junction with the œsophagus; the floor of this ulcer was formed by the submucous cellular tissues of a yellowish colour, its edges, like the mucous membrane of the pharynx generally, were very pale; and there was in the same subject a second small superficial ulcer, seated a little below the one just described. In the fourth case four ulcers were seated on the posterior wall of the pharynx; one of the four ulcers was three quarters of an inch in width, irregular in shape, its edges slightly elevated, sharp, and of a dark purple colour, its floor formed of the congested naked fibres of the middle constrictor of the pharynx; two or three similar, but much smaller ulcers, were seated around the larger one; the whole mucous membrane of the pharynx was dusky red. In the three latter cases, the mucous membrane of the larynx was pale, and healthy in aspect; in three of the four cases, therefore, the larynx was healthy. In a fifth case there was ulceration and enlargement of both tonsils—increased vascularity of the pharynx, and redness of the upper and under surface of the epiglottis. In two cases there was distinct thickening (from submucous effusion of serosity) of the arytæno-epiglottidean folds and epiglottis; in one of the two there was also effusion of serosity beneath the whole mucous membrane of the pharynx and larynx—in both the serosity was colourless and transparent, and the mucous membrane itself of the larynx and pharynx pale. Both patients were suffering from erysipelas of the head and face at the time of their death.

In one case the pharynx was dusky violet, the upper and under surface of the epiglottis of a bright scarlet colour from minute injection; and there was a small ulcer on the corresponding parts of either chorda vocalis. The larynx appeared healthy in another case, in which there were numerous collections of a purulent-looking fluid, varying in size from a pin-head to a small sweet pea beneath, and elevating the mucous membrane of the pharynx, that membrane itself being pale. This patient suffered from erysipelas a few days before his death.

The conditions of the larynx and pharynx discovered after death in the fifteen cases, may be thus summed up :—

No vestige of old disease in any of the cases.

Both organs healthy in six, or, if the two cases be added in which the disease of the pharynx in the one, and of the larynx in the other, supervened after the termination of the fever, in eight cases, *i.e.*, in the proportion of 53 per cent.

Ulceration of the pharynx in one-third of the cases.

Ulceration of the larynx in one case.

Signs of inflammation of the pharynx in six cases; but in two of these the inflammation set in after the termination of the fever. As two of the cases of ulceration of the pharynx were unattended by any other sign of inflammation of that organ, the pharynx was diseased during the progress of the fever in eight cases.

Signs of inflammation of the larynx in five cases.

Larynx and pharynx diseased in the same subject (excluding one case in which erysipelatous inflammation had set in after the fever had terminated), in three cases, that is, in one-fifth of the cases in which the organ was examined.

Larynx healthy, pharynx diseased, in three cases, or one-fifth of the whole examined.

Larynx diseased, pharynx healthy, in one case; but in this case it was doubtful whether the disease was, during life, confined to the larynx, although no morbid appearance could be discovered in the pharynx after death.

From this analysis, I think it may fairly be concluded, that in these cases the laryngeal was secondary to the pharyngeal affection, and that *in typhoid fever laryngitis independent of pharyngitis is extremely infrequent.*

Typhus Fever.—The larynx and pharynx were both examined after death in twenty-six of the forty-three cases of typhus fever I am here analyzing.

Both organs were healthy in sixteen of the twenty-six cases. In one other case they were both healthy, with the exception of a little blood effused beneath the mucous membrane of the pharynx. In the remaining nine of the twenty-six cases, there were unequivocal traces of disease. The larynx and pharynx were deep purple, and covered with slimy mucus in one case. In four there were unequivocal traces of inflammation in both organs; thus, in one of the four the mucous membrane of the pharynx was deep red, of the larynx vivid scarlet, the redness on minute inspection being found to be punctiform; in another of the four the lining membrane of the pharynx was of a dirty yellowish colour, and so soft that it was removeable by the gentlest scraping; the chordæ vocales swollen, the rima glottidis a mere chink, the mucous membrane of the larynx generally vividly injected and covered with muco-purulent fluid, and on the chordæ vocales, and on the mucous membrane lining the larynx above the chords, were numerous shreds of white

opaque lymph-like matter, readily removeable. In the third case, the lining membrane of the pharynx was covered with thick mucus, felt rough, apparently from enlargement of its follicles, and was of a dull purple colour; there was a small ulcer on either chorda vocalis, but no other trace of inflammatory action within the larynx. The pharynx in the fourth case was studded with small yellowish spots, from which, on section of the mucous membrane covering them, a drop of purulent-looking fluid exuded. The lining membrane of the larynx in the same subject was dusky red, the chordæ vocales and arytæno-epiglottidean folds distinctly thickened from effusion of serosity into the submucous cellular tissue.

Old disease of the parts was present in two cases, the subjects being aged respectively fifty-six and forty-five; in one of the two the pharynx was congested; the mucous membrane of the larynx, covered with dirty, frothy mucus, was opaque and somewhat greenish in hue; the under surface of the epiglottis was irregularly thickened, so that it presented a mammillated appearance; just below the left chorda vocalis was a round aperture leading to a small cavity containing some sloughy matter; the base of the arytænoid cartilage ossified and necrosed was exposed. The other of the two cases terminated fatally on the 17th day of the disease. No history of the patient prior to admission could be obtained; after death, there was an old cicatrix on the posterior wall of the pharynx, and a small superficial ulcer on the same part; the mucous membrane of the pharynx and upper surface of the epiglottis was of a deep grey colour. Taking into account the cicatrix, the colour of the mucous membrane, and the duration of the fever, we cannot but conclude that the superficial ulcer was the consequence of chronic inflammation of the pharynx, which had existed prior to the attack of typhus. In one case in which the mucous membranes of the larynx and pharynx were pale and otherwise healthy, there was spread over the former some purulent fluid.

In one case only was there an ulcer in the pharynx, the result of recently established diseased action. It was small and superficial, and seated at the base of the left anterior pillar of the fauces. The mucous membrane of the pharynx was in this case of a deep red colour, and covered with finely granular, yellowish lymph. Similar granules of lymph covered both surfaces of the epiglottis. When the lymph was removed from the latter, the mucous membrane covering it was seen to be vivid scarlet; the lining membrane of the larynx generally was of a pale rose-red, the chordæ vocales slightly thickened. The subject of these serious lesions was attacked with phlebitis after he had completely recovered from a mild attack of typhus fever, and had even left his bed; it was during the second attack that hoarseness supervened. There had been no pharyngeal symptoms during the progress of the fever.

To sum up:—Of twenty-six cases in which the larynx and pharynx were examined after death, they were

Both healthy in seventeen cases, *i. e.*, in the proportion of 65·4 per cent.; or if the two cases be added in which the disease was evidently of old standing, and the one in which it supervened after the patient had recovered from the fever, twenty cases, *i. e.*, in the proportion of 77 per cent., passed through the disease without any affection of these organs.

In five cases there were signs of inflammation of both larynx and pharynx, *i. e.*, in about one-fifth of the cases. In one of the five ulceration of the larynx.

Inflammation of the larynx, the pharynx being free from lesion, in one case.

In no instance was the pharynx diseased and the larynx healthy, but in two of the five in which both were inflamed, the lesion of the pharynx was much more profound than that of the larynx.

Ulceration of the pharynx as a consequence of the fever was not detected in a single instance.

Thus it may be stated that if the laryngeal affection in typhus fever be not invariably preceded by pharyngeal disease, it is almost always so. The difference between the two diseases as exhibited by the great tendency to pharyngeal ulceration, in the cases of typhoid fever (ulceration of the pharynx occurred in one third of the cases) and by the total absence of any such lesion in the cases of typhus fever is remarkably striking.

The relation which existed between the laryngeal and pharyngeal affection and erysipelas was undoubtedly intimate.

ŒSOPHAGUS.

Typhoid Fever.—The œsophagus was examined in seventeen of the twenty-three cases. In sixteen it was healthy.[1] In one it was extensively ulcerated. The ulcerations, several in number, extended five and a-half inches upwards from just above the cardiac orifice of the stomach. The circular muscular fibres of the œsophagus were at places exposed. One of the ulcers was three inches in length, and half an inch in breadth. All were longer than they were broad. The edges of the majority were slightly elevated. The mucous membrane between was here and there finely injected.

Typhus Fever.—The œsophagus was examined in twenty-four of the forty-three cases. It was healthy in all.

Covering the œsophagus is frequently observed a thin layer of opaque white curdy matter. Microscopic examination has demonstrated to me that this layer is composed of epithelium. Whether it

[1] There is in the normal condition of the parts a marked difference in hue between the colour of the mucous membrane of the pharynx and œsophagus. The somewhat purplish colour of the former generally ceases abruptly on a line corresponding to the upper border of the larynx. I mention this because I have occasionally observed this appearance described by those unaccustomed to examine these organs, as a morbid appearance.

separates more readily in the one disease than in the other is worthy of investigation, especially with reference to a condition observed in the kidney tubes. My notes do not enable me to determine the comparative frequency of this probably cadaveric change in the two diseases I am here considering.

STOMACH.

Typhoid Fever.—Excluding the cases in which the stomach was not examined, and those which survived the thirty-fifth day of illness, there remain fifteen cases for analysis with reference to the condition of the stomach generally, and twenty with respect to the presence or absence of ulceration of that organ.

The cases that proved fatal after the 35th day are available so far as concerns ulceration of the organ, because unequivocal traces of that lesion are left in the form of cicatrices for some time after the healing process is completed.

If any appearance of healed ulcers in the stomach had been discovered in the cases that proved fatal after the termination of the fever, then a question might have been raised as to whether the cicatrices had existed prior to the commencement of the fever. But no trace of pre-existing ulceration was detected, therefore these cases may be admitted as unimpeachable evidence.

Colour.—No note of the colour was made in four cases. In these four it must have been healthy, or nearly so. In six cases it was pale. In one there was scattered over the whole mucous membrane some bright red lines and patches. In another there was punctiform redness, and softening of the mucous membrane to the extent of three inches, commencing from the pylorus, and stretching towards the cardiac extremity; it was in the same case mammillated to near the cardiac extremity, *i. e.*, commencing at the pylorus. The mucous membrane retained in this case its normal consistence at the cardiac extremity. There were a few rugæ.

In two cases the mucous membrane was somewhat grey. In one of these cases the posterior portion of the great cul-de-sac was speckled with deep red, the mucous membrane of the cardiac extremity was rather soft, the remainder of the organ normal in consistence and thickness. In the other of these two cases the mucous membrane was rugose and mammillated with the exception of the great cul-de-sac, generally softened, the softening especially affecting the pyloric half. In two other cases there was marked increase in the vascularity of the cardiac extremity of the stomach.

Ulceration.—In one case only was there recent ulceration. In this case the mucous membrane of the stomach was pale and smooth, its consistence and thickness normal. Scattered over the anterior and posterior wall and greater curvature were numerous minute ulcers, varying in size from a pin-point to a line in diameter; their edges and floors were pale; the former rounded, the latter formed of submucous cellular tissue.

In no case was there any trace of old ulceration, or of chronic inflammation[1] of the organ.

Softening.—In ten cases the mucous membrane retained its normal consistence. In two it was slightly softened at the cardiac extremity. In three near the pylorus; in one of these three there were numerous red spots on the posterior wall of the stomach, varying in size from a mere point to a pin-head. The softening in this case was limited to the red spots and their immediate vicinity, and in another of the three cases the softening of the pyloric portion of the stomach was accompanied by punctiform redness.

In no instance was any softening approaching perforation observed. In no instance was the mucous membrane firmer than natural; in no instance softened throughout the organ.

Thickness.—In one case the mucous membrane was noted to be generally somewhat thickened. In six cases it was recorded to be of normal thickness.

Mammillation.—In two cases there was no mammillation; in one nearly the whole membrane was in that state; in two cases it existed from the pylorus to near the cardiac extremity; in three it was limited to the vicinity of the pylorus; while in five of the fifteen cases no note of this particular was made.

Typhus Fever.—Omitting the cases of which no notes were made with respect to the condition of the stomach, and those which survived the 4th week of disease, there remain for analysis generally thirty-seven cases, and with respect to ulceration forty-two.

Colour.—The stomach was noted to be pale or healthy in colour in twenty-three of the thirty-seven cases. Of an uniform dusky grey hue in two cases. There was punctiform redness along the greater curvature, and anterior and posterior surfaces of the mucous membrane adjoining, in one case. Minute hemorrhagic spots in the cardiac extremity in two cases. Some redness of the great cul-de-sac in one case. The mucous membrane in the four last-mentioned cases retained its normal colour in the other parts of the organ.

There were signs of chronic inflammation of the mucous membrane of the stomach in seven cases, *i. e.*, it was grey, speckled with red, thickened, altered in consistence, or covered with tough mucus, and in four of the seven were unequivocal cicatrices.

Ulceration.—In one case the mucous membrane of the whole stomach was remarkably rugose, mammillated from pylorus to cardia, and of a dull roseate hue; in the great cul-de-sac the redness was remarkably vivid, and on minute inspection was found to be punctiform. Three inches from the pylorus, scattered over a space about an inch and half in circumference, and seated on the posterior wall of the stomach, were nine ulcers, varying in size from a pin-point to

[1] I do not include mammillation of the mucous membrane of the stomach under the term of chronic inflammation of that organ. The true signification of this condition of the membrane, it appears to me, has yet to be determined.

No. 4 shot; their edges were well defined and not discoloured; there was no softening of the mucous membrane; on the contrary it was noted rather tough.

In no other case was there evidence of recent ulceration, for it so happened, as has been before remarked, that the old cicatrices were observed only in those cases which proved fatal during the progress of the fever. The ages respectively of the subjects in which the cicatrices were noted were 33, 40, 42, 56, and 61 years.

Mammillation.—The presence or absence of this condition of the mucous membrane was recorded in fourteen cases only. In one, the case before referred to, it was general. In six, it was limited to the vicinity of the pylorus. In seven cases there was no mammillation.

Consistence.—The mucous membrane was noted to be of normal, or nearly normal, consistence, in twenty-two cases. It was generally rather firmer than natural in four. In one of these four there were recent ulcers; in two others cicatrices of old ulcers; in one case, in which the mucous membrane was of a reddish hue posteriorly, of a darker red anteriorly, and of a vermillion tint along the great curvature, the redness being punctiform and capillary. The consistence, which was generally natural, was, at the reddest parts, slightly increased; the same parts were also thicker than natural. No note was made with respect to the consistence of the mucous membrane in one case. In four, it was generally softer than natural; in one of these four the cardiac extremity was disproportionately soft. In four cases, there was such extreme softening of the great cul-de-sac, that it ruptured in the removal, or in the washing of the organ. The ages of these four subjects were 33, 49, 50, and 50. Two of these cases terminated respectively on the 12th and 16th day of disease; the other two during the course of the fever, but the exact date was never ascertained.[1] In no case was the mucous membrane in the vicinity of the pylorus softer than that covering the cardiac extremity.

Thickness.—In three cases the mucous membrane was noted to be generally increased in thickness. In one case (before referred to) it was partially thickened; it twenty-one cases it was noted to possess its normal thickness.

DUODENUM.

Typhoid Fever.—Excluding eight cases, which either survived the fever, or were not examined with reference to the condition of the duodenum, there remain for analysis fifteen cases. There was no marked deviation of the duodenum from health, in ten of the fifteen. In one, its mucous membrane was finely injected, and in another it

[1] I shall reserve, till another opportunity, the minute analysis of these cases of perforation; their number is too few to lead to any satisfactory conclusion; and I have accounts of others, which may be added to these, though not admissible in this place, because they do not come within the limits I have laid down for the cases here analysed.

was reddish-grey; in both of these cases there was redness and softening of the pyloric portion of the stomach. In one case there was vivid redness of the upper part of the duodenum, the stomach being perfectly healthy in appearance. In two cases the mucous membrane of the duodenum was pale grey; in one of the two the stomach was greyish, and in the other there was considerable redness, in patches and lines, of the latter organ. In no case was there any trace of ulceration.

Typhus Fever.—Six cases being omitted as not available for analysis, except in so far as traces of ulceration of the organ are concerned, there remain for examination thirty-seven of the forty-three cases here considered.

In twenty-eight the mucous membrane appeared perfectly healthy. In two it was stained yellow, and slightly softened; in one of these two the mucous membrane of the stomach was in the same state. In one case, the mucous membrane of the duodenum was vividly injected; in five others it was of a grey colour. In one of these five Brunner's glands were remarkably prominent. Signs of chronic inflammation of the mucous membrane of the stomach existed in four of the six cases last referred to, old cicatrices having been noted in that organ in four of them; and in the other two of the six the mucous membrane of the stomach was pale grey, though otherwise healthy in appearance.[1]

In one case there were some hemorrhagic spots beneath the mucous membrane, on the edges of the valvulæ conniventes, the intervening membrane being pale and healthy.

In no case was there recent ulceration, and in no case traces, in the form of cicatrices, of healed ulcers.

From this analysis, I think the conclusions may legitimately be deduced, that the duodenum is rarely affected in fever, and that it is not common for it to be the seat of chronic inflammation, unless in conjunction, and probably secondarily to the same affection of the mucous membrane of the stomach.

JEJUNUM AND ILEUM.

Typhoid Fever.—Seventeen cases proved fatal before the thirty-fifth day of the disease; only these seventeen, therefore, can be examined with reference to the colour and consistence of the mucous membrane; but all the twenty-three are evidence with respect to the presence or absence of ulceration.

Colour.—The mucous membrane generally was pale or healthy in

[1] As some persons have spoken of the solitary glands at the lower part of the ileum as Brunner's glands, I think it well to state, that I intend, here and elsewhere, to limit the term to those glands described by Brunner as existing in the duodenum. They are well known to be very different in structure and situation from the solitary and agminated glands found in the ileum.

eleven of the seventeen cases. The whole membrane was finely injected in one case; in another there was ramiform injection in patches; in two others the lower part of the ileum, in one two feet, in the other five inches, upward from the ileo-cœcal valve, was finely injected; in this latter case the jejunum was, like the duodenum, greyish, while the upper part of the ileum was pale. In another case the jejunum and upper part of the ileum was ash grey, the lower part of the ileum pale. In one case two feet of the ileum were of a grey colour; this case proved fatal on the 25th day of the disease.

Consistence.—Notes on this point were made in fourteen cases only; in eleven of the fourteen it was natural, in three softer than normal. In one of the three cases the softening was confined to the lower two feet of the ileum; in the other it was general, but slight in amount; in all three the mucous membrane was pale, and of normal thickness.

Agminated Glands or Peyer's Patches.—These organs, the functions of which are entirely unknown, but whose structure Boehm has so well described, were ulcerated in the twenty-three cases I am here considering; and as the proof that the lesions of these patches observed in England are the same as those observed in France is of importance for establishing the identity of the cases I have here called typhoid fever with those described by Louis and Chomel under the same name, I must enter a little fully into the description of the changes these patches had undergone in the twenty-three cases here analyzed.

In one of the cases which proved fatal on the *twelfth day, i. e. the earliest* period of the disease at which either of the twenty-three patients died, the following lesions were noted to exist in the ileum. About seven feet above the cœcum, the solitary glands were very distinct, in the form of raised opaque white spots. Six feet above the ileo-cœcal valve, was one of Peyer's patches, the *inferior portion* of which was thickened, and of a pinkish hue; from thence downward every patch was thickened; the amount of the thickness and the superficial extent to which each patch was affected, increased as they approached the ileo-cœcal valve. On one patch, situated about three feet from that valve, the mucous membrane was destroyed at several spots, by as many minute ulcers, each about the size of a pin's head; from this point downward every patch was more or less ulcerated, thickened, and distinctly, and in some places considerably, raised above the level of the surrounding mucous membrane. The lowest elliptical patch was three inches in length, thicker than any above it, covered with thick rugose mucous membrane. On it were several ulcers, the largest irregular in shape, about three lines in diameter, the smallest not bigger than a pin's head. The floors of the ulcers were formed of submucous cellular tissue of a deep yellow colour. The mucous membrane on the diseased patches was of a pale purplish colour, and softened; the minute pits observable na-

turally on the patches, were much more apparent than in the healthy state. The thickening of the patches appeared due to an increase in the thickness of the mucous membrane, and of the submucous cellular tissue, and not to any deposit in the latter. The size and thickness of the solitary glands increased as they approached the ileo-cœcal valve; near this part some were as large as split peas. On the apices of several was a small opaque yellow spot, and on others a minute black point. The mucous membrane between the glands at the lower eighteen inches of the ileum was highly vascular; but neither softened nor altered from its normal thickness. The peritoneum immediately over the diseased patches was of a palish purple colour, finely injected, the vessels running in two directions longitudinally, and transversely, *i. e.* crossing each other at right angles.

The following is the description of the condition of the last three-and-a-half yards of the ileum in a man who died of hemorrhage from the intestines on the *twenty-fifth day* of the disease.

About three-and-a-half yards above the ileo-cœcal valve was one of Peyer's patches, opaque and thickened at intervals; the opacity and the thickening were seen, on section through the substance of the patch, to be due to a white deposit, seated in the submucous tissue. The mucous membrane covering the patch was pale, and of normal consistence. On the next patch were two thickened spots, each three-eighths of an inch in diameter. The deposit in the submucous tissue which caused the thickening was of a pink colour; the mucous membrane covering this patch was pale and softened.

Every patch from the last described, to about half a yard above the ileo-cœcal valve, was more or less thickened. The thickening did not, in the majority of cases, affect the whole gland, but was found in spots only; the portion of the patch between the spots appeared tolerably healthy. Where, however, the whole gland was slightly thickened, then some spots were much thicker than the general mass of the patch; the parts most affected measured about a line and a half in thickness, and had a pink colour; the thickening being evidently due to a pale friable deposit in the submucous cellular tissue. The mucous membrane covering the whole gland was softer than that adjacent to it; and that over the pink and thickest spots, softer than that over the pale and thinner portion of the patch. The solitary glands, at this part of the intestine, were, generally, scarcely visible; but here and there were a few opaque, and slightly elevated, and two were as large as split peas. The mucous membrane over the two was slightly softened, and decidedly softer at their apices than at their bases. The submucous tissue was of deep red at these spots. About half a yard above the ileo-cœcal valve was a Peyer's patch, much more thickened than those above described. The mucous membrane covering it was of pulpy softness. At its superior part was a round spot with red edges, the centre of which was occupied by an opaque yellow structureless mass, readily detached; by its removal, a thin layer of sub-

mucous cellular tissue was exposed. On the lower part of the patch was a spot about one inch in diameter, thicker than the other parts of the gland, of a red colour, its centre occupied by an ulcer three lines in diameter. The floor of this ulcer was formed of the transverse muscular fibres of the intestine; to its edges, which were jagged, and overlapped the floor, were attached shreds of tough, opaque, deep yellow, sloughy looking matter, similar to that observed at the upper part of the patch, and described above as structureless, *i. e.* to the unassisted eye. Passing downwards there was, a little nearer to the cœcum, another patch of a deep grey colour, having on it an ulcer resembling the last described, but larger and deeper; attached to its edges were, as in the last, opaque yellow sloughs. Water thrown into the superior mesenteric artery welled forth freely from the edges of this ulcer; doubtless from it, therefore, the hemorrhage of which the patient died proceeded.

Every patch from this point to the ileo-cœcal valve was more or less destroyed by ulcers resembling the last described. The solitary glands in this part of the ileum were larger than those in the upper portion.

The patient whose intestine presented the appearances I am now about to describe, died on *the forty-sixth day* of illness from perforation. The appearances illustrate well the process of healing of the ulcers, and also the chronic-atonic ulcers, which often continue long after the fever has run its course, and frequently prove fatal by perforation. I cannot refrain from pointing out to the reader the importance of the knowledge of the existence of these atonic ulcers in a practical point of view, as influencing, that is, the treatment and the prognosis. The lower part of the jejunum and the upper part of the ileum, in this case, had a peculiar opaque appearance; their consistence and thickness were normal. About three feet above the ileo-cœcal valve, at the inferior portion of one of Peyer's patches, was a grey spot, half an inch in diameter; the centre of this spot was pale, smooth, shining, and slightly depressed below the level of the surrounding tissue. There was no puckering around it. This mode of healing of typhoid ulceration of the gut explains the fact, that diminution of the calibre of the intestine never follows the healing of this form of ulcer, as pointed out by Rokitanski. It appeared to be the remains of an ulcer. From this spot downwards, to about eight inches above the cœcum, every patch was more or less diseased, *i. e.*, of a deep grey colour; and on every one were healed, or apparently healing, ulcers. In neither of these healing ulcers was there any free fringe of mucous membrane. Between the patches were some small round superficial ulcers, the floors of which were pale, the edges grey, but not elevated. Nine inches from the ileo-cœcal valve, all the coats of the intestine were perforated, and an aperture from the interior of the bowel into the peritoneal cavity was formed, three-eighths of an inch in diameter. The edges of the aperture were well defined; the mucous membrane around it pale, and slightly softened. Through this opening the contents of the bowel had escaped before death.

The whole of the coats of the gut had been perforated, during life, through the next Peyer's patch, but the fæcal matter had been prevented from escaping through the aperture, in consequence of the peritoneal surface around the orifice having adhered, by dense false membrane, to the fundus of the uterus. The edges of the opening, like the remainder of the patch, were grey, and not thickened. On the same patch were two ulcers, the size of the aperture last described; their floors were pale, and formed by the transverse muscular fibres of the gut; their edges scarcely, if at all, elevated, and, like the whole patch, deep grey. On the patch, immediately above the valve, were several small superficial ulcers, and some smooth, pale, shining, and slightly depressed spots, apparently healed ulcers. Between the three last described patches were many small round grey spots. On these there were no traces of previous ulceration; they appeared to have undergone resolution without proceeding to the stage of ulceration. They were scarcely thickened, and the grey colour assumed a deeper tint when the surface of the mucous membrane was removed by gentle friction. This latter is a very important fact with reference to the question, whether the change of colour was cadaveric, or the result of diseased action. The whole mucous membrane of the lower portion of the ileum was pale, rather soft, somewhat opaque, but of normal thickness.

In the following account of the appearances found in the ileum after death, on the 28th day of the disease, are detailed the conditions which precede one mode of perforation, viz., that by *rupture* of the peritoneal coat. The upper part of the ileum was of a pale ash grey, normal in consistence and thickness. About four-and-a-half feet above the ileo-cœcal valve was one of Peyer's patches, slightly thickened, the mucous membrane covering it softened, and very vascular. The next patch was similarly affected. On the next, *i. e.*, descending toward the valve, was a large ulcer, by which the coats of the intestine had been so much thinned that an aperture was formed in washing them. From this point downward every agminated gland was more or less destroyed by ulceration. On the floors of some of the ulcers were large sloughs of a yellow colour, soft, but moderately tough; around the edges of others were small masses of pale yellowish deposit, friable, and of cheesy consistence. The floors of the ulcers generally were formed by transverse muscular fibres. From the centre of some of them, however, the muscular fibres had disappeared, *and a delicate layer of peritoneum only separated the contents of the intestine from the abdominal cavity*. The edges of the ulcers were elevated, and of a grey or slate colour. The mucous membrane, between the agminated glands, was rather pale, and slightly softer than natural. The peritoneum covering the ulcerated patches was more vascular than elsewhere; and on that membrane, corresponding to the floor of some of the ulcers, were some shreds of recent lymph.

I have detailed in the above four cases two forms of thickening of

the agminated glands. One, in which the thickening depended on the deposit of a pale whitish friable matter, of cheesy consistence, *in the substance* of the submucous cellular tissue—the *plaques dures* of Louis; and another, in which the thickening was due to swelling of the mucous and submucous tissues,—the *plaques molles* of the same author. Ulceration follows both forms of thickening.

These cases farther illustrate the following facts:—

1*st*, That ulceration of the solitary and agminated glands may commence in two modes; on the one hand, by softening of the mucous membrane, abrasion of the extremely softened superficial tissue, and then enlargement of the breach of continuity thus formed, in depth and extent, by simple ulceration; on the other, by sloughing of a portion of the submucous tissue containing the before-described deposit, and of the mucous membrane over it, and then extension of the ulcer in breadth and width, by the separation of minute sloughs from the edges of the breach of continuity, left after the separation of the slough first formed.

2*d*, That when the whole of the deposit has sloughed out, no fresh deposit is formed; and that, consequently, as the whole of that deposit is seated in the submucous tissue, destruction of the muscular fibres of the intestine must be the result of simple ulceration.

3*d*, That resolution of the disease affecting the patches may in some cases occur before ulceration has taken place.

4*th*, That ulcers of considerable size may heal.

5*th*, That no contraction follows, within a short period, the healing of the ulcers.

6*th*, That ulcers, dependent for their origin on the presence in the system of the fever-poison, may, after the fever has run its course, continue to spread, retard recovery, and even cause death by perforation.

7*th*, That while some of the ulcers are undergoing the healing process, others may be spreading; or, as Rokitanski says, may pass into the state of atonic ulcers.

These atonic or simple ulcers, left after the termination of the fever, are a frequent cause of lengthened duration of illness in cases of typhoid fever.

Four cases only of twenty-three offered examples of the plaques dures.

In no case that proved fatal after the 30th day was any of the whitish deposit discovered in the submucous cellular tissue; and this agrees with the experience of Louis, for the thirteen examples of the plaques dures detailed in his great work on Typhoid Fever, proved fatal before the 30th day of the disease. I doubt, however, if it be correct to regard all the cases which proved fatal at a later period as examples of the plaques molles, because it is probable that the deposit constituting the plaques dures takes place at an early period of the disease.—(I have seen it very extensive in the intestine of a girl who died four days after she had been engaged at the wash-tub, at which

time she felt quite well. I am indebted for the history of the case to my friend Mr Sankey. I saw the girl the last day of life, and was present at the examination after death.)—And it is farther probable, that before the 30th day the whole of this foreign matter, incapable of more than the lowest cell-development, would be thrown off by sloughing.

This view of the matter receives confirmation from the condition of the mesenteric glands in many cases.

From these organs the typhous matter, as it has been called, is not thrown off, as from the agminated glands, by sloughing. In them, accordingly, we find this deposit long after the 30th day. In one of the cases above described, which proved fatal on the 28th day of the fever, had the patient survived another day or two, the few pieces of yellowish cheesy matter which still adhered to the sides of the ulcers would have been thrown off, and there would have remained no proof, that the case had been an example of the plaques dures.

The ulcers, as a general rule, increased in superficial extent as they approached the ileo-cœcal valve. When one portion of a patch was thicker than another, the portion of the patch next the ileo-cœcal valve was usually the thicker.

Healing ulcers were found in every case that proved fatal after the 30th day, and also in one case that *probably* had been ill only twenty days when the fatal termination took place. No effort at separation was discovered in the other cases that proved fatal before the 30th day. Another inducement to believe that the attempt I made to fix the duration of the disease from the duration of the eruption, at the commencement of this paper, was successful.

Perforation.—Three patients, or rather less than one-eighth of the patients whose cases are here analyzed, died from perforation of the intestine, respectively on the 17th, 31st, and 42d day of disease. The perforation in all three took place through the floor of an ulcer seated on one of the agminated glands. In two of the three, perforation occurred in the lower nine inches of the ileum; in one, three feet above the ileo-cœcal valve. In two of the three the coats of the intestine were destroyed through their whole thickness at another spot from that at which the perforation, which proved fatal, took place; but the contents of the bowel had been prevented escaping through the aperture first formed by adhesions, in the one case, to the fundus of the uterus, and in the other, to a fold of the intestine. It will be observed, that in one of the three cases certainly the fatal perforation took place after the termination of the fever.

This fact, as I have before stated, is of the highest practical importance. It is not to be supposed that a case continues to be typhoid fever so long as ulceration, even active ulceration, of the agminated glands exists. The fever, the true fever, runs its course, but having excited, during that course, ulceration or inflammation of any organ or set of organs, that ulceration, or that inflammation, continues to

its termination irrespective of the duration of the specific disease. This appears to me to be the key to much of the difficulty surrounding the study of the duration of continued fevers.

Solitary Glands.—These bodies were visible in six cases, enlarged slightly in three, ulcerated in six, no note of their condition was made in eight cases, so that in these eight they could have deviated little, if at all, from a healthy appearance, *i.e.* could have been scarcely, or not at all, visible. Four of these eight survived the fever. They were generally more diseased in the vicinity of the cœcum than higher up the intestine.

When ulcerated, the breach in the continuity of the mucous membrane over them appeared to take place, either by the formation of minute sloughs on their apices, which, when thrown off, left small ulcers, or by softening of the mucous membrane covering their most prominent part.

Typhus Fever.—Colour.—The mucous membrane of the jejunum and ileum was healthy in colour in thirty-four of the thirty-nine cases of which notes on this point were made; in four, however, of the thirty-four, there was some ramiform injection of the larger vessels, at intervals apparently due to position.

In two cases hemorrhagic spots existed beneath the mucous membrane; in one of the two, these spots, about twenty in number, were scattered over the upper eighteen inches of the jejunum; their size varied from a small pin's head to a line and a half in diameter; the largest were of a deep purple colour, the smaller red. All were hard to the touch, and distinctly elevated. When firmly pressed, their contents, which was fluid, spread irregularly beneath the surrounding mucous membrane. Apparently entering into several of these spots, small vessels, filled with fluid blood, could be traced. The mucous membrane between was pale and of normal consistence; in the other of these two cases, the mucous membrane of the ileum from four feet above the ileo-cœcal valve to nine inches from the same spot was finely injected. Some of the larger veins were filled with dark blood, and in their vicinity were small spots of ecchymosis. The mucous membrane itself was rather thin and soft. In one case the jejunum was finely injected, while the ileum was pale; in another, both divisions of the intestine were deep grey; and in another case, the fine injection was limited to the lower part of the ileum.

Consistence.—In twenty-nine cases the mucous membrane was of normal, or nearly normal, consistence; in six, it was generally slightly softened; in one case before referred to, the softening was confined to the lower part of the ileum, the same part being finely injected. The upper part of the jejunum was rather soft in a case in which the mucous membrane of the ileum was normal in consistence. In one case only was there any notable thinning of the mucous membrane, and in that case it was conjoined with softening and redness.

Peyer's Patches, or the Agminated Glands.—With three exceptions, these organs were perfectly healthy, *i.e.*, neither elevated, reddened, softened, nor ulcerated. In fourteen cases they were scarcely visi-

ble; in sixteen they were noted to be visible, and normal in appearance. I have not considered that appearance, likened by French pathologists to the newly shaven beard, as a deviation from health. This condition was observed in four of the above cases. In the other ten instances, the whole intestinal canal was carefully examined, and recorded as *perfectly healthy*, but no note was made as to whether the patches were visible.

The following are the particulars of the appearances presented by the three cases in which the patches deviated from their normal condition. In one case, the patient, a child æt. ten years, died on the forty-fourth day of illness, and the thirty-first of her residence in the hospital. There was found, after death, extensive tubercular deposit in both lungs, and a large gangrenous abscess in the left. About eighteen inches from the commencement of the jejunum, was a Peyer's patch, beneath the mucous membrane of which, *in distinct points*, was some yellow opaque matter; the tissue of the gland between these *points* was healthy in all particulars. The next three or four patches resembled the last described. Then followed six perfectly healthy; then one resembling the first, after which every patch was normal in all respects. The mesenteric glands were enlarged, especially about the centre of the mesentery. On section, some of them were found studded with yellowish white masses, about the size of pins' heads.

This was evidently a case of tubercular disease of the agminated and mesenteric glands. These organs bore *no* resemblance to the same parts taken from a patient who had died at the same period, after recovering from typhoid fever. The situation of the diseased patches and mesenteric glands, the form and nature of the foreign matter deposited in them, and finally, the fact of the whitish matter of the plaques dures, with which alone it could by any possibility have been confounded, never having been seen in the agminated glands after the 30th day, while this patient died on the 44th day of the illness,—all unequivocally prove that this was not a case of typhoid fever; while the general appearance, and the situation of the deposit, and the existence of tubercle in the lung, as undoubtedly prove that it was an example of abdominal tubercular disease.

In the second case, which proved fatal during the fourth week of the disease, the lower eighteen inches of the ileum were intensely conjested—the large veins distended with dark blood—the mucous membrane of a dusky vermillion hue, the bodies of Peyer's patches slightly thickened, and of a deeper red than the surrounding membrane. At the same part the mucous membrane was somewhat softened, but not more so on the patches than around them. Some of the mesenteric glands in this case were visible, but not enlarged; the mucous membrane of the cœcum, and ascending colon near to the cœcum, was highly vascular and softened—the under surface of the ileo-cœcal valve an uniform dusky purple red. This case, then, was one of

dysentery, in which the inflammation extended somewhat higher up the ileum than usual, and involved the mucous membrane covering the elliptic patches, *in common* with that around them. The inflammatory action probably commenced at the under surface of the ileo-cœcal valve ; at any rate, that spot deviated the most markedly from a healthy appearance. The third case was that of a woman aged thirty-nine years, who died on the 19th day of the disease. The mucous membrane of the jejunum and ileum was tinged with bile ; the larger vessels were considerably congested. The thirteenth Peyer's patch, counting from the cœcum, was finely injected. The other patches were healthy, with the exception of the last and the last but one, on either of which was a very slightly thickened spot, of a greyish colour, about two lines in diameter, with a minute depression in the centre. The mesenteric glands were visible, but not enlarged ; the mucous membrane over the patches was of normal consistence and thickness, the deviation from health was so trifling, that a friend present, well acquainted with the normal and diseased condition of these organs, considered them healthy ; and I really cannot say, the thickening was so trifling, and the discoloration so pale, that there was any actual disease, but as I noted these appearances at the time, and there was a question whether there was a loss of mucous membrane at the minute depressed points, I have thought it well to give the particulars.

Thus, it may be said, that recent disease of Peyer's patches was absent in every one of the forty-three cases of typhus fever.

Solitary Glands.—There was no deviation from the healthy condition of these organs in any of the cases of typhus fever.

(*To be continued.*)

ARTICLE II.—*On Anormal Nutrition in Articular Cartilages.* By P. REDFERN, M.D., London, Lecturer on Anatomy and Physiology, and on Histology, at the University and King's College, Aberdeen. (*Continued from p.* 1075.)

OBSERVATION IX.—*Disease of nearly the whole Articular Cartilages in the Body, with Porcellanous Formations in a few spots ; Fracture of the Neck of the left Femur eighteen months before Death ; excessive Softening of the Bones of the left Lower Extremity ; Incomplete Paralysis ; Death from Exhaustion.*

J. N., female, æt. 60, hawker. Was in the habit of drinking whisky for seven years before death ; had three children, but was never married. Stated that since the year 1846 she had suffered much from headache and giddiness, and that in the end of that year she fell down whilst washing clothes. In falling, she cut her forehead, but knew nothing of the accident for some time, when, on recovering from insensibility, she found that she had been bled, and that active purgatives had been given to her. About the end of the year 1847, she fractured the neck of her left femur in leaping out of a cart, and was removed to the hospital, and placed under the charge of Dr Keith. She recovered from this accident, and walked about tolerably well on crutches until the end of March 1849, when she began to complain of pains and numbness in the right

arm and leg, and the right side of the body. From the time of the fracture, she had also pain in the left hip-joint during movements of the limb. After the end of March she never left her bed, and appeared to be sinking gradually. Wine was ordered by her medical attendant, and continued until her death. The bowels were habitually constipated. Complete retention of urine took place, requiring the introduction of the catheter on the 27th of May, and every day afterwards up to her death. The urine became exceedingly offensive: it was thick, of a reddish-brown colour, and was believed at times to contain blood. On the 4th of June, the patient was removed to the poor's house, where she continued to sink, and died on June 10th.

Post-mortem Examination, made in the dissecting rooms. The body is considerably emaciated; numerous white lines on the skin of the abdomen indicate previous distension; the left lower extremity is two and a-half inches shorter than the other; it rotates and moves in every other direction more freely than natural; the greater trochanter appears to project considerably, and is found much higher than usual, and not more than three inches distant from the anterior superior spinous process of the ilium.

Head and Spine.—The membranes and substance of the brain externally appear healthy. On making sections of the right corpus striatum, an elongated reddened spot is seen of three-fourths of an inch in extent from before backwards. The structure of this part appeared perfectly healthy on microscopic examination. The spinal cord was exposed by removing the lamellæ of the vertebræ, and when the dura mater was slit open posteriorly, both the membranes and the cord appeared healthy, except that three small calcareous plates were found on different parts of the arachnoid at the lower part of the dorsal region. On removing the cord and membranes from the vertebral canal, a yellowish-white curdy deposit of an inch and a-half in length and half an inch in breadth, was found lying between the dura mater and the posterior part of the bodies of the 2d and 3d cervical vertebræ. Part of the deposit was evidently in the substance of the dura mater, though it could not be seen from the inner surface, whilst anteriorly it was lodged in the substance of the periosteum, which had completely given place to it, in the position of the body of the 3d vertebra, allowing it to rest on the bone. Microscopic examination—not made, however, until a considerable time after death,[1]—showed the curdy matter to consist of a multitude of irregularly rounded, oblong, or triangular granular cells, measuring from $\frac{1}{2000}$th to $\frac{1}{3500}$th of an inch, lying in a sort of blastema, and associated with a number of small loose granules. Acetic acid produced no change in these structures, besides rendering the cells very slightly more transparent.

Chest.—Both lungs are firmly attached to the walls of the chest in every part by old pleural adhesions. At the apices of the lungs there are slight puckerings. Heart and pericardium healthy.

Abdomen.—All the organs are perfectly healthy, except the mucous membrane of the bladder, which is covered by a thick brownish layer of mucus. It is also softened and covered by a multitude of small and irregular ulcerations. Six or eight ounces of thick brownish and excessively fetid urine were found in it.

Left Lower Extremity.—Hip-Joint.—All the muscles in the neighbourhood, and especially the glutei and the pectineus, are much atrophied. The fibres of the gluteus medius and minimus are considerably shortened. The capsular ligament is much thickened, but complete. Above the joint, the cotyloid ligament is almost wholly destroyed, and the capsular ligament is attached to the

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[1] The body had been injected with a strong solution of arsenite of potash;—the nervous matter of the cord was softened, but the curdy matter did not appear to be decomposed. On several occasions I have purposely examined tubercular and cancerous deposits under similar circumstances, and found their structural characters well preserved in the parts not exposed.

periosteum half-an-inch beyond the edge of the cavity; below, it is attached quite close to the trochanter minor and the psoas; and behind, it is blended with the external rotator muscles at their insertion. At all these points, the original capsule appears to have been detached or destroyed, a large portion of the present capsular attachments being apparently formed by effusions of lymph, which have become converted into dense fibrous tissue. On opening the capsule, the head is found to be separated from the bone, and lying in the acetabulum, presenting, towards the neck, a flattened but rough surface, the margins of which correspond precisely to the edge of the cartilaginous surface. The neck cannot be said to exist, having been almost wholly removed by absorption. Between the inter-trochanteric lines there is an extremely irregular surface directed towards the head. From this surface and its margins, several dense fibrous bands pass to be attached to the capsule near the head, and one or two to the irregular surface on the head itself. In the substance of these bands are several loose portions of bone, which appear to have been separated at the time of the accident. On withdrawing the head from the acetabulum, to which it is attached in the usual way by the round ligament, the latter brings away with it a dense white membrane, which covers the whole surface of the cartilage, and is reflected from it to the ligament. Where the membrane has been torn off, the bone at the bottom of the acetabulum is left rough and bare, and where the membrane has been withdrawn from the surface of the cartilage, the latter is roughened. At the upper part of the acetabulum, the cartilage, covered by the membrane, presents several depressions and irregularities, and similar appearances are seen also at the lower part near to the edge of the cartilage. In the latter position there is one part, where the bone appears to be covered merely by a thin membrane, of the characters of the one described on the depressed patches on the condyles of the femur in Obs. IV.

A thin fibrous membrane covers the fractured surface of the head of the femur, and is continuous with a denser membrane, which can be traced over the whole cartilaginous surface to the round ligament. The membrane and round ligament can be readily stript off, and they expose bare bone near the attachment of the ligament, and at the very edge of the articular surface; but, on the remaining parts, a layer of cartilage, of considerable density and granular on the surface, is found. A brownish-red patch, three-fourths of an inch long and half-an-inch broad, immediately above the round ligament, presents a very thin layer of cartilage, which at one spot is quite yellow; whilst another patch of half-an-inch in diameter, below the ligament, has an extremely thin covering of reddish-brown cartilage. The bone, under these two patches, and at the irregular one near the round ligament, from which the cartilage has been altogether removed, has its texture completely opened out and softened, being in some parts a mere shell, which breaks down readily under the fingers.

Knee-Joint.—The cartilage of the femur presents a deep depression, elongated from before backwards, on the inner condyle, near its junction with the trochlea. Over a space of the size of a shilling, on the centre of the trochlear surface, there is a patch of fibres, many of which project half to three-fourths of an inch beyond the surface. The cartilage of the external glenoid surface of the tibia is depressed in the centre, and furnished with projecting fibres, and that of the internal surface is slightly fibrous in the centre, but the cartilage of both surfaces is nearly healthy under the semilunar cartilages. The cartilage of the patella has a large and irregularly depressed patch in the centre, running towards the internal edge of the bone. Its whole surface appears thinly covered with fibres, and, in some places, is rough and granular.

The cartilages of the ankle-joint, and of the articulations of the foot, appear healthy; but a few are thinned at their edges, and the bones at such parts are excessively soft.

The shaft of the femur of this side has a very large medullary canal, and very little compact tissue; its lower end is considerably softened. The whole of the tibia consists of soft spongy substance, which admits of its being crushed

in the hand or twisted in any direction. The fibula is similarly changed, and can be twisted round the fingers very readily. All the bones of the foot can be crushed between the finger and thumb with the greatest ease. The cancelli of these bones contain an unusual quantity of oil.

Right Lower Extremity.—The neck of the right femur is attached to the shaft at an angle of 125°. A section through the head, neck, and shaft, shows a lamina of dense bone, nearly one-fourth of an inch thick, at the lower part of the neck, supporting it. The shaft consists of a thin shell of bone, leaving a large medullary canal, in which there are only a few slender spiculæ.

Hip.—The cartilage of the head of the femur is rough and velvetty around and below the attachment of the round ligament. It terminates at the edge of the surface by a very irregular line, where it is much thinner, and runs towards the bone, which is nearly bare. The cartilage of the acetabulum is thinned at the edges, but otherwise appears healthy.

Knee.—The cartilage of the femur is soft and velvetty on the centre of the trochlear surface, and at an elongated spot near the inner edge of the inner condyle. The latter spot is an inch-and-a-half long, by one-fourth of an inch broad, and presents a number of loose fibres projecting from it for three-fourths of an inch. At the posterior part of the external condyle, there is a depressed spot of the size of half a split pea, where the bone appears covered by a very thin layer of membrane or cartilage. The cartilages on the glenoid surfaces of the tibia are very soft and spongy, where they are not covered by the semilunar cartilages. The external surface is much more diseased than the internal, and it has long fibrous processes projecting from it. The lower surface of the external semilunar cartilage is covered with fibrous processes near its free edge at about the middle of its length. The lower three-fourths of the cartilage of the patella is soft and velvetty; the upper part is dense, but presents three small depressions of considerable depth.

Ankle and Foot.—On the edge of the anterior articulating surface of the astragalus, there is an elongated depression one-fourth of an inch long, where the bone is bare. The other cartilages of these parts appear healthy. The bones present their usual firmness, and contrast remarkably with those of the opposite limb.

Microscopic Examination.—Left Hip-Joint.—The fibrous membrane covering the surface of the cartilage of the femur and acetabulum, consists of fibres which run for the most part horizontally, but in some places interlace with each other. In the acetabulum, distinct cells, $\frac{1}{700}$th of an inch in their greatest diameter, are seen amongst the fibres. These cells are beautifully rounded or oblong, and contain patches of granules in their centre. In many parts, as on the centre of the surface on the femur, a number of the cells of the superficial layers of the cartilage are seen to be mixed with the fibres, these cells being in different stages of degeneration. On the addition of acetic acid the fibres swell and become excessively transparent, a number of elongated granular nuclei, or elongated patches of dark granules, becoming visible at the same time. On examining the part of the membrane in contact with the cartilage, it is evidently seen to be formed of the superficial layers of that tissue, the cells becoming granular, and constituting the nuclear bodies which appear on the action of acetic acid. Distinct nipple-shaped processes of the membrane are seen lodged within the notches produced by the evacuation of the contents of the diseased cartilage cells.

The superficial layer of cells is only found in some parts. Its cells exhibit all stages of the changes represented in Figs. XI., XII., XIII., XXV., and XXXIV. In different parts they are found slightly swollen, and have their usual arrangement—still more enlarged and disposed irregularly—or they are seen to be so much swollen as to be irregularly rounded, constituting large granular or molecular patches or cavities. Their contents are the usual nuclei increased in number, small corpuscles, dark granules, or a molecular mass. The deep cells are similarly changed. When they measure $\frac{1}{200}$th of an inch in dia-

meter, scarcely any trace of nuclei can be seen, and the cells contain dark granules and molecular matter. The hyaline substance near to the surface has indistinct fibres running in the direction of the superficial cells; whilst, in the deeper parts, it is dark and molecular, or indistinctly fibrous, the fibres running perpendicularly to the surface.

The yellow spot on the cartilage of the head of the femur is found to be owing to the existence of a quantity of oil in the substance of the tissue; but no fatty matter exists within the cells, though they are much enlarged. In some places, where the cartilage is not more than $\frac{1}{45}$th of an inch thick, the superficial cells are not all removed, and yet no progressive enlargement of the deep cells is observed on passing down to the bone, as it is on passing towards the free surface at parts where that constitutes the principal or sole point of absorption. On examining the edge of the cartilage at the margin of the surfaces, and near to the attachment of the round ligament, after stripping off the fibrous membrane from it, the hyaline substance is found to be converted into distinct interlacing fibres, amongst which are numbers of cells or corpuscles, the whole tissue at these parts being almost precisely similar to fibro-cartilage.

The microscopic appearances in the diseased parts of the other cartilages of these extremities are similar to those above-named. Where there is a fibrous membrane on the surface, or where fibrous processes project from it, these consist of bands or fibres running in different directions according to the position of the cells where the fibres are formed. Amongst the fibres are a few cartilage cells, or granular patches indicating their former existence, and always seen after the action of acetic acid. The mode of junction of the fibrous layer, found on the surface, with the cartilage, and the changes which are seen to have taken place in the superficial and deep cells are precisely similar to those which have been already described in the cartilages of the left femur and acetabulum.

Upper Extremities.—The cartilage covering the heads of both humeri presents whitish patches, as if there were a deposit of dense matter immediately below the surface. The cartilage of the glenoid cavities is healthy.

Elbow-Joint.—The cartilage on the lower articular surfaces of the humeri is reddened and velvetty on the small head and the adjacent ridge,—that on the right humerus presents an abraded spot on the trochlear surface, and an elongated but deep notch, $\frac{1}{4}$th of an inch long, and $\frac{1}{12}$th broad, at the anterior part of the ridge, separating the trochlear surface from the small head. The cartilage on the heads of the radii is reddened and velvetty or fibrous, especially at the edge of the humeral surface, and at the part near to the ulna, where nearly the whole thickness of the layer is diseased. The cartilage on the ulna is slightly reddened and velvetty. On each side, it is divided into two parts by an irregular membranous band passing across at the junction of the olecranon with the shaft. At this point, the bone appears almost bare, except at the radial end of the band on the left ulna, where there is still a layer of cartilage.

Wrist-Joints.—The left wrist-joint is healthy. In the right one the cartilages covering the lower surfaces of the radius and ulna, are converted into fibres at their margins, whilst their central parts are covered by a thin membrane.

Right-Hand.—Many parts of the cartilages of the bones in the lower carpal row are reddened, velvetty, and abraded. The semilunar surface of the unciform, and the unciform surface of the fourth metacarpal bone, present irregular spots, where the bone is completely bare, smooth, and covered with porcellanous deposit.

Left-Hand.—The cartilage on the lower surface of the scaphoid, and on the semilunar surface of the unciform bone, is removed on each for an extent of 1-6th of an inch. The exposed bone is perfectly smooth and porcellanous, and the edge of the surrounding cartilage is loose and velvetty. The surface of the trapezoid for the trapezium is in the same state. Several other cartilages of the carpal bones are reddened and velvetty, and especially the cartilages in the carpo-metacarpal articulation of the thumb.

Microscopic Examination.—On examining vertical sections of the cartilage of the heads of the humeri, passing through the white spots, these are found to be produced by a deposit of dark-brown granules in the intercellular substance of the superficial layers of the cartilage, the cells of the same parts being slightly enlarged and granular—(Fig. XL.) On the addition of acetic, or the stronger

Fig. XL.

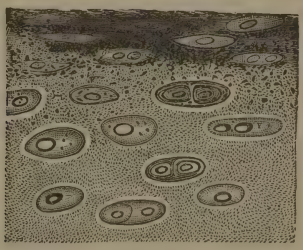

Fig. XL.—*Vertical section of the superficial layers of cartilage from the head of the right humerus having the whitish deposit between the cells.*

acids, to such sections, the dark-brown deposit gradually disappears, passing through different shades of brown and yellow. A slight effervescence takes place at the same time, but the bubbles appear to proceed from all parts of the cartilage, and certainly not in greater number from the dark deposit than from other parts. On neutralising the acid solutions thus obtained, and adding a solution of oxalate of ammonia, a copious and immediate deposit of crystals of oxalate of lime takes place. Similar indications of the existence of a smaller quantity of lime are given by acting upon other portions of the same cartilages, in which there is no deposit in the superficial parts.

All the velvetty parts of these cartilages present the usual bands or fibres into which the hyaline substance has been split, the horizontal or vertical arrangement of these depending on their position in regard to the superficial or deep cells. Where the cells of the superficial layers exist, they are enlarged, especially in breadth, separated from each other, and granular. The deeper cells measure, in many places, $\frac{1}{200}$th of an inch in their long diameter, and one, which appears ready to burst, measures $\frac{1}{140}$th of an inch. Their contents are, a number of beautifully spherical cells, which measure $\frac{1}{1400}$th of an inch, and contain round and bright granules—(Fig. XLI.) ; whilst, in some places, the cells contain corpuscles of the ordinary irregular form.

Fig. XLI.

Fig. XLI.—*Large cells, containing smaller granular ones, found in many cartilages.*

Where the cartilage is covered by a membrane, this consists of interlaced fibres, amongst which are nuclei in all stages of elongation, patches of granules, and a few cartilage cells. The membrane presents rounded notches, where it is adapted to the surface of the cartilage. The membrane covering the ulna, at the junction of the olecranon with the shaft, contains a number of small bodies like osseous lacunæ, which appear surrounded by a cell wall, as if formed by the calcification of cartilage cells.

Overhanging and bordering the parts which are becoming porcellanous, the

hyaline substance of the cartilage is split into bands and fibres, amongst which are large cells of the characters of that delineated in Fig. XLI.; or, a number of smaller cells are found amongst fibres which interlace with each other, composing a tissue which is almost precisely similar to fibro-cartilage in its structural characters.

Remarks.—Without any symptoms which could be supposed to indicate diseased action in the joints in this case, almost every articular cartilage in the body became seriously affected, and parts of several were almost wholly destroyed. The nature of the disease in every joint was similar to that before detailed in Obs. I., II., IV., and VII. On many of the cartilaginous surfaces there was a dense fibrous membrane, which was evidently produced by splitting of the hyaline substance of the cartilage into fibres, and by changes in the cells, which had reduced them to granular nuclei or isolated patches of dark granules. In the elbow-joint an apparent conversion of cartilage cells into osseous lacunæ, by calcification of their walls, was observed. The softening of the bones of the left lower extremity, independently of those of other parts, was very remarkable, and might possibly have been produced by diminished nutrition of the whole of the parts of this extremity since the occurrence of the fracture.

The nature of the disease which led to the death of the patient cannot be made out very positively. The symptoms were those usually produced by cerebral congestions or chronic lesions of the nervous centres, and with them must be considered the deposit in the upper part of the vertebral canal. This deposit had every appearance of tubercle; but it was not found until too late a period to allow of its nature being very positively stated by microscopic examination. The ulceration of the bladder probably hastened the fatal termination, and may have arisen from the retention of urine, as there was no disease of the other parts of the urinary tract.

Observation X.—*Gelatinous Degeneration of the Synovial Membrane of the Ankle and Tarsal Articulations—Loosening and Destruction of the Cartilages—Softening of the Bones—Anchylosis of the Astragalus and Os Calcis—Amputation—Recovery.*

David Wood, æt. 25, farm-servant, admitted into the Aberdeen Royal Infirmary on April 11, 1849, under Dr Pirrie, for disease of the left ankle. He is tall and thin; complexion and hair light.

States that he was confined to bed for rather more than a week, from an attack of scarlet fever, in the beginning of November 1848. In the beginning of the year 1849, his feet became uneasy on walking; and about the second week of February his left ankle first became the seat of greater stiffness and uneasiness than other parts, inducing him to walk on the outer part of the foot rather than on the sole. This slight uneasiness in the ankle gradually increased, and the toes became unusually stiff and extended. After two or three weeks, he observed a swelling on the outer side, which gradually extended round the joint, with increase of pain. The pain was but little felt when sitting quietly, but it was very severe in walking, obliging him to give up work a month previous to his admission, and of late destroying his rest in bed. He applied a few leeches to the joint a short time since, but with little or no benefit.

On admission, there is a considerable, hard, and but slightly elastic swelling of the joint, very prominent over both malleoli, and at these parts of a red colour. The foot is immoveably fixed at right angles with the leg, the tendons of the muscles in the anterior tibial region being excessively tense. There is very considerable gnawing pain in the whole articulation, increased by pressure, or by attempting to walk, and generally alleviated, or temporarily removed, by rest. In bed, however, the joint is usually uneasy, and at times so painful as to disturb his sleep.

Perfect rest was enjoined—leeches were applied with some relief—cooling lotions were used and afterwards poultices. Matter formed successively on the inner and outer sides of the ankle-joint, and when evacuated by incisions in these situations, it was found to be of an unhealthy scrofulous appearance. These evacuations gave temporary relief, but the openings closed, and the pain soon became as bad as before. He was put upon a course of iodide of potassium and cod-liver oil, but the ankle did not improve, and towards the end of May the pain became exceedingly severe, his rest was much disturbed, and he became feverish. Anodyne draughts and febrifuge medicines were administered, and soothed him considerably, but no amendment took place in the ankle. The constitution was much affected by the irritation and confinement, which he appeared unlikely to be able to bear much longer, and therefore amputation was resolved on, and performed in the lower third of the leg, on June 16, 1849. A severe attack of erysipelas came on in the stump; but on July 21 the wound was nearly healed.

Examination of the Foot.—Fistulous openings in front and behind the lower tibio-peroneal articulation lead to bare bone upon the lower end of the fibula and adjacent part of the tibia. All the sub-cutaneous tissues about the ankle and tarsus are infiltrated with a soft, brownish, gelatinous mass, and the parts of the synovial membranes which cover the ligaments form very thick, brown, soft, and pulpy masses.

The ankle-joint contains a quantity of pus. The cartilage of the lower extremity of the tibia is smooth, and white on the surface; in many places it is not more than half a line in thickness, and over the whole surface it separates very readily from the bone, bringing with it small osseous spiculæ. The surface of the bone is highly vascular, and the part immediately in contact with the cartilage is softened; but at the distance of a line it appears more healthy. The contiguous non-articular parts of the tibia and fibula present rough bone, covered only with the brownish gelatinous substance. The cartilage of the fibula is reddened and pulpy over half its surface, and it is readily detached from the bone. The greater part of the upper articulating surfaces of the astragalus is covered with white and dense cartilage; but, in many places, it is reddened, pulpy, loose from the bone, and even perforated, so that the bone can be felt bare through the openings in the cartilage.

The inter-osseous ligament between the astragalus and os calcis is almost completely destroyed. What remains of it consists of softened fibres, which can be readily picked away with forceps, whilst firm ossific union has taken place at those parts from which it has been removed by disease. The cartilages of the anterior and posterior articular facettes, between the above-named bones, are $\frac{1}{16}$th of an inch thick, and applied close to each other, without union. They appear healthy, except at the portions immediately in contact with the ossified parts, where they are somewhat gelatinous and transparent.

The cartilages on parts of all the surfaces in the articulation between the two rows of tarsal bones are reddened, thinned, and capable of being easily torn from the bones. Those situated further forwards in the foot appear healthy on the surface; but, under all, except those of the phalanges, the bone is considerably softened in its texture. All the bones of the tarsus, and the extremities of the metatarsal bones, cut readily with a knife, and the tarsal

ones, near many of their articulating surfaces, can be broken down with the fingers.

Microscopic Examination.—The reddish-brown gelatinous mass contains a number of fusiform and nucleated cells; a few exudation corpuscles, only partially filled with granules; and a large number of spherical, finely granular, and indistinctly nucleated cells, varying in size from $\frac{1}{2300}$th to $\frac{1}{1400}$th of an inch. On the action of acetic acid, these cells show a distinct, rounded, and granular nucleus, measuring in all the cells about $\frac{1}{3500}$th of an inch. All the structures named as components of the gelatinous mass lie in a finely granular blastema.

The cartilages of the ankle-joint have no trace of the horizontal layer of cells on the surface, but this presents a number of free fibres, $\frac{1}{350}$th of an inch long, projecting into the joint, and formed of hyaline substance. At the attachment of the bands and fibres to the hyaline substance it is finely molecular, and excessively transparent. The same molecular, disintegrated, hyaline substance is seen on the deep surface; but on this there are neither bands nor fibres. In the centre of the cartilage the cells are of the usual size, but their nuclei are extremely irregular in form; nearer the free surface the nuclei are seen in all stages of conversion into granules, whilst the cells are rounded, and, instead of lying in groups, are separated at equal distances from each other. Several of these cells have a finely mottled appearance, as if the granules had been converted into molecules. (See the large molecular patches in Fig. XIII.) Near the deep surface the cells are but little changed, whilst directly upon it they are molecular, and apparently undergoing the same process of disintegration as the hyaline substance. On adding acetic acid to these cartilages, the hyaline substance becomes very transparent, but the granules are in no way affected.

In the articulations further on in the foot, similar changes have taken place to a less extent. On the posterior surfaces of the scaphoid and cuboid bones, the superficial cells are swollen and granular, as well as separated from each other at a greater distance than in the natural state. On the cuboid the superficial cells measure $\frac{1}{850}$th by $\frac{1}{1500}$th of an inch, and contain irregular or rounded granules, very much like oil, and measuring $\frac{1}{3500}$th of an inch when of the largest size, whilst there are all sizes below this. The deeper cells are quite healthy.

The anterior surfaces of the cuboid and scaphoid bones present similar changes in their superficial cells, but only in some places, and to a slight extent. The adjacent surfaces of the cuneiform bones are considerably softened, and their cartilages are more diseased; the cells are swollen and granular, and the hyaline substance is broken down and molecular, or is indistinctly fibrous. The cartilages applied close to each other in the anchylosed articulation have their superficial cells crowded with eight or ten corpuscular nuclei in each. They are $\frac{1}{470}$th of an inch long, and separated from each other. The usual clusters of deep cells are only seen in a few parts, for the cells have in most places separated from each other, and have become swollen and rounded. Near the anchylosed part the cells are further changed, and the majority of them measure $\frac{1}{470}$th by $\frac{1}{580}$th of an inch, and are crowded with corpuscles. Many of the smaller cells are perfectly spherical, and present a nucleus without evident boundaries, as it appears to be only a denser portion of the gelatinous contents of the cell. In all these parts the hyaline substance is finely mottled. The portion of the cartilage exactly in apposition with the anchylosed part of the bones has its hyaline substance converted into perfect fibres, interlacing in every direction, and forming spaces of $\frac{1}{470}$th of an inch in diameter, which are occupied by dilated cells, or by clusters of corpuscles which have evidently belonged to single cells. In other parts the corpuscles are quite free, and lie in hyaline substance, or perfect fibrous tissue formed from it. They measure from $\frac{1}{2300}$th to $\frac{1}{3500}$th of an inch in their long diameter, and but little less transversely.

Remarks.—At the period of amputation, the bones, and other structures entering into the composition of the articulations, were all seriously diseased. Except in the case of the cartilages, it is not easy to point out what course the diseased action was taking, nor is it possible to state with certainty in what tissue it commenced. The bones had their articular lamina almost wholly destroyed by softening and opening out of the texture; and thus the attachment of the cartilages was loosened. Beyond their articular surfaces, several of the bones were not so much diseased, but the tarsal ones were softened throughout. All the cartilages were in a much more advanced stage of disease on their free surfaces than elsewhere; and those in which the disease was just commencing showed this at the same part, notwithstanding that the bones under them were completely softened. The disease in the synovial membranes and soft tissues had the characters of the gelatinous disease of authors. I am disposed to regard the disease as one of anormal nutrition, depending on a constitutional cause, but manifesting itself at a particular part. It is not unlikely that all the textures suffer at once, but that the effects of the disease are first manifested in some cases in the softer tissues, and in others in the bones; but little pain being produced until the last-named texture becomes seriously involved. If the disease is a general one, it is at once explained why local treatment, especially in hospitals, so uniformly fails, and occasionally proves injurious, whilst the administration of Ol. Jecoris Aselli and good diet, in a pure atmosphere, is at times attended with the most satisfactory result.

Observation XI.—*Disease of the Articular Ends of the Bones, and of the Cartilages of the Knee-Joint—Gelatinous Degeneration of the Synovial Membrane—Amputation.*

James Jackson, æt. 34, of scrofulous diathesis, admitted into the Aberdeen Royal Infirmary on March 17, 1849, under Dr Pirrie, complaining of pain, swelling, and stiffness in his right knee.

States that whilst at work, two years and a-half ago, he gave his knee a violent twist, after which it was painful for a few days, and then got quite well. About eighteen months ago he first felt deeply-seated and gnawing pain, generally confined to one spot below the patella, but occasionally affecting both sides of the knee. The pain was most severe when he was warm in bed, and was of the same character as it has maintained throughout. For eight or nine months the knee has felt stiff; but no swelling was noticed about the joint, to the best of his recollection, until four or five months after the commencement of the pain. Twelve months ago he began to use a liniment, and continued its application for six weeks. On the 23d of April 1848, he first applied to a medical practitioner. His knee was blistered, and various ointments were applied until about five weeks before his admission, but without benefit.

On admission, the joint is considerably swollen, especially on the lateral parts above the articulation, where the swelling is soft and yielding; but it is hard in other parts. There is great stiffness of the joint, owing to the swelling, which is increased by standing or walking, and diminished by rest in bed. Severe, and nearly constant, gnawing pain exists below the patella, and is increased on pressure. Pain of a similar character is also occasionally felt on the outer, and less frequently on the inner, side of the joint, being worst in

bed, and on beginning to walk; whilst, after walking a little distance, it is less perceived. Severe pain is also excited by any sudden shock applied to the foot, as in making a false step.

March 20.—Eight ounces of blood to be drawn from the knee by cupping, and one grain of proto-iodide of mercury to be taken night and morning.

March 27.—Scott's dressings of compound mercurial ointment were applied to-day.

April 4.—The pain has almost left the knee. A short splint is applied behind the joint, to secure perfect immobility. To continue the pills.

April 26.—On removing the dressings, the swelling is seen to be less than at any other period since its commencement; the pain is very slight, and has only been felt at intervals since the application of the dressings.

April 28.—A small blister to be applied below the knee, on the inner side, and to be dressed with mercurial ointment.

May 6.—To discontinue the pills. The mouth has not been affected.

May 8.—A slight swelling, with fluctuation, exists on the inner side of the lig. patellæ. On making a small puncture into it, a little pus escaped; the opening was carefully closed, and absolute rest enjoined.

May 10.—Three drachms of scrofulous pus were evacuated to-day, by a small incision made on the inner side of the patella, and afterwards closed.

May 24.—Has been using a lotion of spirit and acetate of ammonia, and taking the syrup of iodide of iron. More pus had accumulated, and it has been evacuated as before. A portion of the head of the tibia can be felt bare with the probe.

June 5.—To have cod-liver oil, instead of the syrup.

June 12.—To-day the pus has escaped from an opening spontaneously made. Poultices to be applied.

June 16.—Excruciating pain in the knee, on the slightest motion, has gradually come on of late. The patient is much thinner and paler than when admitted. As there appears to be no prospect of a cure, amputation has been resolved on.

June 23.—Chloroform was administered to-day, and the limb amputated above the knee.[1]

Examination of the Limb.—Considerable swelling exists about the knee-joint. It is round and pulpy, and the skin over it is white. All the sub-cutaneous tissues in the neighbourhood of the joint are infiltrated with a yellowish gelatinous substance; and, on opening it, a quantity of brownish purulent fluid is found.

The articular surfaces of the femur and tibia are covered with white and dense cartilage, which is smooth on its free surface, and can be very easily detached from the bones, tearing away small spiculæ with it. The edges of the surfaces are overlapped to the extent of one-eighth of an inch all round, and to that of half an inch at the junction of the trochlear and condyloid surfaces of the femur, by a highly vascular and soft membrane, continuous with the synovial membrane covering the ligaments. Under the thickened or newly-formed membrane, the surface of the cartilage appears abraded and uneven. The edges of the cartilages of the tibia are grooved half-way through their thickness, and rough upon their surface where the internal edge of the semilunar cartilage is applied upon them. The last-named cartilages themselves are soft and pulpy, and overlapped by the vascular membrane.

The internal part of the articular surface of the patella is destitute of cartilage over a rounded spot of half-an-inch in diameter, which has bare and smooth bone at its bottom. The cartilage of the lower half of the surface is thin, rough, and covered by the pulpy and vascular membrane an eighth of an

[1] The history and symptoms of the disease, up to the period of amputation, in this case, and in that recorded in Obs. X., have been abstracted from a report, kindly furnished me by Dr Pirrie.

inch thick. On the upper half of the surface there is tolerably smooth and thick cartilage, which is split by vertical fissures near the junction of the upper and lower halves. Here the superficial layers of the upper part of the cartilage overlap the granular surface of that of the lower half to an extent of one-sixth of an inch, so that they present a distinct and irregular fringe, running across the bone.

On cutting through the head of the tibia, its texture is found to be opened out and much softened, so that it can be readily broken down with the fingers. The articular end of the femur is also softened to a less extent, whilst the patella presents a much denser texture and a more firmly adhering cartilage.

Microscopic Examination.—The vascular membrane on the free surface of the cartilages, and the pulpy vascular mass between them and the bone, are of the same structure. Both consist of a mass of fusiform cells and blood-vessels, with a few exudation and plastic cells intermixed; whilst in a few parts there are perfect fibres. All these structures exhibit their usual reactions with acetic acid. The deepest part of the membrane on the surface contains, amongst its other structures, a granular mass, and a series of corpuscles, which appear to have been discharged from cartilage cells, and changed in their characters. Under the membrane the surface of the cartilage presents a number of very large cells, which are open on one side towards the fibro-nucleated and granular mass, and closed on the other. By this arrangement the surface of the cartilage presents notches, and the pulpy membrane a series of nipple-shaped processes which are lodged within them.

The inter-cellular substance, near the edge of the surfaces, consists of perfect fibres, whilst the cells measure $\frac{1}{140}$th of an inch long, and are crowded with distinctly granular corpuscles, measuring $\frac{1}{2300}$th by $\frac{1}{3500}$th of an inch.

Except at the very edge of the surfaces, the superficial cells remain. In the centre they are elongated, and full of granules, like oil; nearer the edges they are very much swollen, and present a molecular or granular mass in their interior. Many of such cells measure $\frac{1}{470}$th by $\frac{1}{700}$th of an inch.

The deeper cells are much less changed. They are somewhat enlarged, and their contained corpuscular nuclei are granular. Those at the deepest part have undergone scarcely any change, even on the surface which is so easily detached from the bone. The edges of sections passing through this surface show a number of notches; but it does not appear that these have any connection with the changes in the cells. The hyaline substance, through the whole thickness of the cartilage, is darker and more mottled than natural; whilst in many parts a filamentary arrangement of the molecules may be seen, similar to that described by Leidy in healthy cartilages, but much more evident than it.

In some parts the corpuscles are set free from their cells, and lie in the tissue; in others the walls of the corpuscles cannot be seen, and small patches of granules indicate their position.

The cartilages of the tibia present the appearances above-named. Where they are grooved under the semilunar cartilages their cells are all swollen, very large, granular, and presenting the appearances represented in Figs. XIII. and XIV.

The semilunar cartilages consist of a mass of fibres, interlaced in every direction, and have amongst them a number of loose granular corpuscles, many of which are as distinctly fusiform and splitting into fibres as the fusiform cells of granulations.—(Fig. XLII.)

Patella.—The superficial cells are removed from every part. Where the cartilage is dense, the cells are much enlarged, rounded, filled with corpuscles, and they lie in hyaline substance, which is darker and more granular than usual. A few of the deep cells have calcareous matter in their walls. In many places the cartilage is softened, and consists of a mass of fibres, mixed with clusters of granules, with granular corpuscles, or with large dilated granular

Fig. XLII.

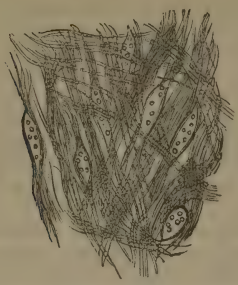

Fig. XLII.—*Fusiform cells splitting into fibres from the semilunar cartilages.*

cells. These softened parts are almost precisely similar in structure to those described in Obs. II., in the cartilage of the tibia.

Remarks.—At the period of amputation, the articular ends of the bones, the cartilages, and the synovial membrane, were all seriously diseased. The history of the case, and the character of the symptoms, would lead to the belief that the disease commenced in the tibia, and then extended to the other structures—to which view there is nothing to oppose in the anatomical characters of the diseased parts. The loosening of the attachment of the cartilage to the bones was apparently similar to that described in several cases by Mr Mayo, whilst there can be no doubt that the pulpy and vascular membrane overlapping the edges of the cartilages was a similar one to that which Mr Key believes to be the cause of ulceration. The disease was undoubtedly in a more advanced stage at those parts of the cartilage which were overlapped by the pulpy membrane, or the semilunar cartilages, than in others, which indicates simply that the cartilages so diseased derived their supply of nutritive material from the blood circulating near them in the membrane named, and not that the membrane in itself possesses any peculiar virtue by which it is enabled to act upon the cartilage, and cause its removal. The tolerably healthy appearance of the deep cells, where the cartilage could be separated from the bone by the slightest force, is opposed to the view that these textures derive their nutritive fluid principally from the dilated vessels of the bone. Lastly, it is not to be forgotten that the superficial cells of the cartilage were diseased in every part—that no evident membrane could be seen on the central parts of the surfaces—and that the deeper parts of the cartilages were least affected by the disease: circumstances which appear to point to the synovial fluid as possibly concerned in the supply of nutritive matter to the cartilages, both in health and disease.

Observation XII.—*General Disease of the Articular Cartilages—Death from Fever.*

The disease was found in this case in a female (J. A.), æt. 60, who died of fever in the Royal Infirmary of Aberdeen, on the 26th of May 1849. She had

been married, and had had several children. Other particulars of her history could not be obtained; for, when brought to the infirmary, two days previous to her death, she was in a state of insensibility, from which she never recovered.

The body was that of a well-developed female. In the post-mortem examination, made in the dissecting-rooms, there were no traces of disease in any of the organs, except in the existence of cicatrices and puckerings in the apices of the lungs, and old but not extensive adhesions of the pleuræ. The fibres of both recti muscles of the abdomen were torn, a little below the umbilicus, on both sides, and diffused clots of blood were found in the same positions. There was also some sero-sanguineous effusion in the sub-cutaneous cellular tissue over the ruptured part of the right rectus, this muscle being torn to a greater extent than the left one. No evidence of contusion existed on the skin.

Examination of the Joints.—Shoulders.—The cartilage covering both glenoid cavities was healthy; but the dark deposit, described on the head of the humerus in Obs. IX., was found here, also, in small quantity, in the superficial layers of the cartilage of both humeri.

Elbow-Joints.—The whole of the cartilages are of a rose-pink colour, especially where their surfaces are soft and velvetty. The cartilage on the lower end of the humerus of both sides is soft and velvetty on the anterior surface of the small head, and on the ridge separating this from the pulley-like surface for the ulna. The cartilage on the head of the radius on both sides is soft and velvetty on the whole surface opposed to the humerus, and especially for one-sixth of an inch around the cup-shaped cavity. The cartilage of the ulna, on both sides, is soft and velvetty, or flocculent, near the centre, and is divided into two parts by a band, one-eighth of an inch in breadth, which runs across at the junction of the olecranon with the shaft. At this part the bone is covered by a thin uneven membrane, except at a small spot, on the left side, where it is bare, and at another on the outer part of the band on the right side, where the cartilage still remains.

The cartilages of all the other joints of these limbs are healthy in structure; but some of the cartilaginous surfaces on the carpal bones are reddened near their edges.

Hip-Joints.—The cartilage on the head of the right femur is abraded near the edges of the surface. At the attachment of the round ligament there is a spot, of an inch in diameter, where the cartilage is reddened, velvetty, and fissured—the fissures radiating from the ligament. A large portion of the surface nearer the circumference is whitish and mottled, as if there were some deposit in the superficial parts. Several velvetty portions of cartilage are also found near the circumference of the acetabulum. Similar appearances occur in the left hip-joint, the cartilage of the acetabulum, and the round ligament of this side, being of a bright yellow colour.

The other joints of the lower limbs are healthy, except the tarso-metatarsal articulations, the cartilages of which have some deposit in their superficial parts.

Microscopic Examination.—All the parts which are velvetty and flocculent to the naked eye have their hyaline substance split into large bands, and these again into smaller ones. Where the cartilage is soft and pulpy, its hyaline substance is indistinctly fibrous and molecular; in a few places it is converted into fibres, which are arranged, in relation to the cells, like the fibres of fibro-cartilages. There are but few parts of any of the diseased cartilaginous surfaces which are quite healthy. Over the greater part of each, the superficial cells are enlarged, and somewhat rounded; containing, in some places, their usual nuclei; in others, the nuclei contain dark granules, or their former position is merely indicated by small patches of granules. The deeper cells in many parts are enlarged, rounded, crowded with granular corpuscles, or with similar ones without granules. These cells are of all sizes, up to $\frac{1}{120}$th of an inch. Many of the largest appear molecular, and are similar to those before represented in Fig. XIII.

The whitish mottled spots on the heads of the thigh bones, and in other parts, are precisely similar to those described on the humerus, in Obs.

IX. The superficial cells of such parts are enlarged, rounded, and contain globules of oil of different sizes. They lie in a dark brown granular substance, which, on being acted on by acetic or the stronger acids, loses its colour, passing through different shades of brown and reddish-brown to yellow, the colour in one stage being precisely the reddish-brown colour assumed by the intercellular substance in many of the former cases of fibrous degeneration. Some of this dark deposit was found in the cartilages of the right elbow-joint, where it could not be observed with the naked eye.

The cartilage of the ulnæ, near the line separating the olecranon from the shaft, presents a number of beautiful hyaline bands on the surfaces, and in the deeper parts rounded cells of larger size than in any of the foregoing cases: many measure $\frac{1}{100}$th by $\frac{1}{120}$th of an inch, and a few are considerably larger. Some of the cells are seen to be breaking up, each of the numerous contained corpuscles appropriating to itself a part of the gelatinous contents of the cell, so as to complete itself, and constitute another nucleated cell:—others contain small patches of molecular or granular matter, which are the only indications of the previous existence of the nuclei or corpuscles.

In the examination of the yellow parts of the cartilage of the left acetabulum, the whole colouring matter is found within the cells, giving to the texture a very beautiful appearance.

Remarks.—This case was so similar to that recorded in Obs. IX., and some of the former ones, that scarcely any remark is necessary. The disease was general, and possessed of the usual pathological characters.

Observation XIII.—*Complete Destruction of the Cartilages, Ligaments, and Synovial Membrane of the Knee-Joint—Exudation, with Vegetable Productions in the Pharynx, Œsophagus, and upper part of the Larynx—Death.*

This patient, a female, æt. 22, was admitted into the Aberdeen Royal Infirmary, under Dr Laing, in May 1849, for disease of the knee-joint. I did not see her during life, but was enabled, by the kindness of Dr Laing, to collect the few following facts from his case-book, and the account given by himself, the dresser, &c.:—

The patient had previously suffered from disease of the elbow-joint, but she stated, on admission, that it gave her no trouble, notwithstanding that considerable grating of the ends of the bones took place on motion of the joint. One ankle-joint had also given her uneasiness. The disease in the knee-joint had existed fifteen months. When admitted, this joint gave her considerable pain, especially on motion; it was swollen and rounded; the skin was perfectly entire, and had been so from the commencement of the disease; the leg was unusually moveable at the joint, and required support by pillows, to prevent its bending laterally upon the thigh. The disease appeared to progress gradually; the pain continued very severe; the leg became more moveable laterally at the knee, but the skin remained entire throughout; and not the slightest tendency to the formation of an abscess was observed. After about six weeks from the time of admission, she complained of being very unwell; in two days from this time she had severe rigors, with difficulty of breathing and deglutition; and death took place within twenty-four hours of the occurrence of the last-mentioned symptoms.

Post-mortem Examination.—(Thirty-six hours after death.)—The lower lobes of both lungs are intensely congested, and calcareous concretions, apparently occupying the bronchi, and of a branched form, are found in two or three places. A greyish-white curdy mass of exudation is found covering the whole of the pharynx, and three or four inches of the upper part of the œsophagus. It can be readily detached from the mucous membrane, which is slightly reddened, and it extends to the margins of the glottis, covering the aryteno-epiglottidean folds. In the interior of the larynx a thin and more

transparent lymph is found in small quantity. The organs in the chest and abdomen, which have not been named, are healthy. Head not examined.

Knee.—On opening the joint, five or six ounces of dark-brown and thick pus escaped. The sub-cutaneous tissues are gelatinous,—the lateral ligaments are completely destroyed, and the crucial ones are considerably diseased. All the surfaces of the bones entering into the formation of the joint are of a dirty-brown colour, and appear rough and completely bare. On more careful examination, a few small and thin patches of cartilage are found on the femur, whilst every other portion of cartilage, and the semilunar cartilages, have disappeared. The texture of the bones is opened out, but they cannot be crushed between the fingers.

The elbow and ankle joints not examined.

Microscopic Examination.—The cells immediately on the surface of the patches of cartilage are very large, rounded, and molecular, opening on the surface; those situated deeper are less changed in shape and size, but they contain a number of dark granules in lieu of their nuclei. The hyaline substance presents an indistinct and mottled appearance.

The lymphy substance found in the larynx is a granular mass, with which primitive filaments (Bennett) and epithelial scales are intermixed; that in the œsophagus and pharynx contains a mass of epithelium, with granular matter and vegetable productions, such as are found in ordinary aphthæ, but with stronger stems and larger sporules. The vegetations form at least half the bulk of the pharyngeal exudation.

Remarks.—One peculiarity in this case was the complete destruction of every texture in the knee-joint, and probably, also, of the cartilages in that of the elbow, with less suffering and constitutional affection than happens in many cases where there is only very slight destruction of tissues. The brown degeneration had gone on in the knee until five or six ounces of dirty brown pus were formed, and yet there was no approach of the matter to the surface in any part,—but slight emaciation had resulted,—and there is reason to believe that had the knee-joint been the only diseased part, recovery might have taken place after amputation.

The fatal disease in the pharynx, œsophagus, and upper part of the larynx, was of an aphthous nature, the greater part of the exudation being composed of the usual vegetations. There is no reason to believe that it had existed more than three days previous to death, but whether it resulted from the disturbance caused by the diseased joints, or arose altogether independently of that cause, there does not appear to be sufficient evidence to decide.

(To be continued.)

ARTICLE III.—*Note on the Existence of Bile in the Stools of Cholera, and in the first Urine Secreted after the Algide Stage.* By E. A. PARKES, M.D., Professor of Clinical Medicine in University College.

I FORMERLY[1] expressed a doubt whether the peculiar violet, pinkish, or purplish colour which is frequently produced in the rice water

[1] London Journal of Medicine, February 1849.

stools of cholera, by the action of a small quantity of nitric acid, and in a less degree by hydrochloric acid, when aided by heat, was to be attributed to bile. This doubt was chiefly based on the following grounds:—1st, The common green colour produced by the action of nitric acid on biliphein was always absent; 2d, I had found no certain evidence from the examination of healthy and diseased stools, and of the bile itself, that bile-pigment, or any other ingredient of the bile, could give this reaction; and, 3d, the peculiar reaction so strongly resembled that given with nitric acid, by certain urinary colouring matters, which were not supposed to be derived from the bile, or to be only indirectly derived from it, that I could not but believe I had to do with the same substance in the case of the rice water stools.

But additional inquiries have led me to alter this opinion, and to believe the statement of Simon, that the substance giving this reaction is "modified bile," to be the correct one. In a perfectly healthy stool, I have lately obtained this violet reaction instead of the ordinary evidence of bile. I have found a similar tint almost invariably in the liquid stools of typhoid fever, and occasionally in those of typhus fever. And as I have at present been able to find no evidence that normal or morbid pancreatic fluid, or any exudation from the intestinal mucous membrane, can give this reaction, I infer that it is attributable to bile. I have not been able to discover the cause of this peculiar, though, as it has occurred in a perfectly healthy stool, not abnormal condition of the bile. It is not owing, as might be supposed, to excessive dilution, which might permit the bile-pigment to be acted on so rapidly by the nitric acid, that the green tint would be only momentarily seen, and would pass at once into one of the colours which in healthy bile succeed to it, and which might be conceived to be more stable. For if the bile of cholera, as of any other disease, be diluted to the utmost extent, so that only the slightest tint is given by nitric acid, still that tint is invariably green; and if the solution is as dilute as it can be to give any colour at all with nitric acid, an excess of acid destroys the green colour without any transition tints. Possibly, as Simon supposed, the bile-pigment may become oxidised, or otherwise altered by the fluids with which it is mixed, or some normal constituent of the bile may be present in unusual quantity, while the common bile-pigment may be at the same time deficient. Whatever be the nature of this substance, it gives no reaction with sugar and sulphuric acid.

But although this violet and pink tint can be obtained from many cholera stools in the early stage and in the milder cases, it cannot generally be procured from the fluid passed during the greatest height of the intense disease. So that at this period there is probably complete retention of bile; at any rate, there is no evidence whatever, that I can discover, of any passage of bile into the canal.

In the first urine passed after the cold stage, there is generally a large quantity of a substance which gives the same characteristic

colour with nitric acid. Whatever may be in other respects the condition of the urine, or of the fluid which represents it, more or less of this modified bile is present, and can be readily detected by nitric acid, either at once or by forming an alcoholic solution of the extract. Thus I have found it in a fluid, drawn from the bladder after the cold stage, in a case in which there was no further secretion. This fluid, about ʒi. in quantity, was almost like coffee grounds, from the presence of a great number of blood-particles; it contained, of course, albumen, but no urea, and gave, with nitric acid, a deep violet colour, passing into a blackish purple. In other cases in which the urine has been transparent, yellow, acid, without urea, or with mere traces, and containing albumen, the substance has been present in variable quantities. In one case I found it in urine which contained not a trace of albumen, but so much of this modified bile that the colour given by nitric acid appeared almost black by reflected light; there was also some urea, but the quantity was not determined. In another case I found a large quantity of albumen, with a mere trace of this substance; so that the manifestation of this substance did not seem connected with the appearance either of blood-particles, albumen, or urea. It seemed to pass off much more rapidly than the urea, and speedily disappeared when the urinary secretion was re-established, although the urea did not for some little time reach its normal standard. In the urine, as in the cholera stools, this substance seemed to be nearly destitute of colour till nitric acid was added. If there was any sediment in the urine, either of exudation or pus corpuscles, phosphates, epithelium, casts of tubes—or, at a later date, of uric acid, urate of ammonia, or of soda—this substance gave them no colour. Its alcoholic solution rapidly decomposed, so that in three weeks or a month the characteristic reaction was almost or entirely lost.

I believe this substance to be the same with that present in the cholera stools, for the following reasons:—

1. In both cases the substance is soluble in alcohol.

2. In both a most peculiar violet or pink tint is given by nitric acid to the alcoholic solution.

3. In both an excess of acid destroys the tint, and produces a light yellow colour. In the cholera stools, sometimes a shade of blue can be obtained after the violet; and in the urine sometimes a shade of green precedes the violet or puce colour.

4. In both cases, if albumen be present, and be coagulated by heat and nitric acid, it is tinged of a red or violet colour, unless too much acid be added.

I have not yet succeeded in satisfactorily detecting this substance in the blood. It is possible that it will shortly be found there.

This substance is by no means peculiar to the urine of cholera. I have found it in the urine of various diseases. In Bright's disease occasionally, as noticed by Heller; in some obscure abdominal affections without albuminuria; in a case of abscess of the liver; in one of cholecystitis; in two cases of dysentery (although in the greater

number of cases of dysentery it is not present); and in a complicated case of purpura, rheumatism, and Bright's disease. In these cases the urine was generally clear, pale, acid or neutral, and quite different from very dilute common icteric urine, which gives the ordinary reaction of bile-pigment.

I believe, for reasons which will be more fully considered at another time, that this substance has been described by Heller as a normal urinary colouring matter, under the name of uroxanthin. Uroxanthin, with nitric acid, gives the pink tint, and passes first into urrhodin, and subsequently it passes into the blue uroglaucin. I believe these changes may sometimes be followed in cholera stools, with which not a particle of urine has been mixed. So, also, I believe, that this modified bile has been included by Dr Golding Bird in his description of purpurine, although it does not altogether correspond to this substance. But it would be foreign to the object of this note to pursue the subject further at this time.

ARTICLE IV.—*On the Fœtus in Utero, as inoculating the Maternal with the Peculiarities of the Paternal Organism; and on the Influence thereby exerted by the Male on the Constitution and the reproductive Powers of the Female.* By ALEXANDER HARVEY, M.D., Lecturer on the Practice of Medicine in King's College; and Physician to the Royal Infirmary, Aberdeen.

INSTANCES are sufficiently common among the lower animals, where the offspring exhibit, more or less distinctly, over and beyond the characters of the male by which they were begotten, the peculiarities, also, of a male by which their mother had at some former period been impregnated,—or, as it has been otherwise expressed, where the peculiarities of a male animal that has once had fruitful intercourse with a female, are more or less distinctly recognised in the offspring of subsequent connections of that female with other males.[1] A young chestnut mare, seven-eighths Arabian, belonging to the Earl of Morton, was covered in 1815, by a quagga, which is a species of wild ass from Africa, and marked somewhat after the manner of the zebra. The mare was covered but once by the quagga; and, after a pregnancy of eleven months and four days, gave birth to a hybrid which had distinct marks of the quagga, in the shape of its head, black bars on the legs and shoulders, &c. In 1817, 1818, and 1821, the same mare (which had in the meantime passed into the possession of Sir Gore Ouseley), was covered by a very fine black Arabian horse, and produced, successively, three foals, all of which bore unequivocal marks of the quagga.[2] Several

[1] Alison, Outlines of Physiology, 3d Ed., p. 443.

[2] Philosophical Transactions, 1821, p. 20; Dunglison's Human Physiology, 3d Ed., vol ii., p. 387.

other examples illustrative of the general fact above stated will presently be given.

Great difficulty has been felt by physiological writers in regard to the proper explanation of this kind of phenomena. They have been ascribed by some to a permanent impression made somehow by the semen of the first male on the genitals, and more particularly on the ova, of the female; and by others to an abiding influence exerted by him on the imagination of the female, and operating on her mind at the time of her connection subsequently with other males, and perhaps during her pregnancy. But they seem to be regarded by most physiologists as inexplicable.

Very recently, in a paper published in the "Aberdeen Journal,"[1] an intelligent veterinary surgeon, Mr James M'Gillivray, of Huntly, has offered an explanation, which seems to me to be the true one. His theory is set forth in the following statements quoted from that paper:—"When a pure animal of any breed has been pregnant to an animal of a different breed, such pregnant animal is a cross ever after; *the purity of her blood being lost*, in consequence of her connection with the foreign animal;" and again: "If a cow, say of the pure Aberdeenshire breed, is in calf to a bull of the short-horn breed (known as the Teeswater breed), in proportion as this calf partakes of the nature and physical characters of the bull, just in proportion will the *blood* of the cow become *contaminated*, and herself a cross, for ever incapable of producing a pure calf of any breed." "It is maintained, therefore (Mr M'Gillivray adds), that the great variety of non-descript animals to be met with are the result of the crossing system; the prevailing evil of which is, the admission of bulls of various breeds to the same cow, *whereby the blood is completely vitiated*."

In explanation of his theory, Mr M'Gillivray enters into particulars as to the nature of the connection subsisting between the fœtus in utero and its mother, with the view of showing (what seems to him essential to the validity of the theory) that there is a *direct* vascular communication between the two; and that, while a portion of the mother's blood is continually passing by direct transmission into the body of the fœtus, the latter returns to the former so much of that blood as is not needed by it, and that this superfluous blood, after circulating through the system of the fœtus, passes as directly into the system of the mother, and, commingling with the rest of her blood, *destroys its purity, contaminates, vitiates it.*

Mr M'Gillivray is quite wrong, I apprehend, in assuming that there is, in any case, a direct vascular connection between the fœtus and its mother. Nor does the assumption appear to me at all necessary to establish the theory. But waiving, for the present, all discussion of that point, it may here be observed that Mr M'Gillivray regards the influence exerted by the male on the female ani-

[1] March 21 and 28, 1849.

mal, through the medium of the fœtus, as *constitutional;* and perhaps the best general expression of the theory is, that the fœtus, partaking, as it must, of the characters or peculiarities of its father, *inoculates* therewith the blood, and, generally, the system, of its mother.

The subject now opened up is certainly one of great interest in general physiology, as well as of considerable practical importance to breeders. It cannot but be interesting to inquire whether the fact instanced in Lord Morton's mare, is or is not a general law in animal physiology; and, if it be, whether and how far it is modified, in its operation, in different animals, and under different circumstances. But to the human physiologist, and to the physician, it is of more immediate interest to inquire whether or not the fact extends also to his own species; and, if it does, to ascertain how far it applies, and whether it does not admit of illustration by, and serve itself, in its turn, to illustrate certain known facts in regard to the communication and the constitutional effects of the syphilitic and other morbific poisons, the scrofulous diathesis, &c. And, in particular, it can hardly fail to suggest some such curious questions as the following, viz. :—

1st, Whether, in the case of a woman who has been twice married, and borne children to both husbands, the children borne to the second husband ever, or generally, partake of the peculiarities of the first husband.

2nd, Whether, in a family of several children, the younger children rather than the elder, are disposed, *cæteris paribus*, to exhibit the characters of the father.

3d, Whether a woman who has borne several children by the same husband, may not ultimately acquire some of the physical characters, or at least imbibe and manifest some of the morbid tendencies, of the latter.

In treating farther of this singular subject, I shall *first* state the facts at present known to me regarding it; and, *secondly*, consider the theories offered in explanation of it.

I. In regard to the facts of the subject, these will be most conveniently noticed, *first*, in relation to the lower animals; and, *secondly*, in relation to the human species.

(1.) As regards the brute creation :—Besides the instance already quoted of the mare belonging to Lord Morton, there is another similar case recorded. A mare belonging to Sir Gore Ouseley, was covered by a zebra, and gave birth to a striped hybrid. The year following, the same mare was covered by a thorough-bred horse, and the next succeeding year by another horse. Both the *foals* thus produced were striped, *i.e.*, partook of the characters of the zebra.[1]

[1] M'Gillivray, "Aberdeen Journal," March 28, 1849. Paintings of these animals and their skins are said to be preserved in the Museum of the Royal College of Surgeons of England.

And it is stated by Haller, and also by Becker, that when a mare has had a *mule* by an ass, and afterwards a *foal* by a horse, the foal exhibits traces of the ass.[1]

In the foregoing cases, the mares were covered, in the first instance, by animals of a different species from themselves. But cases are recorded of mares covered in every instance by horses, but by different horses, on different occasions—where the offspring partook of the characters of the horse, by which impregnation was first effected. Of this Mr M'Gillivray gives two examples. Thus, in several foals, in the Royal stud at Hampton Court, got by the horse *Actæon*, there were unequivocal marks of the horse *Colonel*,—the dams of these foals were bred from by Colonel the previous year. Again, a colt, the property of the Earl of Suffield, got by *Laurel*, so resembled another horse *Camel*, "that it was whispered, nay, even asserted, at New-Market, that he must have been got by Camel." It was ascertained, however, that the mother of the colt was covered, the previous year, by Camel.

It has often been observed, also, that a well-bred bitch, if she have been impregnated by a mongrel-dog, will not, although lined subsequently by a pure dog, bear thorough-bred puppies in the next two or three litters.[2]

The like occurrence has been noticed in respect of the sow. A sow of the black and white breed (known as Mr Western's breed) became pregnant by a boar of the wild breed, of a deep chestnut colour. The pigs produced were duly mixed, the colour of the boar being in some very predominant. The sow being afterwards put to a boar of the same breed with her own, some of the produce were observed to be stained or marked with the chestnut colour that prevailed in the former litter. And, on a subsequent impregnation, the boar being still of the same breed as the sow, some of the litter were also slightly marked with the chestnut colour. What adds to the value of the fact now stated is, that in the course of many years' observation, the breed in question was never known to produce progeny having the smallest tinge of the chestnut colour.[3]

Breeders of cattle are familiar with analogous facts as occurring in the cow. A pure Aberdeenshire heifer was served with a pure Teeswater bull, to whom she had a *first-cross* calf. The following season, the same cow was served with a pure Aberdeenshire bull;

[1] Haller, Element. Physiol., viii., p. 104, ; Becker, Physic. Subterran. Lips., 1703. Quoted from Dunglison's Physiology, vol. ii., p. 387.

[2] Kirkes' Handbook of Physiology, p. 613.

[3] Philosophical Transactions for 1821, p. 23. "Apart from a state of domestication," says Mr M'Gillivray, "I do not believe that there is one solitary instance in which an animal has produced offspring of various colours. Animals, left entirely to the operation of natural causes, never exhibit this sporting of colours; they are to be distinguished by various and often beautiful shades of colour; but then each species is true to its own family type, even to a few hairs or small parts of a feather."

the produce was a *cross* calf, which at two years old had very long horns, the parents both hummel.[1] A pure Aberdeenshire cow was served, in 1845, with a cross bull—*i. e.*, an animal produced between a first-cross cow and a pure Teeswater bull. To this bull she had a cross calf. Next season she was served with a pure Aberdeenshire bull,—the calf was quite a *cross* in shape and colour.[2]

Mr M'Gillivray, after narrating the whole of the foregoing examples, says:—"Many more instances might be cited, did time permit. *Among cattle and horses they are of every day occurrence.*"

(2.) As regards the human species. The facts bearing on this division of the subject are exceedingly few, and not to be relied on; and the observations which follow are intended rather to suggest and direct, than to satisfy, inquiry.

Dr Allen Thomson, in his article on generation, in the "Cyclopædia of Anatomy and Physiology," remarks:—"It is affirmed that the human female, when twice married, bears occasionally to the second husband, children resembling the first, both in bodily structure and mental powers." And Dr George Ogilvie, of this city, informs me of a case, which fell under his own observation, where a woman was twice married, and had children by both husbands, and where the children by both marriages were scrofulous, although only the first husband had marks of that diathesis; the woman herself, and her second husband, being to all appearance quite healthy.

Dr Ogilvie's case is clearly beset by so many sources of fallacy, that we cannot venture at present to regard it as a case in point. Dr Thomson does not bring forward any instances, nor offer any proof, in support of his statement; and, indeed, he gives it, without saying whether he thinks it well or ill-founded. Any such statement, it is plain, based on observation of the children of European parents—*i. e.*, where the female and both her husbands and their children are all white—must be comparatively difficult of verification; but it is equally plain that means exist for subjecting it to a pretty decisive test. There are equally distinct breeds of the human family as of any of the lower animals; and all that seems requisite in regard to determining the question under consideration is, to observe accurately, whether the children of European parents, where the woman has, in the first instance, had offspring by a negro, exhibit traces of the latter in the colour of the skin, the form of the features, &c.; or, *vice versâ*, whether the children of negro parents, where the woman had, first of all, been impregnated by a European, exhibit the peculiarities of the latter. Of the former case, a medical friend informs me that he recollects hearing of an instance of the kind as occurring in this neighbourhood, but cannot vouch for the truth of it. Of the latter case, if

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[1] M'Gillivray, loc. cit. [2] Ibid.

the general fact applies to the human species, instances must abound in our West India colonies, in the United States of America, and in other parts of the world. My colleague Dr Dyce tells me, that he has certainly known one instance (if not more) where a creole woman bore fair children to a white man; and that the same woman had afterwards to a creole man other children, who bore much resemblance to the white man, both in features and in complexion. But two very intelligent friends—the one a West India proprietor, the other a medical man—both long resident in Jamaica, tell me that they never noticed, nor ever heard, of an instance of the kind, although connections of that sort are common there, and children born under such circumstances very numerous. It is singular, indeed, if instances of the fact in question do occur, and still more if they are of frequent occurrence, that they should not be notorious. It is conceivable, however, and by no means improbable, that cases do exist, but that they have been overlooked from the traces of the European being so minute as to escape ordinary observation, and the fact have remained unknown, from special attention never having been directed to it.

If the male does exert any such influence as is here in question on the constitution and the reproductive powers of the female, it is conceivable that, by each successive impregnation effected by him, that influence may be increased; and, if so, the younger children begotten by him, rather than the elder, might be expected, *cæteris paribus*, to bear their father's image. And this more special fact, if ascertained, would establish also the more general one. I am not aware, however, of any specific facts bearing upon it, nor of any popular notions regarding it. But my colleague Dr Laing is cognisant of the case of an English gentleman who had a large family by a negro woman, in the West Indies, and where the children successively exhibited more and more the European features and complexion.

But, however this may be, there is a popular belief that, in the course of years, a woman comes to resemble her husband, and that not merely in respect of temper, disposition, or habits of thought, but in bodily appearance. But, in so far as the notion may hold good, it may be true only of the features, and of these only as they indicate or bespeak the inward feelings of the mind, which, from long and familiar intercourse, may, to a certain extent, become common to man and wife. In so far as the notion is true in any other respect, and the parties have had several children, it may suggest the question, whether the assimilation is not referable to an influence exerted by the husband, through the medium of the fœtus in utero, on the constitution of the wife? The question is probably an idle one, and the notion only a popular error. In so far, however, as there is anything in it, the explanation suggested gives a peculiar, and it may be added, a physiological significancy,

to the language of Scripture relative to man and wife, at least when their intercourse has been fruitful—"They twain shall be *one flesh*."

It is of more immediate interest, however, and of greater practical moment, to ascertain whether, through the medium of the fœtus, the husband may impart to his wife either the syphilitic virus, or the scrofulous diathesis, or any other constitutional morbid tendency (*e. g.* insanity) which he may possess. Facts are wanting on this subject; but it is not undeserving of patient inquiry. Dr Ogilvie's case, formerly referred to, if it could be relied on, would be an instance of it. Before the mother could have imparted the scrofulous taint to her offspring by the second husband, she must herself have imbibed it from her first husband through the medium of his offspring while in utero. And, although still seemingly free of the taint, it may have required only the appropriate external conditions to call it into full activity in her own person. And, with regard to the syphilitic poison, there is no difficulty in understanding, and it is quite within the bounds of probability, that the fœtus, if contaminated with it by its father, may convey it to the mother. Messrs Maunsell and Evanson, after mentioning that they have notes of the case of a syphilitic child, whose mother had been infected by a former husband (they do not say in what way)—and to all appearance cured five years before its birth—the father of the child (her second husband) being in good health, state that their experience would enable them to adduce many curious facts bearing on the communication of the syphilitic poison.[1] Perhaps their experience might furnish an affirmative solution of the question at issue. It has been affirmed, indeed, that a man who has once had syphilis, but been seemingly cured of it for many years, may yet so retain the taint of it as to contaminate his offspring, without, at the same time, tainting his wife. Very possibly. But this does not prove that he may not contaminate his wife also; and the observation itself is in that respect fallacious, inasmuch as, in any given case of the kind, the wife may really have imbibed the virus, although in a latent form, and might subsequently give proof of the reality of the fact by tainting the offspring begotten by another and a perfectly healthy husband. Adopting this view, it may be found of importance, in contemplating marriage with a widow, to inquire into the constitutional peculiarities of her deceased husband!

II. Of the general fact now under consideration, and clearly established in respect of the lower animals, only two explanations, that are at all rational, have been offered. The first is that suggested by the great Haller, who ascribes it to a permanent impression made by the semen of the male on the genitals, and more particularly on the ova, of the female; the second, that suggested

[1] On the Management and Diseases of Children, 5th edit. pp. 452, 453.

by Mr M'Gillivray, who ascribes it to an influence exerted by the fœtus in utero on the constitution of the mother. The notion entertained by Sir Everard Home and others, that it is an affair of the imagination, seems too absurd to require serious consideration.

Haller's knowledge of the subject appears to have been very limited, and his explanation of it to have been offered incidentally. He was aware that when a mare has had a mule by an ass, and afterwards a foal by a horse, the foal exhibits traces of the ass; and he remarks, "that the female organs of the mare seem to be corrupted by the unequal copulation with the ass,"[1] *i. e.* that the semen of the latter exerts an influence on the genitals, and, of course, on the ova of the mare, which appears subsequently on the impregnation of these ova by males of her own species.[2]

It may be stated, in support of Haller's theory, that, in the case of birds, a single intercourse is known to impregnate many eggs which are laid successively after it; but, on the other hand, the influence of such intercourse extends only to the eggs of one season, or rather of one brood,—the several eggs being laid in tolerably quick succession, and all of them probably in a state of maturity, and actually impregnated at the time of that intercourse. This fact, therefore, goes but a short way to favour Haller's theory, and may, indeed, be said to tell as much against as for it. And if it shall be clearly ascertained (as seems presently to be the belief of physiologists), that any single ovum remains but a short time in the ovary, Haller's theory must be given up. But even if it could be shown that an ovum may remain in the ovary for a series of years, the fact would be of little value, unless it could also be shown that the semen can exert some definite kind of influence on an ovum, which it does not at the time actually impregnate. There seems little probability, however, of this being done; and there is one fact known in regard to the ova which makes it difficult to conceive it possible—the fact, namely, that unripe or immature ova lie deeply imbedded in the stroma of the ovary.

Mr M'Gillivray's theory seems to me to meet the whole facts of the case, and to derive support from a great variety of facts in regard to the reception and constitutional effects of morbific poisons and morbid diatheses.

Mr M'Gillivray, indeed, supposes, as was formerly noticed, that there is a *direct* vascular connection between the fœtus in utero and its mother; and he seems to consider the validity of his theory to hinge on this assumption. The assumption, however, is untenable,

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[1] Dunglison's Physiology, vol. ii. p. 387.

[2] Dr Kirkes also appears to regard the cause as a local one. Referring to Lord Morton's case, he observes,—"The single impregnation, by the seminal fluid of the quagga, had impressed its character not only on the ovum then impregnated, but on the three following ova impregnated by horses."—Handbook of Physiology, p. 614. Such, too, is Mr Mayo's view. Physiology, 2d Ed., p. 490.

nor is it at all necessary for the establishment of the theory. The researches of Dr John Reid and of Mr Goodsir, on the structure of the placenta, have demonstrated that the connection is *indirect* only —the fœtus and the mother imbibing materials from each other, very much in the same way that the lacteal vessels take up the nutritive portions of the food in its transit along the small intestines; or that the roots and leaves of vegetables take up nourishment from the soil and the atmosphere,—the materials imbibed, in each case, passing through a pervious, but not a perforated, tube or membrane, and being taken up by a real act of *absorption*, during which act they are more or less altered in their character, or *assimilated*. But, independently of the considerations now stated, it appears from the observations of Prevost and Dumas, and of others, that the corpuscles of the fœtal blood are differently shaped from, and, in the later stages, larger than those of the mother[1]—a fact which shows, at least, that no entire corpuscles of blood are transmitted from the one to the other, and, indeed, taken in connection with the facts ascertained as to the structure of the placenta, proves that it is by *transudation* only, that the contents of the uterine and fœtal vessels mutually pass into each other.

In so doing, the materials in question are more or less altered in their character, or undergo what physiologists term a process of *assimilation*. In the case of the lacteal vessels, the chyle which they contain can never be detected *as such* in the alimentary mass; nor is the sap of vegetables precisely the same fluid that exists in the soil and in the air. In like manner, the blood in the umbilical vessels, doubtless differs from that existing in the uterine sinuses. At the same time, the assimilating process does not go the length probably in any case of wholly changing the character of the fluids concerned in it; and there is reason to believe, that, in different cases, it proceeds to a very different extent—in some the change effected being to a less extent than in others. And possibly, in the case of the fœtus and its mother, the amount of the assimilation is not considerable. No interchange of corpuscles takes place, but in respect of the other constituents of the blood, it is difficult to conceive why they should not be transmitted nearly unchanged. Professor Simpson, of Edinburgh, has recently shown that the small-pox virus may pass unaltered from the mother to the child in her womb, and produce in it the actual disease, even although, by reason of previous vaccination, the mother may herself remain unaffected by it.[2] And a similar fact has long been known in regard to the transmission of the syphilitic virus from the mother to the fœtus in utero.

We can, therefore, have no difficulty in understanding, in respect

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[1] Alison, Outlines of Physiology, 3d edit. p. 426.—Kirkes' Handbook, pp. 66, 7.

[2] Edinburgh Monthly Journal of Medical Science for April 1849.

of the fœtus itself, that, although its connection with the mother is indirect only and merely to the extent of allowing the passage of the *liquor sanguinis*, and although this may even be so far altered in the passage, the constitutional peculiarities, derived to it from its father, and inherent in its blood, may, with the blood, be imbibed by its mother. And when we reflect on the length of time during which the connection between them is kept up, the amount and the activity of interstitial change continually going on in the system of the fœtus, the large quantity of fœtal blood that must eventually be taken into the vessels of the mother, and the probability that the peculiar matter imparted by the male parent to the ovum at the moment of impregnation (be its nature what it may, and its quantity never so infinitesimal), assimilates, like a ferment, much of the fœtal blood to itself, it does not seem too hard to be believed that the blood and constitution generally of the mother may, thereby, become so imbued with the peculiarities of that parent, as to impart them to any offspring she may subsequently have by other males.

Aberdeen, April 30, 1849.

APPENDIX.

I. In the foregoing paper a question occurs as to whether, in the case of a woman who has been twice married, and borne children by both husbands, the children of the second marriage ever resemble the mother's first husband (p. 1132).

The following additional cases, illustrative of this question, have recently been communicated to me: the first by my friend the Rev. Charles M'Combie, of Tillyfour, minister of Lumphanan, in Aberdeenshire; the second by Professor Simpson, of Edinburgh; and the third by Professor Pirrie, of Aberdeen:—

1. Mrs ——, a neighbour of Mr M'Combie, was twice married, and had issue by both husbands. The children of the first marriage were five in number; of the second, three. One of these three, a daughter, bears an unmistakeable resemblance to her mother's first husband. What makes the likeness the more discernible is, that there was the most marked difference, in their features and general appearance, between the two husbands.

2. A young woman, residing in Edinburgh, and born of white (Scottish) parents, but whose mother sometime previous to her marriage had a natural (mulatto) child, by a negro man-servant, in Edinburgh, exhibits distinct traces of the negro. Dr Simpson, whose patient the young woman at one time was, has had no recent opportunities of satisfying himself as to the precise extent to which the negro character prevails in her features; but he recollects being struck with the resemblance, and noticed particularly that the hair had the qualities characteristic of the negro.

3. Mrs H——, apparently perfectly free from scrofula, married a man who died of phthisis. She had one child by him, which also died of phthisis. She next married a person who was to all appearance equally healthy as herself, and had two children by him, one of which died of phthisis, the other of tubercular mesenteric disease—having, at the same time, scrofulous ulceration of the under extremity.

II. In connection with the constitutional influence exerted by the male, through the medium of the fœtus in utero, on the system of the female, another and a very singular question may be raised. In the case of an aboriginal woman of colour, does previous impregnation by an European male render the female incapable ever after of fruitful intercourse with a male of her own race?

This question is suggested by an observation, made in various parts of the world, by the excellent Count de Strzelecki, relative to the effect of fruitful intercourse between an aboriginal female and an European male. "*Whenever such intercourse takes place*," says the Count, "*the native female is found to lose the power of conception on a renewal of intercourse with the male of her own race, retaining only that of procreating with the white men.*" [1]

This, if a general fact, contrasts remarkably with Dr Simpson's case, above-mentioned (one of fruitful connection between a white man and a white woman, after previous impregnation of the latter by a black man), unless, indeed, this be, which probably it is not, an exception to an equally general fact of the same sort. It would limit, also,—nay, absolutely exclude, opportunities of observing whether children born of dark parents, where the mother formerly had issue by a European male, exhibit traces of the latter. But it was before stated (p. 1135), on the authority of two gentlemen long resident in Jamaica, that in our West India colonies—in Jamaica, at least,—fruitful connections of this kind are of common occurrence, and (which I mention at present as in keeping with this) on the authority of Dr Dyce, that, in children born under such circumstances, marks of the European have been observed. Special inquiry, made recently, has served so far to confirm these statements, but not to satisfy me that the issue of such connections is numerous.

The opportunities, however, enjoyed by the Count de Strzelecki, of making observations as to this point, in most parts of the new world, have been very great. "He has lived much (to use his own words) amongst different races of aborigines,—the natives of Canada, of the United States, of California, Mexico, the South American Republics, the Marquesas, Sandwich, and Society Islands, and those of New Zealand and Australia." [2] And, referring to the statement made by him, and already quoted, the Count observes—"*Hundreds of instances* of this extraordinary fact are on record in the writer's memoranda, *all recurring invariably under the same circumstances*, amongst the Hurons, Seminoles, Red Indians, Yakies (Sinaloa), Mendosa Indians, Araucos, South Sea Islanders, and natives of New Zealand, New South Wales, and Van Dieman's Land; and all tending to prove that the sterility of the female, which is relative only to one and not to another male, is not accidental, but

[1] Physical Description of New South Wales and Van Dieman's Land, p. 347.
[2] Op. cit. p. 345.

follows laws as cogent, though as mysterious, as the rest of those connected with generation." [1]

Strzelecki does not state to what extent, or indeed whether, he has met with exceptional cases, *i. e.*, cases where, after connection of the kind in question, fruitful intercourse has taken place between a native man and woman. This it would be important to know. It seems not improbable, at least, that such cases may have been observed by him. They would not, indeed, even were they numerous, invalidate the inference obviously drawn by him from his other observations, provided they were really exceptional. They would merely show that the fact does not hold universally or absolutely. But should the inference be in the meantime disputed, as I think it well may, it can only be determined, in the affirmative, by proofs of the same general kind with those by which (for example) the contagious property of certain diseases is established,—to wit, by comparative observations on the large scale, showing,—*first*, that native females who have once had fruitful connection with European males, are, subsequently, as compared with other native females who have had no such connection, much less fruitful with males of their own race; and, *secondly*, that no other common circumstance, save that of such connection, can be ascertained to exist in the case of most of the women that become barren, and not to exist in the case of most of those that are fruitful.[2]

If future inquiry thus directed shall verify the inference, which at present can only be regarded as an hypothesis, it will establish a general principle in the physiology of generation of the highest interest and importance. Assuming, however, that it is well-founded, and will hereafter be proved to be a fact, it were vain, perhaps, in the present state of our knowledge, to attempt an explanation of it; but, with regard to its bearing on the subject of this paper, I am inclined to think, from the facts already adduced in these pages—uncertain as some of them may be—that the sterility is quite as likely to be owing to the *system* of the female being somehow altered or affected, during her pregnancy, by the fœtus begotten by the European, as to a *local* change in the general mass of ova effected by the mere act of intercourse, *i. e.*, the mere application to them of the semen of the European. Our knowledge, indeed, of the conditions essential to impregnation, and of the mode in which it is accomplished, does not warrant us to say that the change cannot be exclusively local. But the facts ascertained of late years, as to immature ova occupying the *centre* of the ovary,—as to the ova undergoing a process of maturation and coming to the surface of the ovary, prior to, and in order to, impregnation,—and as to the mature ova passing off at each monthly period, and becoming blighted when not impregnated, tend rather

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[1] Op. cit. p. 347.

[2] See Alison's Outlines of Pathology and Practice of Medicine, p. 67.

to set aside the notion of local affection, and therefore to give probability to the other view. And if this other view should be established, the fact itself would clearly furnish additional testimony to the doctrine, and would merge in the still more general fact, that the fœtus in utero does *inoculate* the system of the female with the peculiarities of that of the male.

The alleged relative sterility of the native female, after intercourse with the European male, is brought forward (it may be added) by Strzelecki as affording an explanation, and as being the chief cause, of the gradual diminution and ultimate extinction of the native tribes in most parts of the new world, which follow the introduction of the European races. "Wherever the white man has set his foot-mark, there the print of the native foot is obliterated; and as the tender plant withers beneath his tread, so withers the aboriginal inhabitant of the soil."[1] And "human interference," says Strzelecki, "to avert this melancholy consequence has been hitherto of no avail;—a charter for colonisation granted to one race becomes virtually the decree for the extinction of the other."[2]

Very various causes, doubtless, concur to bring about this result. The one assigned by Strzelecki as the chief is obviously quite adequate, if a real one, to its production. And should his belief as to the reality of this cause be confirmed, and if it shall farther appear, that the principle involved in it applies only to aboriginal females contaminated by European males, and not to European females contaminated by aboriginal males, *i. e.*, should the former class of females only, and not the latter, be rendered sterile to males of their own race by such foreign intercourse, the discovery can scarcely fail, not merely to exhibit the predominancy of the white over the dark races of men, in a particular not previously suspected, but to indicate that the designs of Providence, in regard to the human family in this stage of existence, embrace the ultimate extinction of the primitive varieties of the dark races. Their physical peculiarities and their social degradation—a mystery, if not a standing memorial of a curse visited on their progenitors, in the times of miraculous interposition; the purpose of their existence in respect of this earth—a mystery also, yet somehow subservient, seemingly, to that of their more favoured *brethren;*—their end, after that purpose

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[1] Brooke's Narrative of Events in Borneo and Celebes, vol. i. p. 12.

[2] Strzelecki records the following remarkable circumstance which came within his own personal knowledge:—A party of aborigines in Van Dieman's Land, to the number of 210, were deported by government in 1835 to Flinder's Island, on account of aggressions made by them on the colonists in their neighbourhood, by whom, however, they had been contaminated. They had only 14 children born among them during the next seven years. It is true, that, in the course of that time, their own numbers had dwindled away to 54. Still the small number of births is singular, and contrasts strikingly with the fact, that "each family, in the interior of New South Wales, uncontaminated by contact with the whites, *swarms with children.*" (Op. cit. pp. 350-5.)

is served—extirpation? But these are questions which, besides that they are foreign to the object of this paper, are, perhaps, too deep for human penetration.

July, 1849.

ARTICLE V.—*On the Anatomy and Physiology of the Oblique Muscles of the Eye, in Man and Vertebrate Animals.* By JOHN STRUTHERS, F.R.C.S., Lecturer on Anatomy, Edinburgh.

NEARLY ninety years ago it was remarked by Porterfield, that "though the action of these two oblique muscles seems pretty evident, yet there is scarce any part of the human body about which anatomists have differed more than in assigning them their proper offices": and the observation is still appropriate, although the subject has since engaged the attention of various anatomists.

The question does not admit of being decided by experiments on the lower animals, chiefly because mammals are provided with an additional muscle, or series of muscles, for retracting or suspending the eye, which are beyond the reach of trustworthy experiment, and to which motions of the eyeball might be owing after the straight muscles had been divided. Accordingly, the opinions of most authors on this matter are founded upon a consideration of the manner in which it appeared to them that these muscles, from their direction and attachments, would influence the ball of the eye, so as to change the direction of its axis; and very different conclusions have been arrived at by the most eminent of anatomists and physiologists.

A necessary preliminary to such an inquiry, is a correct understanding as to the manner in which the eyeball actually moves. It appears to me that all the movements of the eyeball are purely rotatory in their nature, as if it moved within a close-fitting socket, or, as it were, on an universal pivot at its centre. The axis on which the rotatory motions occur will always be at right angles to the moving power—transverse when the upper and lower recti act, vertical during the action of the lateral recti, and intermediate in the diagonal movements, which are performed by the conjoint action of two neighbouring recti muscles. Again, in the motion of circumduction, as in turning the eyes round a room, which is performed by the alternate action of the four straight muscles, the centre, as it were, still remains unmoved, one end of the axis being depressed as the other is raised, and so on. In accordance with this we find the straight muscles sweeping over the globe of the eye, to become inserted considerably anterior to its middle, so as to fit them for producing these various motions; by all of which, it will be observed, the direction of its axis is changed. But it is held by some, that, independent of any change in the direction of its axis, the eye, under

certain circumstances, undergoes a movement quite different in its nature—that of rotation on its antero-posterior axis, or what is usually called the axis of the eye,—and that it is the function of the oblique muscles to produce this movement: which I shall call *lateral rotation.*

It appeared to me that light might be thrown on this disputed subject by the evidence which comparative anatomy would no doubt afford; for, although the existence of corresponding muscles has been noticed in passing, by various writers on comparative anatomy, the arrangement of them with reference to their action on the eye does not appear to have been inquired into; and, having made careful dissections of these parts, in a great variety of instances, I am enabled to furnish, from my notes, the following summary of the arrangement of the oblique muscles in the different classes of vertebrate animals,—avoiding notice of the comparative anatomy of the other muscles of the eye, as in no way affecting the present inquiry.

In MAMMALIA, the *superior oblique* has the same general arrangement as in man. The differences may be stated thus:—1, That it is usually fleshy for some distance beyond the pulley, and therefore longer, at the same time that it is relatively larger. 2, It is usually not, as in man, directed backwards to the axis of the eye, but meets it nearly at right angles; and its course from the pulley to the eye is more horizontal than in man: and 3, That its point of insertion is not so far towards the outer and back part of the eye as in man, but is usually only a little external to the vertical axis, and not more behind than in front of it. The insertion varies slightly in different animals, but in the greater number the insertion of the tendon is chiefly concealed by the upper rectus, the posterior border only appearing beyond it; whereas, in man, the tendon passes some way beyond, and also behind the vertical axis of the eye. The *inferior oblique*, also arising as in man, is not, as in him, directed backwards to the axis, but either forwards or at right angles to it. But the most striking difference is in the insertion. This in man is towards the posterior part of the eye, but in quadrupeds it is much farther forwards, as well as lower down. Usually it is attached between the lower and outer recti—its posterior border often reaching a little beneath the latter—and to the anterior third of the sclerotica, in some coming quite close to the margin of the cornea.

In BIRDS, REPTILES, and FISHES, the *superior* oblique arises from the fore part of the orbit, and is precisely the counterpart of the *inferior*, which is disposed much as in the mammalia; and there is so little difference in their arrangement in these different classes of vertebrate animals that they scarcely require separate description in each. The two muscles arise together from the fore part of the inner, or anterior, wall of the orbit. Passing outwards, they soon separate, the superior to be inserted near to the tendon of the upper rectus, the inferior to a corresponding point below; their direction to the axis of the eye being at right angles, or more usually

somewhat forwards. In *birds*, the superior oblique is much the largest muscle of the eye, being two or three times the size of the inferior, which is not much larger than one of the recti. It is broadest at the insertion, which is partly beneath the tendon of the superior rectus, reaching near, and sometimes up to, the outer border of that tendon; but the greater part is attached, on its inner side, at the upper part of the space between it and the internal rectus. The *inferior* oblique is inserted partly in front of the tendon of the lower rectus, and partly to its inner side, and is nearer to the cornea, or osseous zone, than the superior. The space between the eye and the two oblique muscles is occupied by a large branch of the fifth nerve and part of the Harderian gland, which supports the eye in its lower and inner aspects. In *reptiles* they seem not to pass so far outwards. In the frog more particularly, the superior is attached to the inside of the tendon of the upper rectus, and is not larger than the inferior; and the two muscles meet the axis of the eye at right angles. But in *fishes* they pass further outwards—the superior in front of and over the tendon of the upper rectus, to be inserted partly in front of it, and partly to its outer side; and the inferior has a corresponding position and attachment below. They are directed somewhat forwards to the axis of the eye.

In inferring the action of these muscles from their anatomy, it is essential to notice their direction with reference to the axis, and also whether they are attached before, behind, or at the middle of the eye. In man they are attached behind the middle, especially the inferior, and their direction is backwards to the axis. In most mammals the superior oblique is not nearer to the front than the posterior part of the eye, but the inferior is rather nearer to the former, and both the muscles are of great breadth. In man the axes of the eyes are directed almost straightforward, but as we descend the scale of vertebrate animals, they become more and more turned outwards. Among mammals the direction of the axes varies considerably. When they look much forwards—as, for instance, in the cat—the line of action of the oblique muscles is directed backwards, as in man. When the axes are turned quite outwards—as in the rabbit and hare, in which the axes of both eyes cannot possibly be directed to the same object—the oblique muscles are then directed forwards to the axis; but in most quadrupeds,—in which the eyes look forwards and outwards, so that if both eyes are used on one object the visual axis cannot correspond to the axis of the eye,—they may be said to meet the axis at a right angle, as well as to embrace the middle of the eye. In birds, most reptiles, and in fishes, the eyes are placed so that the rays from one object cannot enter both pupils, and from this outward direction of the eyes the oblique muscles are directed somewhat forwards to the axis, although often nearly at right angles. Also they are usually, in these classes, attached rather in front of the middle of the eye, although from their great breadth they embrace a considerable part of it. Whilst I have described

their direction in these classes, and in some mammals, as being rather forwards to the axes of the eyes, their actual direction in the orbits is backwards when the opposite sides are considered together; but in most mammals their course is nearly horizontal, and in some even a little forwards in the orbits, at the same time that they are nearly at right angles to the axes of the eyes; so that one of their uses cannot be to advance the eyes, as has been stated.

As to their comparative development. In the two lower classes of the vertebrata, the two oblique muscles are of nearly equal size, the inferior is, at least, not smaller than the superior, and they are larger than the straight muscles. In birds, while the inferior continues somewhat larger than one of the recti, the superior has undergone a great increase, being, as already stated, two or three times larger or broader than the inferior. Lastly, in mammals, the superior oblique undergoes a further development, at least in length, by its reflection over a pulley, after arising backwards from near the apex of the orbit. Notwithstanding this, it does not seem to be the more powerful of the two oblique muscles, as the inferior appears to contain more muscular substance, from its being broader and fleshy nearer to its insertion, and is besides situated so as to be capable of effecting a more extensive motion, at least of a lateral rotatory kind, than the superior. This is more the case in man than most quadrupeds, in which the superior has the advantage of being fleshy beyond the pulley, which is then large and loose, so as not to confine the muscular fibres. The oblique muscles appear, also, to be in most quadrupeds more developed than in man, when compared with the straight muscles and with the size of the eye; but, on the other hand, in man the oblique muscles pass more round the eye, from which they would appear to gain an advantage, as regards turning the eye on its axis. Farther, the lesser development of the oblique muscles, compared with the recti, in mammals, than in the inferior classes, may perhaps be more correctly regarded as due to the increase of the recti in the former, in accordance with the greater frequency and extent of the movements of the eye, more especially in man, and also in other mammalia.

These observations may be regarded as affording just grounds from which to draw conclusions as to the use of the oblique muscles. Whatever minor differences they may present, they are evidently the same muscles, and provided for the same purpose in all vertebrate animals, and no hypothesis can be entertained if it is incompatible with, or not supported by, comparative anatomy.

It has been a common opinion that the oblique muscles advance the eye, and act as antagonists to the recti, so as also to steady the eye during the action of the latter. To this it has been objected, that they exist in animals where the eye cannot be retracted, and that they are not proportionally increased where a special retractor or suspensory muscle exists. To this it may be added, that they

exist in man in whom retraction of the eye does not naturally occur as a voluntary movement, as is easily ascertained by experiment, although, by the conjoint action of all the straight muscles, or of two opposite ones, such a movement would be produced; and in most mammals the course of the oblique muscles in the orbit is not sufficiently backwards to permit of their advancing the eye, which is probably accomplished by the resilience of the soft parts, against which it has been drawn by the action of the suspensory muscle.

It has also been held, that one use, at least, of the oblique muscles is to compress the eye laterally, so as—by rendering the cornea more convex, and by increasing the distance of the lens from the retina—to fit the eye for the vision of near objects. But, as this could be only a secondary use of these muscles, it may be observed, that, although both eyes usually converge equally when near objects are viewed, the effect would be liable to be produced unequally, and at times when it was not required. Besides, this hypothesis is now rendered unnecessary, the question, as to how otherwise the optical change is effected, having been satisfactorily settled by the recent microscopical observations of Mr Bowman, which confirm the conjecture long since made by Porterfield, that the ciliary circle or ligament is muscular or contractile in its nature, and can thus readily advance the lens, by acting on it through the ciliary processes and its suspensory ligament.

Again, it has been supposed that the oblique muscles might act so as to turn the cornea inwards. It is not easy to see how, singly or combined, they could perform a rotatory movement of this nature, as this would require them to be attached considerably before the centre of the eye, and the internal rectus does not appear to stand more in need of assistance than any of its fellows. This opinion, however, has been brought by some to explain why the power of turning the eye inwards may remain after the internal rectus is divided, or believed to be so, in the operation for convergent strabismus; whilst others explain the same circumstance by supposing that the inner fibres of the upper and lower recti can act so as to invert the eye. Even those who believe that the power of turning the eye inwards may not be destroyed by division of the internal rectus, will admit that such is at least not usually the case, from which it may be inferred, that there is no muscle, or combination of muscles, by which inversion of the eye can be naturally performed after the internal rectus is divided; otherwise there is no reason why the power of inversion should not always remain. This I have never yet observed in any case where I felt satisfied that the muscle was wholly divided. When any power of inversion remains, I regard it as indicating the necessity of searching for some part of the muscle which has escaped division, and I have not yet found it necessary to divide any other muscle, in whole or in part, in addition to the internal rectus, for the complete removal of convergent strabismus. The power of inversion

returns, though to a lesser, but still sufficient, extent, after the muscle or tendon has re-adhered to the sclerotica.

The view most commonly entertained is, that each of the oblique muscles performs, or assists in performing, one or more of the diagonal movements of the eye, whereby the direction of its axis is changed. Acting singly, the superior has been variously held to turn the cornea downwards and inwards, and, again, downwards and outwards; and the inferior upwards and outwards, and, again, upwards and inwards. The names of the most eminent of anatomists and physiologists are attached to these opposite views, although the more strongly supported view is, that each performs the latter of the two movements above assigned to it. Assuming the latter view to be correct,—namely, that by the superior the cornea is directed downwards and outwards, and upwards and inwards by the inferior,—Valentin has founded on it an ingenious theory, chiefly intended to give an explanation of the singular arrangement of the motor nerves of the orbit. But, irrespective of a variety of objections of another nature, which might be taken to this theory, it is enough to remark here, that it essentially rests on the ground that the oblique muscles are capable of performing the movements above attributed to them. This, indeed, they might possibly be believed to do in man, but it is at once evident, from a consideration of their comparative anatomy, that they could not perform these movements in animals; in which, notwithstanding, there is the same peculiar arrangement of the motor nerves. Indeed, there is no good reason to believe that any of the muscles of the eye, or their nerves, are more involuntary than the others, all being alike under that influence which maintains the harmony of the ocular movements; which are alike consensual whether the muscles employed be corresponding, or non-corresponding ones, on the opposite sides.

The view that the oblique muscles are provided for the purpose of turning the eye on its antero-posterior axis, is not a new one. It was advocated, in 1759, by Porterfield, and seems, from the remarks in his treatise on the eye, to have been the subject of discussion previous to that time, some having denied the existence of such a movement, as they could neither perceive its utility nor observe its occurrence. It was brought forward in 1786 by John Hunter, in a short paper on the use of the oblique muscles, among his observations on some parts of the animal œconomy, in which he has endeavoured to explain the circumstances under which such a movement is necessary; and the same view is attributed to Dr Jacob, of Dublin, in the last edition of Quain's Anatomy. The view of Hunter is well advocated by Dr G. Johnson, of King's College, London, in the article "Orbit" in the Cyclopædia of Anatomy and Physiology; and he adduces in support of it the result of experiments performed by him on a newly killed dog, from which it appeared that the contraction of the oblique muscles caused the eye to roll on its axis in opposite directions, without causing, in the slightest de-

gree, any of the movements by which the direction of the axis is changed.

The physiological uses of such a movement are not at first sight very apparent. In those lateral motions of the head, which take place between the atlas and axis, the eyes, in being kept fixed on an object previously viewed, are moved laterally by the recti muscles; but, as Hunter explains, when it is moved laterally, as from shoulder to shoulder, rotatory motion of the eyes, on their antero-posterior axes, is also required, to prevent the picture from changing its place circularly on the retina. Thus, when the head is bent down towards the right shoulder, the right superior and the left inferior oblique will be in action. It is evident, under such circumstances, when the head is twisted round on an axis more or less corresponding to that of the eye or eyes—that the latter must be rotated more or less directly on their axes, and in a contrary direction, otherwise the picture of the object would change its place circularly on the retina. This motion will be more or less combined with those performed by the straight muscles, according as the axis of motion of the head corresponds more or less to that of the eyes; and it may be that the oblique muscles, from their not exactly embracing the middle of the eye transversely, are intended also, and at the same time, to turn the axis of the eye, so as to change, to a certain extent, its direction. It would appear, therefore,—as it is essential to the correct viewing of an object when the head is being moved, that the picture fall in all respects on the same part of the retina,—that a more or less direct movement of rotation on the antero-posterior axis is very necessary; and it may be added, that the nature of this movement—as it causes little or no change in the direction of the eye—together with the circumstances under which it takes place, are such as not to render its occurrence evident to an observer. The circumstances under which this movement would be required in those animals in which the eyes are placed in the side of the head, and look outwards, are different; as, in the simple movement of raising or depressing the head, the latter moves more on an axis corresponding to that of the eyes, so as to render a more direct lateral rotatory motion necessary, in order to prevent the image from changing its place circularly on the retina. In these animals, it is doubtful if the eyes, which cannot be directed together to the same object, are moved simultaneously; but if so, the corresponding oblique muscles will be employed at the same time on opposite sides, whereas, in the circumstances explained by Hunter, the superior oblique of the one side would be in action along with the inferior of the other.

These, however, are so far only theoretical considerations, and do not affect the conclusion to which comparative anatomy points, that the oblique muscles—whatever minor differences may obtain as to their arrangement in different vertebrate animals—are, in all, not only capable of performing this movement in opposite directions, but are evidently intended for doing so.

It would thus appear, that, while comparative anatomy is irreconcilable with all the other theories above alluded to, it affords strong confirmation of the view that the oblique muscles are specially provided for turning the eye, more or less directly, on its antero-posterior axis.

Part Second.

REVIEWS.

Thoughts on Pulmonary Consumption, with an Appendix on the Climate of Torquay. By William Herries Madden, M.D., Physician to the Torbay Infirmary and Dispensary, Author of a Prize Essay on Cutaneous Absorption, &c. London, 1849. 12mo, pp. 219.

On Healthy and Diseased Structure, and the true Principles of Treatment for the Cure of Disease, especially Consumption and Scrofula, founded on Microscopical Analysis. By William Addison, M.D., F.R.S., Licentiate of the Royal College of Physicians. London, 1849. 8vo, pp. 320.

Report on Pulmonary Phthisis, as observed at the Hospital for Consumption, Brompton. By Walter Hayle Walshe, M.D., Professor of Clinical Medicine in University College, London, Physician to the Hospital for Consumption, &c. (British and Foreign Medico-Chirurgical Review, January 1849.)

The works of Drs Madden and Addison, though occupying nearly the same field, differ very essentially in their modes of treatment. The former has sought his materials far and wide, elaborating them principally with a view to a theory, to which we shall afterwards advert, and producing what even those who do not agree with him in his views will acknowledge to be a very talented and well-written work. Dr Addison, on the contrary, has been more anxious to "confine himself to facts arising from his own researches," than to enlarge upon the observations of others; and from strictly original observations, rather than from any extensive course of reading, he has formed a theory of the pathology of nutrition in tubercular and inflammatory diseases, which he unfolds with all the earnestness of a thoughtful and conscientious philosopher, and frequently illustrates with singular felicity and impressiveness. It is somewhat unfortunate that, by a certain scholastic inflexibility of style, and frequent obscurities and repetitions (partly, perhaps, arising from the nature of his speculations), the author has, in

some degree, unfitted this work for popularity among a large class of medical readers; but it is the more incumbent upon us, on this account, to express our decided sense of its value, and to assure those readers who are able and willing to follow Dr Addison through his somewhat profound and difficult microscopic and pathological studies, that they will find this volume replete with observations of great interest; and theoretical considerations which, if they cannot be in all cases directly accepted, at least demand the most careful consideration.

Dr Madden's work (the clear and readable style of which renders the appreciation of its contents comparatively an easy task) opens, after some preliminary observations, with an account of the chemical composition of tubercle, collected from various authors, in which we find nothing very new or very important; indeed, the difficulty of obtaining any substance which can be regarded as chemically pure unmixed tubercle, induces us to sympathise with the objections of Lebert to this mode of analysis, and to distrust all conclusions drawn from it. In the chapter on the histology of tubercle, Dr Madden gives a description, from personal observation, of the corpuscles peculiar to this morbid growth, and advocates strongly their *specific* character. Here, again, he agrees with Lebert, and differs from Dr Addison, to whose opinions we shall presently advert. In asserting the specific character of tubercle, Dr Madden draws a parallel between that deposit and cancer, and springs rather hastily to the conclusion, that "the fountain of the two diseases is alike a vitiation of the blood, and that the phenomena presented by both are what they are, simply because the morbid elements have a certain determinate and altogether specific character; or, to speak with more strict correctness, because the vitiation on which they depend is, in each, peculiar and distinct from all other morbid changes."—P. 22.

What, then, are the peculiar changes in the blood of tubercular patients? is the question next asked by Dr Madden. The chemical analyses by Simon, Andral and Gavarret, Becquerel and Rodier, and Glover, show, as a general result, a diminution of solid constituents, an increase of fibrine, and a very marked and constant diminution of the corpuscles. (We may observe that Andral and Gavarret's analyses show the increase of fibrine not to be present in the early stages of the disease, being probably produced by its secondary inflammatory complications.) Dr Madden enters at considerable length into the consideration of the phenomena of nutrition in the healthy condition, and their modification in the inflammatory process, which he correctly considers as an exaggeration, or rather perversion, of the "natural process by which healthy tissues are built up, or healthy reactions formed." In tuberculous or scrofulous individuals, again, "all parts of the body bear evidence to their imperfect or irregular nutrition. All, at times, declare

that the materials brought for their support are insufficient or unsuited, or that they themselves have not the power to make right use of them." With this rather vague declaration of opinion, we have no difficulty in concurring; but we cannot agree with Dr Madden in his belief that the deficiency of the blood corpuscles in tubercular patients "plays a very essential part in the production of tubercle," since, as he has himself pointed out, such a condition of the blood may continue long enough, as in chlorosis or Bright's disease, without any such morbid development. We think it sufficiently clear that all the ascertained vitiations of the blood in tubercular individuals—the excess of water—the diminution of corpuscles—and the gradual rise of the fibrine—are secondary changes due to the same constitutional condition, whatever that be, which produces the morbid deposit and its results. Similar conditions of the blood may and do exist under certain circumstances in most other chronic diseases.

Up to this point, then, Dr Madden's search for *the special* perversion of nutrition, *the peculiar* vitiation of the blood to which, as we have seen, he ascribes the formation of the morbid deposit, tubercle, has been almost, if not entirely, fruitless. The chemist has yet much to do before the pathology of tubercle can be fixed on anything like a positive humoral basis. But, in the meantime, in the absence of observations, Dr Madden takes refuge in analogies, and by their means attempts to make out a case for the hypothesis which it is evidently the main object of his book to support. According to this hypothesis, scrofula and tubercle (for in the author's opinion they are identical) are caused by a morbid poison circulating in the blood, and *specifically* evolving all the varied forms of disease referable to the special morbid deposit of tubercle. The arguments on this topic are spread over a good many pages, in which the author reviews the diseases generally ascribed to morbid poisons, and endeavours to point out analogies in the symptoms and phenomena of tubercular diseases. We shall present our readers with Dr Madden's brief summary of these detailed arguments:—

"They [the poison-diseases] are plainly members of the same natural family, differing, as individuals will do, in particular details, and in special combinations, but having so many features in common as to show that they spring from the same stock. Fever, we find, is almost universally present, though in varying degrees, and of diversified character; the body is emaciated; the digestive functions are more or less deranged; there are pains in the limbs; the skin is seldom natural in appearance, and its functions are perverted; the pulse is small; the whole system weakened; the secretions and excretions depart more or less widely from their normal characteristics. And then, we have organic changes in many organs; the eye is often affected; the larynx inflamed or ulcerated; pustules or abscesses form in the integuments and the subjacent areolar tissue; the brain is not uncommonly diseased; the bones, and particularly the joints, suffer severely; the mucous membranes are greatly influenced; while the important depurating viscera, the kidneys, the liver, and the lungs, are, in a peculiar manner, liable to destructive alterations.

"Are not the phenomena of scrofula, in general, and of phthisis, in particular, closely analogous, I would almost say identical?"—P. 95.

We must say, that, even after perusing Dr Madden's arguments, we cannot answer the above question in the affirmative. We cannot discover, in the family likeness here indicated by Dr Madden, any more than might be asserted of any two or more constitutional diseases resulting in organic change. Where shall we find a disease, attended, as phthisis is, with a long-continued suppuration and organic alteration of most important viscera, in which there are not almost all the constitutional symptoms here indicated? Are not the fever—the altered secretion and excretion—the prostration of strength, in phthisis—consequences which might equally be expected from the suppuration following a severe external agency? Are not the emaciation—the deranged digestive function—the altered secretions—natural consequences of the invasion of the mesenteric glands and other abdominal organs by the tubercular deposit, just as the cough and dyspnœa are consequences of the pulmonary affection? And if we consider the "organic changes in many organs," upon which so much stress is laid by Dr Madden, is there any real resemblance between the organic changes in phthisis and in small-pox? or any true analogy in this respect between either of these diseases and typhus fever, in which there are often no "organic changes" at all? We contend that there is no limit to such vague analogies, and that Dr Madden's argument must either be extended to all constitutional diseases, or abandoned altogether.

The most superficial consideration, indeed, of the undoubted *poison-diseases* will show that it is not on account of any such fancied nosological resemblances that we infer the existence of a morbid poison. There are surely no such striking resemblances between the symptoms of mercury and ergot of rye as poisons; or between iodine and lead—small-pox and ague—the *constitutional* syphilis and the *local* gonorrhœa, as to lead to any such inference. The idea of a morbid poison is founded upon the *direct* proof we have, or suppose we have, of a specific agent producing a specific deleterious effect. We may not, indeed, be able to collect and isolate the agent, but we must have fair and ample logical proof of its existence; we must be able to see it clearly with the mind's eye—that is, to trace the laws of its origin and diffusion, and the mode of its application to the human frame. Failing this knowledge, the *mere word* morbid poison becomes a cloak for our ignorance of a constitutional or local cause of disease, instead of the expression of any advance of our real knowledge, practical or theoretical; and such do we consider it in the case of phthisis and scrofula.

Dr Madden may think that we have dealt rather rigidly with an innocent hypothesis which we are confessedly not able positively to disprove. But we contend that, according to all principles of logic, the *onus probandi* rests with the author of such a hypothesis, and

we are quite entitled to express our dissent upon the grounds we have stated. Moreover, in the chapter on treatment, the author's theory by no means occupies that humble place to which we think it is alone entitled; and we are not sure that in less judicious hands than his own, the confident assertion of such a theory, with a view to practical deductions, would be altogether free from danger. We find the chapter on treatment opening with the following promising sentences:—

"Our former investigations concerning the true nature of phthisis will prove, I conceive, of signal service in communicating clearness and precision to our views regarding its right treatment. For having once arrived at a definite conclusion as to the essential cause of the phenomena, we know in what direction to work, and acquire an intelligent perception of the end at which we should aim." * * * —P. 149.

"The treatment of tubercular diseases, to be effectual, must be directed to the securing of the following ends:—viz., the neutralization of the poison, or its elimination from the system, and the restoration to a healthy state of the great function of nutrition in all its parts. Combined with these there must be, of course, the meeting of local evils by local remedies; including under that term, not only such means as act directly on the part to which they are applied, but also those agents which, though they may have a general influence, in some measure, on the system at large, have likewise, in addition to this, a special action upon particular organs"—P. 151.

As, however, there is unfortunately no specific remedy for the neutralization of the tubercle-poison, it follows that we must seek chiefly the elimination of it from the system. As to the other indications mentioned, there can, of course, be no difference of opinion.

"The channels by which these substances are carried out of the system, are, mainly, the great blood-depurating organs, the skin,—the lungs,—the intestinal mucous membrane, with its appended glands, especially the liver and the kidneys."—P. 156.

Here is obviously a great field for exertion. The imagination at once suggests how agreeably the phthisical patient might be kept perpetually "eliminating" by a proper succession of powerful remedial applications. Calomel, Dover's powder, squill and digitalis, might, with this view, be called successively into requisition; or the hydropathic treatment might succeed in draining the morbid poison out of the skin, as is sometimes done in gout or rheumatism, while a brisk course of Morrison's pills might accomplish the indication of "purifying the blood" through the intestines. Dr Madden seems not insensible to the possibility of such heroic applications of his own doctrine, and accordingly we find him entering a "caveat" against the "rigorous adoption" of the hydropathic method, while he recommends, in common with most other physicians, the frequent ablution of the whole skin, with a view to preserve its functions in a state of activity. With this he recommends pure air, and "attention" to the state of the liver and kidneys, which,

we presume, means nothing more than care that their functions are duly performed without exaggeration or defect. Dr Madden's advice on these points coincides with that of all wise practical physicians; the only peculiarity being his strong recommendation of the use of the saline carbonates as diuretics, in combination with vegetable acids, as prescribed by Dr Golding Bird. Dr Madden sums up his general idea of hygienic and tonic treatment of phthisis as follows :—

"It is peculiarly imperative upon us to employ such general means as will give tone to the enfeebled fibres, and aid, if it may be, in the construction of better tissues. We cannot *physic* the poison out of the system, but we may help forwards its elimination, and we shall best secure this end by striving to keep the vital powers at their highest point of healthy energy, while at the same time we repress all undue and hurtful action."—P. 190.

In this general sketch we need scarcely say we are almost at one with the author. Indeed the treatment is throughout both simple and judicious, and perfectly in harmony with the most received ideas. We recommend it very heartily to the reader's perusal.

It appears, therefore, to us, that the practical, as well as the theoretical, results of the hypothesis which Dr Madden is so anxious to establish, are by no means commensurate with the importance he appears to attach to it. While we feel bound to make this criticism, we have no hesitation in saying, that as an ingenious speculation, on a difficult subject, this work will command the interest of many readers; while its practical sections are calculated to give a very favourable idea of the experience and judgment of the author.

In attempting to give our readers, in the limited space at our disposal, some idea of Dr Addison's work, we must confine ourselves to the illustration of a very few only of the striking doctrines and observations with which the volume abounds. The author sets out with a short sketch of the known phenomena of vegetable morphology, which is followed up by a description of the structure of the human embryo, in order to show that in all organised forms, the earliest stage of every texture is characterised by corpuscular or cell-growth. The cells possess, at first, very slight coherency; at a later period, in a higher development, they form a coherent tissue, which is illustrated in plants by the chlorophyll-cells of the leaf, and in animals by the parenchyma of the liver, and many other structures. A still later development is that of fibres and tubes both in plants and animals; and last of all come the most highly elaborated textures, peculiar to the latter,—cartilage (?) and bone. The law of metamorphosis of organised structures may be expressed as follows—The lowest type is found in non-coherent cells; the next in cellular tissues; the next in fibres and cells conjoined; and the highest in dense structures, such as cartilage

and bone. The metamorphosis is *regular* when all these structures are developed according to the normal type; it is *irregular* when all the elements are nominally present, but in altered amount or grouping; and it is *retrograde* when the higher phase is in any instance exchanged for the lower.

In the permanent tissues of the adult animal, all these different types may be found. In the more dense and coherent structures, whose function is chiefly mechanical, the fibrous and the higher types prevail; in the secreting structures, on the contrary, including the mucous membranes, the lower or cell-elements are more abundant, and are perpetually discharged to be replaced by new cells. The blood is a fluid composed entirely of non-coherent cells, and therefore belonging to the lowest type of metamorphosis. From it the tissues are formed by the development of its colourless cells or lymph-corpuscles through the successive stages of the regular metamorphosis. In corroboration of this doctrine (which we may observe is peculiar to Dr Addison), he adduces the fact that the earliest stages of cell-growth throughout the body exactly resemble these corpuscles; as may be seen in the cells detached at an early period from mucous structures, and also in pus-corpuscles. In like manner "no wounded or abraded texture heals, and no diseased texture ulcerates, without discharging an abundance of colourless cells;" some of which are, in the former case, carried into the higher grades of development, and go to form tissue; while, in the latter, they are all cast off without undergoing development. In both cases a large excess of cellular elements is set free, in virtue of a law of nature, by which any cause producing stimulation of a part produces a flow of blood and a discharge of colourless cells at the seat of irritation. Inflammation therefore and reparative action, consist of the crowding of these colourless cells into a part, and their subsequent metamorphosis into higher types of structure. They are thus exaggerations of normal processes of growth.

In certain conditions of the system the metamorphosis of tissue takes place *imperfectly*. The colourless cells which are poured out on the surface of wounds (forming granulations), are slow to undergo the change into tissue; in this case the inflammation is called *unhealthy* or *asthenic*. If a similar defect of development accompany the ordinary process of growth, the normal textures become gradually exchanged for, or supplanted by, the products of a *retrograde metamorphosis*, and scrofulous disease is developed.

Scrofula, therefore, is essentially, according to Dr Addison, a retrograde metamorphosis of the tissues, in which colourless cells of a low type are substituted for the coherent cells and fibres, or possibly for the cartilages, bones, &c., which form the structures of the perfect human body. This morbid condition may take the form of ulcers, in which the natural tissues become more or less gradually broken down into pus, the cells of which appear to have no power of forming themselves, first into granulations, and then

into tissue, as in natural inflammation; or it may result in the formation of tubercles, where the colourless cells are deposited in particular points or centres, from which they spread, and finally break down, leaving cavities or ulcers. The "*vis scrofulosa*," as Dr Addison calls this tendency, may also manifest itself in the asthenic inflammation following a wound, or the imperfect reparation of a fracture.

"Inflammation, and organic disease, are appropriate terms when new products *arise in addition to*, or upon, the normal textures; for instance—false membranes—exudations—secreting villi or granulations *on* the pleura; layers of lymph *on* the iris or pia mater; solid matter *in* the air-cells of the lungs; or red fungosities *sprouting from* the conjunctiva of the eye. Scrofula and scrofulous disease are applicable terms when the new products *destroy* the natural fabric—fungosities *supplanting* the cartilage of the joint, or the hard structure of a bone—ulcerations yielding pus *displacing* the papillæ of mucous membranes—and villous secreting fabrics *destroying* the respiratory parenchyma."—Pp. 81.

As in the case of plants, the retrograde metamorphosis may be the result of two causes; there may be an original taint of constitution in the subject of it, or this taint may be acquired from the presence of unfavourable external conditions. Once acquired, it may be transmitted to offspring, and may be lost again at a future period.

Such are the outlines of the very remarkable theory of Dr Addison, which, as the physiologist will at once perceive, contains far too many true and too many doubtful elements to be made the subject of hasty criticism; undeniably bearing, as it does, the traces of profound thought and extensive observation. It is obvious that a difficulty, which presents itself at the outset, is Dr Addison's repudiation of the *specific* character of the tubercle-corpuscle as described by Lebert, who has decidedly every claim to be considered the highest authority on this subject. And yet we can bear out, from personal examination, the following statement of the author:—

"I have repeatedly examined with the microscope the material deposited in the air-cells of the lung in *pneumonia*, and compared its characters and appearance with that forming a tubercle, without being able to detect any more essential or specific difference between them than exist between purulent matter recently excreted and that of an old chronic abscess. The same class of objects, colourless cells and altered protoplasmatous matter, constitute in both cases the bulk of the material. And were we to imagine the fluid elements of old pus, removed or absorbed, the remaining solid matter would appear to possess the characters of tuberculous matter; the colourless elements of blood —pus and tubercle—passing by insensible gradations into each other."—P. 52.

Whether the above statement be the result of imperfect observation or not, it is certain that tubercle-cells are often distinguished with great difficulty, especially in secreting structures.

It is to be observed that, by breaking down the distinction between the tubercular and asthenic inflammatory processes, Dr Ad-

dison naturally adopts the view of the identity of scrofulous and tubercular diseases, about which so much has been written of late years. Those, on the other hand, who, like Lebert or Dr Madden, maintain the specific character of tubercle, have only two courses open to them, either to cut off from the list of *scrofulous* diseases all which are not *tubercular*, or to admit a distinction between the two classes of disease. Dr Madden's book is by no means clear or satisfactory on this point. The later statistical researches of Lebert[1] distinctly show that many of the most decidedly scrofulous affections, such as those of the bones, the eye, the skin, &c., are rarely or never tubercular, and are, in a large proportion of cases also, unconnected with tubercular deposits in other organs; while tubercular deposits of the lungs and glands, on the other hand, frequently occur without a trace of collateral scrofulous affections. Hence M. Lebert does not hesitate to consider tubercle and scrofula as essentially distinct affections, though frequently conjoined in the same individual—a view similar to that of Mr Phillips and others in this country, though argued on apparently more precise data.

Our space prevents us from entering more minutely into the consideration of Dr Addison's views at present. The chapter on Etiology is extremely interesting, though strongly marked with the author's peculiarities of style. He states, we believe quite correctly, that pulmonary tubercles in very early stages, when scarcely appreciable by the unaided eye, are very common, and frequently produce no symptoms. Dr Addison believes, also, the condition of atelectasis of the lungs to play a very important part in regard to the etiology of tubercle.

The elaborate report of Dr Walshe is an official document, prepared "in obedience to a desire expressed by the committee of management of the Hospital for Consumption, that some statement of the results obtained within its walls should be made public." It must have been the result of immense labour—and labour regulated, even to the minutest details, by a perfectly systematic plan, of the intricacy of which it is impossible, without perusing the report itself, to form any idea. We are not acquainted with any production of the "numerical" school of medical inquiry, either in this country or in France, which shows a more perfect command of all the resources of this method, as applied to a series of cases

[1] Published originally in Roser and Wunderlich's Archiv., and now incorporated with M. Lebert's new work, "Traité Pratique des Maladies Scrofuleuses et Tuberculeuses, Paris, 1849;" an expansion of the Author's former researches on Tuberculosis, read to the French Academy of Medicine, and published in Muller's Archiv. See *Monthly Journal*, October 1844, p. 896. We shall take an early opportunity, in the Monthly Retrospect, of directing our readers' attention to these later researches, which are of considerable importance.

of disease. In the generally satisfactory nature of its results, and the scrupulous exactitude of their expression, it may be taken as a model of what such a report ought to be, and fully demonstrates that the unwearied industry necessary for the preparation of such a document is fully repaid by the issue.

The favourable effect of a residence in hospital, in general, upon phthisical persons, is shown by the fact, that out of an aggregate of those admitted (including even those moribund at the time of admission) *more than two-thirds* left the hospital either in an improved condition, or with the disease unadvanced. Dr Walshe further mentions, that *all* the patients who had not reached the stage of excavation underwent improvement, and even when excavation existed, improvement was more frequent than the reverse.

"In 4·26 per cent. of the cases (9 out of 211), complete restoration to health, not only as regards apparent disturbance of the functions generally, but as regards local evidence of active pulmonary disease, was effected." Further, in 7·58 per cent. (16 out of 211), there was a complete disappearance of symptoms, some of the physical signs remaining; and some of these cases appear to have been in the stage of excavation. We think that the publication of these cases, especially those of the former category, would form an interesting contribution to the natural history of phthisis.

Some of the most interesting of Dr Walshe's results are in reference to the doctrine of the hereditary nature of phthisis. Although many persons must, we think, have doubted whether the popular belief on this subject is true to the full extent, yet few will have been prepared for the following statement, which, we can assure them, is a most cautious inference from the complicated evidence adduced in the report.

"From a comparative analysis of the family histories of 446 persons, 162 of them phthisical, 284 non-phthisical, the final conclusion flows:—*that phthisis in the adult hospital-population of this country is, to a slight amount only, a disease demonstrably derived from parents.* It is possible (nay, indeed, *probable*, for, in adults having a parental taint, the outbreak of the disease occurred a mean period of two years and nine months earlier than in those free from such taint, vid. p. 238, Table) that, were investigation extended to infancy, childhood, and youth, the ratio of cases of parental taint among the phthisical, would be proportionally greater than it proves where inquiry is limited to adults. But, on the other hand, there is no single valid reason for *supposing* (prior to actual experience) that the increase in that ratio would be of more than trifling amount. Again, whether the law differs in the adult portion of the middle and upper classes of society, from that holding in the humbler classes (those supplying hospitals), can only be positively determined by an analysis of family histories collected among the former classes; meanwhile, it appears justifiable to doubt the reality of any such difference."—P. 254.

A most important portion of the Report is devoted to the consideration of the laws of hæmoptysis, a condition which existed in

106 out of 180 patients (80·92 per cent.), and in an almost similar proportion in the two sexes. The author has shown satisfactorily that the tendency to hæmoptysis is greater after than before softening of the tubercles; and that it becomes relatively greater with advancing years in both sexes.

The diagnostic value of hæmoptysis, as a symptom of tubercle, is estimated very highly by Dr Walshe from the data in this report. It appears that, with the exception of cancer of the lung, aortic aneurism, and disease of the heart, there are very few cases in which a material hemorrhage takes place from the lung independently of tubercles. Neither those cases of apparently simple chronic bronchitis, nor those of irregular menstruation, in which blood is voided from the lungs to any extent, appear to be exceptions to this proposition, although the tubercles may remain for a considerable period latent after the occurrence of the hæmoptysis. We give the following most important conclusion in the author's own words:—

"The tendency of my experience, then, is clearly to show the vast frequency with which hæmoptysis is in some manner or other an attendant on tuberculous disease. The fact, that individuals are occasionally met with who, after having had more or less profuse hæmoptysis, live on to a good age without exhibiting phthisical symptoms, does not invalidate this result: it simply shows that tuberculisation tending to hæmoptysis may, as well as that not so tending, undergo spontaneous suspension; and the consideration of the point becomes rather a question of prognosis than anything else."—Pp. 252, 253.

We have thus selected for our readers a few of the more important results of Dr Walshe's researches; but we cannot conclude without again recommending the entire document to their careful study.

Copy of a Letter to the Lord Chancellor from the Commissioners in Lunacy, with reference to their Duties and Practice, under the Act 8 *and* 9 *Vict., c.* 100. Ordered by the House of Commons to be printed, 1 August 1849.

Our readers have already been made acquainted with the circumstances which have elicited the above document from the Commissioners in Lunacy. The *dictum* of the Lord Chief Baron, that no lunatic ought to be confined, unless dangerous to himself or others, has been amply proved by Dr Conolly (see our August Number, p. 1000) to be at variance equally with common sense and with the principles of true humanity and justice. The Commissioners in Lunacy adopt, in the present letter, so nearly the same arguments, that we need not go over the ground again; but we think it right to place before our readers the view that those legally responsible to the Government in these matters take of

their own duties, and of those of medical men, in order that the latter may not have their judgment overborne by the show of authority that might be set in opposition to it :—

"Amongst the multifarious duties of the commissioners one is, to discharge patients when 'detained without sufficient cause;' of the sufficiency of the cause, the commissioners are by law constituted the judges; and they endeavour to form the best judgment in their power in each particular case, when the subject comes before them. Thus, if a patient be placed in an asylum without just cause—without having been of unsound mind—the commissioners are bound to discharge him. So, if a patient (originally a fit subject for confinement) be recovered and restored to a sound state of mind, the commissioners are obviously bound to discharge him, if the person under whose care the patient is placed, or the relative who originally authorised his confinement (for upon these primarily the duty devolves), has neglected to do so. But a person labouring under an insane delusion is not detained without sufficient cause.

"Under peculiar circumstances, where, after sufficient observation, a patient, although of weak or unsound mind, appears to be perfectly harmless, the commissioners frequently promote his liberation, if he have a comfortable home, or any friends disposed to receive and protect him and his property from injury; but, where this is wanting, the commissioners do not think themselves justified in removing the patient from the shelter of an asylum, and leaving him at large, and unprotected. They consider it to be quite clear that they are not bound, as a general rule, to speculate upon the chance that a patient who, in their opinion, is still insane, will be perfectly harmless if at large, and therefore to liberate him accordingly."—P. 7.

"It is obvious that the finding of a jury is in no case essential, in order legally to justify the confinement of a person of unsound mind. In fact, out of 4028 private patients (many of them possessed of considerable property), who were confined in asylums on the 1st of January 1848, only 245 had been found lunatic by inquisition.

"To revert to the opinion stated to have been expressed by the Chief Baron, that no person should be placed or detained in any lunatic asylum, unless he be dangerous to himself or others: upon this point it is of vital importance that no mistake or misconception should exist, and that every medical man who may be applied to for advice on the subject of lunacy, and every relative and friend of any lunatic, as well as every magistrate and parish officer (each of whom may be called upon to act in cases of this sort) should know and be well assured that, according to law, any person of unsound mind, whether he be pronounced dangerous or not, may legally and properly be placed in a county asylum, lunatic hospital, or licensed house, on the authority of the preliminary order and certificates prescribed by the Acts 8 and 9 Vict., c. 100, or c. 126 (as the case may be).

"The order and certificates thus obtained show that the person mentioned therein is either a lunatic, an idiot, or a person of unsound mind, and a proper person to be confined, and fully justify all parties in the matter, and they enable the proprietor or superintendent of any hospital or licensed house to plead them in defence to any action, and are, in the words of the statute, a justification for 'taking, confining, detaining, or retaking,' the patient—(see 8 and 9 Vic., c. 100, s. 99)."—Pp. 9, 10.

Part Third.

MEDICAL NEWS.

AN APPEAL TO LEGISLATORS ON MEDICAL REFORM.

We wish to be as considerate as possible in our demands upon statesmen, to make all proper allowances for the difficulties of their position, and all just admissions as to the extent to which these difficulties have been aggravated by the conduct of the members of our profession; but after the most deliberate review of the past history of the question of medical reform, it is our opinion that we are bound, at the present juncture, to proclaim, on the part of that profession, our great dissatisfaction with the dilatoriness and inefficiency of our legislators in relation to this very vital matter. There is nothing similar to it in the treatment which other men in other professions have experienced at their hands. Divines of the churches of England, Scotland, and Ireland, and even of Dissenting and Roman Catholic churches, have their grievances considered, and, when they are reasonable, redressed. The same thing is true of lawyers, of merchants, of agriculturists, and of manufacturers. Each of these classes may seek redress for unreal grievances or improper remedies for real ills, and may therefore often encounter disappointment in its applications to the legislature; but there is no instance among them of serious grievances, admitted at all hands to be such, admitted, too, to be the direct result of faulty legislation—to deserve a remedy, and to be in their nature insusceptible of any other than a *legislative* remedy—remaining altogether unremedied for a third part of a century, during all which time an act of the Legislature has been respectfully, earnestly, and assiduously solicited by those who suffered from the want of it. To what cause is this long-continued inaction to be attributed? We much fear that the conduct of our legislators may be justly likened, in this matter, to that of a certain judge, of whom every one has heard, who feared not God nor regarded man, and who was willing to grant to the importunity of those who might have troubled his repose that which he would not concede to more temperate and less rude petitioners. Medical men have little disposition and little power to raise a hurricane about the ears of a cabinet-minister. But he *must* give attention to lawyers, divines, agriculturists, and others, because, unlike medical men, they possess efficient means of agitation, and, if unreasonably neglected, would shake Downing Street as with an earthquake, and threaten a whole cabinet with instant dissolution. This theory of our's has all the qualities of a good philosophical theory, for it assigns such causes only as can be proved to exist, and are adequate to the explanation of the phenomena; and it suggests to us, as a practical inference, that, if we wish to succeed, we must be more importunate, more troublesome, to ministers of state and to members of Parliament, than we have ever been hitherto, and must convince them by our conduct, that till they do us the justice of making the reformation of medical legislation a part of their policy, they have no chance of escaping from such severity of criticism as their apathy deserves, and as we have it in our power to apply to their conduct.

Let us, then, in conformity with this very necessary policy, examine some of the pleas usually urged by statesmen in justification of their own dilatoriness and indifference in matters medical, and see whether such pleas excuse these deficiencies in their obvious duties. We think it right to premise, that, in what we are about to say, we disclaim all individual and personal allusions. Not only so, but we think a debt of gratitude to be justly due to the members of the Select Committee of the House of Commons on Medical Registration and Medical Law amendment, and to others who preceded them in the laborious work of inves-

tigating the machinery of our medical institutions. To such men we shall always be ready to do ample justice. Our censures are wrung from us by the postponement, on what we deem improper grounds, of those important reforms to which our profession has, in every possible way, urged its just claim; and we again say, they are *general* in their application, and in no case pointed at individual statesmen.

By far their most usual excuse is, that medical men are not agreed among themselves on the *nature* of the legislation which they require. And pray, my lords and gentlemen, is it your uniform practice to delay all remedies in other cases till complainers are *agreed* about these remedies? Did you do so in the case of educational bills in England, Scotland, and Ireland? Were divines agreed in their views about Lord Stanley's Irish education bill, or Sir Robert Peel's Irish colleges bill? Were agriculturists agreed about the corn bill, West Indians about the sugar bill, or ship-builders and ship-masters on the navigation bill? Do lawyers never differ about law reforms, or commercial men about commercial reforms; and is it your invariable practice to wait till these parties are agreed before you venture on a remedy? You know well that such is *not* your practice. And to do you justice, you frequently act manfully in the very teeth of violent and unreasonable clamour, when some important public interest demands your authoritative interposition. Why, then, in the case of *medical* legislation, will you fold your hands and proclaim it to be your principle to postpone legislation *dum defluat amnis*, that is to say, till doctors cease to differ? Even if the question related merely to the interests of our humble profession, this course is nothing better than a deliberate abandonment of your duty to us, as a portion of the body politic. But it is worse than all this—it is an abandonment of your duty to that public for which you as legislators, and we as professional men, exist. Without being chargeable with over-rating our own importance, we think we may venture to assert that it is a matter of no small benefit to the public to possess thoroughly well educated and competent medical advisers. Yet you deny to the best educated men whom the kingdom contains, the right of legally practising their useful profession, except within certain lines of artificial demarcation, while you secure to the vendor of patent nostrums the legalised monopoly of the right of circulating his pernicious trash in all parts of the British dominions. Is not this conduct on your part, so obviously unjust to the profession, also in the highest degree injurious to the public?

Let us consider the effect of the restrictions to which educated medical practitioners are subjected on the position of ignorant quacks and knavsih pretenders, licensed and unlicensed. There are many such; and they are agreed in one thing, if in nothing else—that all the knowledge accumulated by educated medical men, from Hippocrates downwards, ought to be repudiated. One of them may think that hydropathy ought to be substituted; another, homœopathy; a third, mesmerism; a fourth, something else: they differ as to the substitute for science, but agree as to the point that men of science and integrity are their natural antagonists. And truly they are so. The game of these pretenders is to work on human credulity to the greatest possible extent, and they dread nothing so much as the approach of professional men of real education and high attainments. It is far from our wish to subject these men to pains and penalties for the mere act of giving advice to those who think it worth having; on the contrary, we are deliberately opposed to such penalties, either in the case of the licensed or of the unlicensed, as being at once tyrannical and inefficient: —tyrannical, inasmuch as they hinder the people of these kingdoms from taking such advice as they may happen to confide in; and inefficient in the prevention of quackery, inasmuch as quackery, both intra and extra-professional, is most rampant in those parts of the kingdom, in which penalties against the unlicensed are most in use. But there is a great security against the prevalence of ignorance and of knavery on the part of medical practictioners in the possession of professional men of high moral and scientific

character; and of this only genuine security, the existing motley system of medical legislation, persisted in as it has been for so many years in opposition to the most earnest and repeated remonstrances, in a very great degree impairs the efficiency. In other departments of industry, the benefit of the removal of artificial obstacles of every kind from the enterprise of British subjects is fully recognised. We all admit that the old chartered boroughs, with their privileges of exclusive trading, were "public pests;" and surely those local exclusive rights, which are held in the case of shoemakers and tailors to be injurious to the public, cannot be innocent in the case of a more important and more intellectual profession. The simple fact is, that penalties for unlicensed practice are scarcely ever directed against any but the educated. Artificial lines of demarcation divide from each other different portions of this kingdom, and a new expense for a medical qualification, and, what is still worse, a fresh examination, perhaps after many years of practical experience, are the penalties of a change of residence. If it be the wish of legislators to give encouragement to professional knaves and fools, they can by no other means so effectually accomplish it as by thus discouraging and encumbering the professional exertions of honourable and enlightened men. Our profession, unlike the professions of divinity and of law, has few salaried offices to look to. Without official patronage, and dependent on our individual character and efforts, we esteem it to be more necessary to our success than to that of any other class of professional men, that we should be free from all unnecessary shackles. We seek to put down ignorance by knowledge, and secrecy and mystery by openness and frankness. Nothing is in such bad odour with our profession as the concealment of facts connected with the principles of medical practice. It is the object of our unmixed detestation and contempt. Really instructed men of all professions carry along with them humanising influences wherever they go, and far from seeking to monopolise information, take a special delight in imparting it to all inquiring persons who come within the sphere of their influence. Medical Reform would facilitate the introduction, into the villages and hamlets of this country, of a more highly educated class of medical gentlemen, and would thus be one of the most effectual of all antidotes to dishonest and ignorant medical practice. It would also, by facilitating the interchange of medical men between the different divisions of the kingdom, be very effectual in the removal of local prejudices, and of diffusing more widely the knowledge of all useful improvements in the healing art.

But is it true, after all, that there is a want of union and concert among us as to the nature of the evils under which we suffer, and of the remedy which we require? On the contrary, we contend that there is as much union as our legislators have a right to expect. We all agree that there ought to be a uniform education for men of the same grade, without distinction as to the place of their education. We agree in being willing to submit the question of the minimum to a board properly constituted. We do not quarrel about the constitution of such a board; and are willing to have it nominated by the Crown. We contend that all licensing boards should be placed under proper control so as to secure as much uniformity of action as can be attained. We contend that a medical man once licensed ought to be held qualified in all parts of the British dominions. These great principles, which no important part of the profession will impeach, are surely a sufficient basis for the operations of legislators who are willing to do their duty; and if there are disagreements which cannot be removed but by the strong arm of legislation, it ought to be recollected that such disagreements are most generally the effect of the corporate spirit, and of the vices for which a remedy is demanded; and that to reason against that remedy, because of such disagreements, is to make the existence of the disease a reason for withholding the only possible means of cure. This is reasoning in a circle. It is the plea of laziness and indifference, and we hope it will not be repeated.

The labours of investigation are now over. Every legislator has access to

the fullest information ; there is not the semblance of an excuse for longer delay. Difficulties there are undoubtedly, but not greater difficulties than those which usually impede the course of useful legislation in all its departments. Were it the case of a corn bill or a sugar bill, the Legislature would have to deal with those whose interests, or *imagined* interests, are bound up with existing abuses. Such difficulties are usually only to be vanquished by making their removal the act of a responsible government. We believe that the government might calculate on important assistance from many of their political opponents in a fair attempt to grapple with the difficulties of medical legislation; and it is much for their interest to endeavour to do so during the approaching session, inasmuch as longer postponement of that which is so necessary will be the cause of the greatest dissatisfaction and impatience. We trust, therefore, that the government will be persuaded of the necessity of taking up the question on their official responsibility, and on the strength of the information which they have received; and, without further negotiation with corporations and sections of medical men, will lay upon the table of the House of Commons, at the beginning of the session, the matured result of their deliberations.

REPORT OF THE EPIDEMIC OF CHOLERA IN THE SIXTEENTH DISTRICT OF THE CITY OF GLASGOW, FROM DECEMBER 20TH TO FEBRUARY 21st 1848-9. BY JOHN M'ETHERAN, L.R.C.S.E., MEDICAL SUPERINTENDENT.

The district is situate chiefly on the southern side of a steep hill, extending about five furlongs east and west, in the northern part of the city parish, and extends over the hill in the direction of the seventeenth district, where the disease at first prevailed. The population contains about 2000 poor, of whom at least three-fourths are congregated in a few streets, which are, however, widely apart from each other. The other fourth are scattered in top and sunk flats all over the district.

The cholera in my district was severe only in particular localities, namely, High Street on the east, along the ridge of the hill in Rottenrow Street, and in Dempster Street, which goes down the hill on the west. These streets are mainly inhabited by paupers and working people. Dempster Street is entirely so, but four streets of the same class lying in the declivity between these infected ones, and another on the borders of the district, had only a case or two in each ; and several dirty lands and closes, which used to suffer much from typhus fever, escaped altogether, so far as I know. In other respects, too, cholera did not spread as typhus usually does. The poorer, and especially the dirty confined places, were seized last, but, in all cases, suffered most.

Dr M'Kim saw the first eight known cases in the district ; they occurred in eight different lands, in six different streets. My first six cases occurred in six different lands, in three different streets. They were not located near Dr Kim's, or any other cases that I or the students who were appointed to make daily visitation could hear of. In none of these first cases could Dr M'Kim or I gain any fact which could establish even a probability of contagion, although we inquired most minutely. During the progress of the epidemic, the inhabitants of infected tenements were constantly exposed to contact with the sick —there were generally three or four people at a time rubbing the patients so long as the spasms lasted. The numbers who were thus exposed and who escaped the disease was very great ; and I cannot sincerely say that any one case of cholera happened which appeared to me to arise from contagion. Persons in the infected houses and tenements took bilious diarrhœa and rice-water purging; but, in such cases, I could find satisfactory predisposing causes, in the nature of the place itself, in the natural or acquired debility of the subject, or in the depression produced by grief, fatigue, and wakefulness in attending the patients, and, in almost every case, that woful curse of society, the using of spirituous drinks,—taken on these occasions, "to keep off the disease." The cases of malignant cholera succeeded each other in the most eccentric manner

as regards locality. And there was always at least four or five days' interval between cases occurring in the same house, excepting when several members of families were seized within an hour of each other. Three instances of this kind happened, the weaker members passing at once into cholera, and the stronger continuing some hours in bilious diarrhœa. The great majority of houses and tenements affected had only one case of malignant cholera in each, evidently because the visiting students watched them particularly, and were careful in checking premonitory diarrhœa when it affected any of the inhabitants.

During the first 20 days, 32 cases of malignant cholera occurred, and 20 lands were affected ; 13 of these lands had but 1 case in each during that time.

The total lands affected with malignant cholera were 44, 6 of them had more than 2 cases in each, 8 had only two cases in each, and 30 had only one case in each. Total houses affected, 59; 47 had only 1 case in each, 12 had more than 1 case in each, and 2 only had as many as 4 in each. But the choleraic influence was more general and intense than what these bare facts might imply, because the cases of bilious diarrhœa and rice-water purging were principally in the cholera localities. But, according to the vistors' reports, and my own experience, the houses infected with malignant cholera did not suffer much more from bilious diarrhœa than the other houses in the same tenement, and the people in the former had, of course, more predisposing causes to contend with.

I can point to at least twenty houses wherein I attended cholera patients, and where no case of diarrhœa followed.

THE LATE DR JOHN REID.

We are happy to learn that, in compliance with the wishes of many of Dr Reid's most intimate personal and professional friends, a memoir of this eminent physiologist will shortly appear from the pen of Dr George Wilson, of Edinburgh, who will obtain their co-operation and assistance. We shall, therefore, with the consent of those gentlemen who have kindly furnished us with the fragmentary notices which we had intended for insertion in this number, place these in Dr Wilson's hands ; and we take this opportunity of inviting the friends of Dr John Reid, who may be among the number of our readers, to furnish Dr Wilson with whatever information they may possess.

APPOINTMENTS.

Mr Hilton has been appointed surgeon to Guy's Hospital, in consequence o the vacancy caused by the death of Mr Aston Key.

Dr J. Risdon Bennett has been appointed physician to St Thomas's Hospital, in place of Dr Henry Burton, recently deceased.

The following is a list of the recent appointments to Queen's Colleges, Ireland :—Dr Thomas Andrews, Professor of Chemistry—Belfast ; Dr J. Blyth, Professor of Chemistry—Cork; Dr James V. Brown, Professor of Surgery—Galway ; Denis B. Bullen, Esq., Professor of Surgery—Cork ; Dr William Burden, Professor of Midwifery—Belfast; Dr Alex. Carte, Professor of Anatomy and Physiology—Belfast ; Dr Hugh Carlile, Professor of Anatomy and Physiology—Cork ; Dr N. Colahan, Professor of Medicine—Galway ; Dr George Dickie, Professor of Natural History—Belfast ; Dr John C. Ferguson, Professor of the Practice of Medicine—Belfast ; Dr Alex. Fleming, Professor of Materia Medica—Cork ; Dr Alexander Gordon, Professor of Surgery—Belfast ; Dr J. A. Harvey, Professor of Midwifery—Cork ; William Hincks, Esq., Professor of Natural History—Cork ; Dr Croker King, Professor of Anatomy and Physiology —Galway ; Dr Simon M'Cay, Professor of Materia Medica—Galway ; Dr A. G. Melville, Professor of Natural History—Galway ; Dr D. C. O'Connor, Professor of the Practice of Medicine—Cork ; Dr Thomas O'Meara, Professor of Materia Medica—Belfast ; Dr E. Ronalds, Professor of Chemistry—Galway.

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*Further Observations on Chloroform in the Practice of Midwifery.* Part I. By Edward W. Murphy, A.M., M.D., Professor of Midwifery, University College, London. Illustrated with Cases.

Some observations on the use of chloroform in the practice of midwifery were published in January 1848, with illustrative cases; the subject was then new, and not a little obscured by the clouds of controversy that were gathering about it. The writer was anxious to avoid, as far as possible, taking part in these discussions, but, at the same time, was equally desirous to observe its effects both in assisted and unassisted labour. It was desirable to determine not only its nature as an anodyne, but its immunity from subsequent mischief to the patient; and, with these views, he continued to give chloroform cautiously, and to observe its effects according as opportunity presented itself. Three questions proposed themselves for examination. I. Whether chloroform interfered with the action of the uterus during labour? II. Whether the safety of the child was hazarded by its administration? III. Whether any ill effects subsequently manifested themselves by which the safety of the patient was compromised?

The first cases in which chloroform was given by the writer (some of which were published[1]) gave evidence in the negative; since then, however, much injury is said to have been produced by it, and a sufficient number of fatal accidents have occurred from its use, to ren-

[1] Chloroform in the Practice of Midwifery, by Ed. W. Murphy, M.D. Taylor and Walton, London.

der a careful and impartial inquiry into its properties absolutely essential, in order to determine its value. Chloroform, and those who administer it, have been charged with many high crimes and misdemeanours: it is said to cause hemorrhage, convulsions, ruptured uterus, and even "sloughing of the maternal passages from impaction of the head during labour;"[1] and, as to its after effects, "prolonged sickness, sickness with depression following nervous excitement, continuing for days and for months, cases of threatened death from apoplexy, of mania, of bronchitis, pneumonia, prolonged giddiness, loss of memory, paralysis,"[2] form only a part of the catalogue of ills that stand at the head of the indictments. Those also who are bold enough to use it are liable to the charge of committing "an act at once unjustifiable, and which it is questioned if it will not be considered criminal according to law."[3]

It would be a digression from the important question before us, almost equally unjustifiable, if, influenced by such language, the writer were to deviate into a refutation of such charges—if, excited by the spirit of controversy, he were to enter its arena, to rebut and confute, to meet charge by charge, and attack with repulse. However it might serve the purpose of an ephemeral triumph, nevertheless it would lead us very far away from the investigation of truth—it would leave the unbiassed practitioner in doubt, whether chloroform is a valuable agent in the practice of midwifery or not. Besides, the observation of the effects of chloroform in parturition is more important than abstract discussions on the propriety of employing it, and with this view the writer has been more anxious to judge for himself of its true value, by testing it in practice, than to decide whether he should administer it, by the *a priori* arguments that an animated controversy has elicited.

In the selection of cases for its administration, the first class chosen were those in which operations were necessary. In such instances the diminution of the intense pain that generally attends them is an object of importance, the patient is not kept very long under its influence, and there is at least no danger that labour may be protracted. Chloroform was, therefore, given in all cases, with one exception, where operations were performed.

The second class were cases of natural labour. Chloroform was not administered as the rule in all such cases. The writer freely admits that he did not adopt, in these instances, the bolder and more decided course taken by Dr Simpson and others. No doubt he may be liable to the charge of unnecessary hesitation and timidity in exercising so much caution, but he wished to avoid any interference with natural labour that was not perfectly justifiable. In the administration of an anodyne that was new, that was powerful, that had been fatal, it was essential to determine how far it could be used without risk to the patient. To relieve pain is certainly a most de-

[1] Misapplication of Chloroform, p. 62, Gream. [2] Ibid, p. 67.
[3] Employment of Anæsthesia, p. 35, Gream.

sirable object; the pains of parturition are severe and often agonising; but if the remedy for those pains be attended with risk, or if the effects that follow its administration are more injurious to the patient than the consequences of the pains themselves, the use of the anæsthetic becomes an unwarrantable interference. With this view the writer restricted chloroform, first, to cases where there was great intensity of suffering—assuming that the risk of interruption to labour, and other ill effects from such agonising pains, was quite as great as any that chloroform could produce when cautiously administered. Finding in these trials that chloroform not only produced no unpleasant consequences, but in some acted most beneficially, he was encouraged to administer it sometimes simply to relieve pain; and although as yet, for the reasons stated, his experience of it in natural labour is limited, every successive case in which he has used it gives him greater confidence in its value.

Case I.—*Difficult Labour; First Child; Forceps; Chloroform.*

In July 1848, I was requested by Mr Powell to see with him a case of labour in the Old St Pancras Road. It was the patient's first child; she had been thirty hours in labour; the pains were vigorous, but the head made no advance for the last three hours, and there seemed no probability that labour would terminate by the natural efforts. The head presented in the third position, fixed rather low in the pelvis; there was sufficient room to apply the forceps, and we agreed to deliver her. Some chloroform was sprinkled on a handkerchief and placed before the mouth and nostrils, which soon rendered her unconscious; respiration becoming slightly stertorous. The forceps were immediately applied, and the position of the head being changed, she was delivered, without much difficulty, of a living child. She continued asleep for a short time afterwards, the uterus contracted, and the placenta was expelled in about ten minutes. This woman was quite unconscious during the operation—the uterus contracted perfectly without the slightest hemorrhage, and she recovered without a single unfavourable symptom. The quantity of chloroform given was not accurately measured.

Case II.—*Protracted Labour; First Child; Forceps; Chloroform.*

In August 1848, Mr Latty sent for me to see a patient of his, near the Edgeware Road, who had been twenty-four hours in very severe labour. Like the preceding case, there was no appearance of its coming to a termination, and he wished to decide whether she should be delivered artificially. The head was here also in the third position, and rather tightly fixed in the cavity of the pelvis. We agreed to deliver with the forceps. Chloroform was administered through an inhaler, and the patient was gradually brought under its influence. While the forceps were being applied she seemed partly conscious of their introduction; she uttered a complaint that vanished in incoherent ramblings. In about twenty minutes she was delivered of a living girl. In five minutes after, the placenta was detached and expelled from the uterus without the least difficulty. When consciousness was quite restored, she declared that she felt no pain, and was quite unaware that she had made any complaint. About three drachms of chloroform were administered in drachm doses, at intervals.

Case III.—*Tedious Labour: Third Child; Forceps; Chloroform.*

I was sent for by one of my pupils to see a patient in Warren Street, who had been in labour the whole of the previous day and night. She was a delicate woman, and seemed very weak. Her pains were feeble, and she was very anxious about herself. The os uteri was quite dilated, and the head in the

cavity of the pelvis, but the pains did little to advance it; and, under all the circumstances, I was unwilling to allow labour to be longer protracted. Having decided, therefore, to deliver with the forceps, she was given chloroform, which she inspired through the inhaler I am in the habit of using. She was rendered insensible and unconscious, and was delivered of a living girl, without much difficulty. The uterus rather doughy after delivery, but, by firm pressure, it contracted strongly, and expelled the placenta without the least hemorrhage. She suffered no pain during the operation, and recovered without a single drawback. She inhaled about four drachms of chloroform in the same manner as the preceding case.

CASE IV.—*Difficult Labour; Head fixed in the Brim of the Pelvis; Forceps; Chloroform.*

I was requested by Mr Wailes, then my obstetric assistant, to see a case of difficult labour in Warren Street. The patient was in labour of her fourth child; the first was born by the natural efforts; the second was delivered by instruments—she did not know of what kind; the third was natural. In this instance the head was fixed in the brim of the pelvis, and remained so for nearly ten hours; the pains throughout were regular and strong, but were evidently inadequate to accomplish their purpose. I determined, therefore, to apply the forceps. She inhaled chloroform in the same manner as the preceding patient, through an inhaler, and was soon rendered unconscious. Having Dr Haighton's forceps beside me, I applied them, but found no small difficulty in locking them, not from any imperfection in the instrument, but because the spaces in the brim that admitted the blades were not parallel, and it was impossible to bring the blades round from these spaces, so as to lock them. After having withdrawn and re-introduced the blades several times, without success, I resolved to use them without locking. Each blade was held separately as nearly as possible in the proper direction, and extracting efforts being thus made, the head was sufficiently advanced to admit of the blades being altered, and then locked. When the head was brought through the brim, there was no further difficulty. The child, a boy, was delivered living, in about ten minutes, although it occupied nearly an hour before any advance through the brim of the pelvis could be obtained. About six drachms of chloroform were inhaled, and although the operation was very troublesome, and, judging from similar operations, I have no doubt would have been extremely painful, she did not suffer in the least. She was much delighted at her delivery—contrasted this with former operations that had been performed on her, in which the child was still-born, and described it as nothing in comparison.

CASE V.—*Difficult Labour; First Child; Forceps; Chloroform.*

Dr Burke, of Montague Street, requested me to see a patient of his who had been in severe labour for upwards of twenty-four hours. It was the lady's first child; her sufferings were very great; her pains were strong, but the head, which was in the cavity of the pelvis, made no advance for about nine hours. The head was in the third position, so low in the pelvis as to prevent its rotation. The pelvis not being sufficiently large to allow it to pass through in this position, it was arrested. It was agreed to deliver her with the forceps. About half a drachm of chloroform was poured upon the sponge of the inhaler, and renewed from time to time. The forceps were readily applied, and as soon as the position of the head was changed, she was delivered without difficulty. The child (a boy) was apparently still-born; the heart, however, was acting, and by the application of cold to the anterior fontanelle and back of the neck, irritation of the thorax, artificial inflation, and the warm bath, the child gradually recovered. About three drachms of chloroform were consumed. The patient was rendered insensible to pain, but not completely unconscious. During the extraction of the child she bore down with her pains, and when her delivery was completed, she was quite conscious of it, and much pleased with the result. This patient recovered without a single unfavourable symptom.

Case VI.—*Protracted Labour; Second Child; Hysteria; Forceps; Chloroform.*

A patient on the hospital books, living in Melton Place, was taken in labour of her second child, August 2, 1849. She was of an hysterical temperament, nervous, and highly excitable. Labour began on the previous day, and continued actively during the night, but the pains altogether ceased towards morning. The os uteri was, however, dilated, and the head entered the cavity of the pelvis in the third position. Thus she lay, watchful and anxious, expecting the return of her pains, of which there did not seem to be the slightest appearance. She had already taken some ergot of rye, which had no effect. I thought it better, therefore, to apply the forceps than to allow her to remain longer in this state. I wished to bring her moderately under the influence of chloroform, but found great difficulty in getting her to inhale it properly; she was rather excited by it than otherwise, and complained loudly of the least pain in the introduction of the instrument. There was no other difficulty, however, in the operation, and the child (a girl) was delivered living, in about half an hour; when the head had passed she became more tranquil, and then began singing, "Home, sweet home," remarkably well. She soon, however, woke up, asked where she was, but soon recollected herself. She had a confused recollection of having felt some pain, but she said not much. She had either forgotten the pain she really endured, or that which she was conscious of bore no proportion to her expressions during the operation. About three drachms of chloroform were consumed, but it is quite uncertain what quantity she inhaled; very little affected her.

Case VII.—*Very Protracted Labour; First Child; Forceps; Chloroform.*

August 17th, 1847.—I was requested by Mr Scott, of Camden Town, to visit a patient of his in that neighbourhood, who had been in labour from the Tuesday previous (14th instant). It was her first child; the pains had become quite feeble, and had ceased for some time previous to my seeing her. It was desirable, therefore, to conclude the labour. The head occupied the cavity of the pelvis in the first position, and was rather tightly fixed there—a tumour was formed on the head. The forceps were applied, and she was delivered of a living child in about half an hour. Previous to the operation she was given chloroform. About a drachm was poured on the inhaler. At first, when breathing it, the effect was slight. She lay very quiet, but when the forceps were being applied, she complained of pain. Presently, as chloroform was renewed, it took more effect; she muttered her complaints to herself, and then rambled. When the head was passing over the perineum, she called out, "My back, my back," and placed her hand over the sacrum; but when the child was being born, she uttered no complaint, and for a short time afterwards remained asleep. When she awoke she heard the baby cry, evidently with surprise. I therefore asked her if she did not feel pain when she was delivered. She said, "No, I felt none." Then, said I, you certainly expressed suffering; she repeated, "I did not feel any, nor knew that the child was born."

Case VIII.—*Difficult Labour; First Pregnancy; Twins; Forceps; Chloroform; Failed.*

Dr Tilt requested me to see a patient of his who had been in labour for about twenty hours previously. The pains were short and inefficient, and seemed insufficient to expel the head, which had been resting for some time on the perineum. She had been a good deal excited as well by her pains as by rather an animated discussion upon chloroform. The lady was most anxious to inhale it, but her husband and friends were equally opposed to its being used. I was called upon to decide. My friend, Dr Tilt, agreed with me in the propriety of delivering by the forceps, so I had no hesitation about employing chloroform. About a drachm was poured upon the inhaler, which she breathed, but without any effect; while the forceps were being applied, she complained loudly of pain, and

as the child was passing out, she became much excited, but the moment the head passed, she became quite tranquil. As the moderate dose of chloroform did not produce its usual effect, I was unwilling to increase it, as it seemed to cause excitement; it was therefore withdrawn. After the birth of the first child, a second descended, presenting the breech, and was expelled, enveloped in its membranes, by the uterus. Both children were boys. The placenta came away in five minutes after, without any difficulty.

CASE IX.—*Protracted Labour; First Child; Forceps; Dutch Liquid.*

A. B., æt. 36, residing at Salisbury Terrace, Maiden Lane, was taken in labour of her first child. The membranes were ruptured early in the labour, since which time her pains had continued regular and powerful for forty-eight hours. When I saw her the os uteri was fully dilated, the head in the pelvic cavity so tight that I could scarcely feel the ear. I determined to deliver with the forceps, but before doing so requested Dr Snow to aid me in administering the anæsthetic. Dr Snow was desirous of ascertaining the value of the Dutch liquid as a substitute for chloroform, and therefore we agreed to use it. Dr Snow put half a drachm of the Dutch liquid into the inhaler, and the patient began to breathe it. She objected a little at first, on account of the pungency of the vapour, but afterwards inhaled readily, and in about two minutes appeared unconscious, the pupil being turned upwards, and the eyelids firmly closed, and resisting the attempt to open them. I proceeded to apply the forceps; the patient cried out a little, when another half drachm of the Dutch liquid was put into the inhaler, and she became quiet, only muttering and rambling. There was considerable difficulty in extracting the head, which, however, was effected in about half an hour. The child (a boy) was very large, and born alive. As soon as the child was delivered, the patient remained unconscious; the uterus contracted strongly and expelled the placenta. Immediately after, with returning consciousness, she vomited a good deal, and nausea continued for some time afterwards. This patient knew nothing of what had taken place, was greatly astonished to find that all was over, and recovered without a single unfavourable symptom beyond the attack of vomiting immediately after delivery.

CASE X.—*Protracted Labour; First Child; Firm Band connecting Uterus and Vagina: Artificial Os Uteri; Forceps; Dutch Liquid; afterwards Chloroform.*

Mrs ——, a patient of Mr Jakins, of Osnaburg Street, was taken in labour of her first child, April 23, 1849. Her pains proceeded regularly during the day, and a vaginal examination being made, no os uteri could be felt; the pelvis was of good capacity, and the finger being carried as high as possible, encountered a very firm circular band like the os uteri, excessively tense, and as thick as cartilage; beyond this septum nothing could be felt. Mr Jakins trusted that the labour in its further progress would unravel this anomaly. She was seen again in the evening, but there was no alteration beyond a disposition in the pains to recede. The patient passed a quiet night, and strong pains did not return until about noon the following day.

24th.—Matters were in *statu quo;* the pains were vigorous, but the septum appeared thoroughly to prevent the descent of the head. They again slackened towards evening, and she had only a few during the night.

25th.—The pains returned vigorously about noon, and continued for some hours, but were quite ineffective. Mr Jakins, believing delivery impossible without a division of the septum, requested my assistance. I found the septum described by Mr Jakins in front of, and closely applied to, the head, attached to the anterior wall of the cervix, which was thereby expanded over the head, and passing back in a spiral course towards the posterior wall of the vagina. No os uteri could be felt. When a child the patient had received some injury in the vagina, and it seemed as if an adhesion had taken place between the anterior portion of the cervix uteri and the posterior wall of the vagina,

giving rise to this cartilaginous band, and connecting both in such a manner, that the anterior portion of the cervix could not ascend; the whole efforts of the uterus were directed upon this portion of the cervix, causing its expansion over the head of the child. The os uteri could not be traced in any direction; and, under these circumstances, the only course left us was to make a new os uteri, divide the septum, and deliver her. I again requested the able assistance of my friend, Dr Snow, to render her insensible. He first used the Dutch liquid, and afterwards chloroform. He gives the following account of the effect:—"Half a drachm of the Dutch liquid was put into the inhaler at the commencement; this gradually induced a state of unconsciousness, during which the speculum was introduced; the uterine contractions and expulsive efforts continued as before. A little more Dutch liquid was put into the inhaler from time to time, so as to keep up the state of unconsciousness. The pupils were turned upwards part of the time; there was no excitement or muscular rigidity. About three drachms of the Dutch liquid were consumed, which had kept up insensibility for about an hour, but being exhausted, chloroform was used. There was little appreciable difference between its effects and those of the Dutch liquid, except that they were produced by much less inhalation. In about an hour after the commencement of giving chloroform, the patient was delivered. About half a fluid ounce of chloroform was used." The operation was extremely difficult and embarrassing. The finger was passed up to the most depending part of the presentation, and a scarificator passed along it; with this an incision was made through the cervix, and a director being afterwards passed between it and the head, the opening was enlarged; the septum was then cut through, and sufficient room was thus obtained to apply the forceps. The child being small, was easily extracted. It breathed feebly at birth, but gradually recovered itself. Still, however, it remained weak, crying feebly, like a premature child. After delivery, the uterus contracted strongly, and expelled the placenta. There was not the least hemorrhage. The operation occupied about an hour; but a good deal of time was occupied previously in making a careful examination of the relations of all the parts.

The woman remained asleep for some time after, when we left her in the charge of Mr Jakins. He reports that on the following day she was as comfortable as could be expected. The child, however, died on that day. On the third day the breasts became troublesome, and were relieved by hot fomentations. From this time the patient continued to progress daily, and was, when I last heard of her (July 3d), walking about without any inconvenience.

Before this patient was etherized, she complained very much of the vaginal examinations that were made. I feel satisfied that it would have been extremely difficult, if not impossible, to go through the steps of the operation just related, if she had been fully alive to her sufferings; she could not remain sufficiently quiet to prevent risk. It is also remarkable that the uterus was not the least affected by the chloroform.

Case XI.—*Deformed Woman in Labour of her Second Child; Ovate Distortion of the Pelvis; Conjugate Measurement of Brim 2½ Inches; Perforation; Dutch Liquid, afterwards Chloroform.*

A woman, residing in Upper Seymour Street, Euston Square, was taken in labour of her second child, June 11, 1849. She had been delivered of her first by perforation after a very severe and protracted labour, from which she slowly recovered. She was very short in stature, and the lower extremities quite deformed. A vaginal examination being made, the characters of the ovate pelvis were recognised; the cavity and outlet were wide; the sacrum and coccyx strongly curved forwards, and the brim contracted in its antero-posterior measurement to two and a-half inches; the head was above the brim of the pelvis, and the dilatation of the os uteri slight.

June 12.—About two this morning, her pains became regular; and, at four, the

os uteri was dilated. After this, the action of the uterus continued with increased force, powerful pains returning every five minutes until ten o'clock A.M.; the head was found still above the brim, having the funis before it; the pulsations had ceased; the membranes were unbroken. Finding that the child had died, and that the disproportion was too great to admit the head within the pelvis, I determined to perforate, and thus to deliver her. In this case also I requested Dr Snow's assistance, who kindly undertook the management of etherization.

The patient inhaled a little Dutch liquid for the first two minutes, but owing to the limited supply of it, and a fault in the inhaler, she was not rendered quite insensible by it; chloroform was then used, and she was soon rendered first unconscious, then insensible. There were occasional contractions of the uterus and expulsive efforts during the operation; chiefly, at those times when the patient was less under the influence of the chloroform, when inhalation was intermitted. The head was perforated, the brain evacuated, and the crotchet applied. The difficulty of extraction was found to be very great. The crotchet and craniotomy forceps were used alternately, the broken bones of the cranium removed by them, the head itself brought into the brim, but there it remained fixed for some time; at length, after repeated efforts, it began to advance; and when it descended into the cavity of the pelvis, the delivery was soon completed. Some delay, however, was caused by the shoulders. The operation lasted thirty-five minutes, and she was completely under the influence of chloroform for about three quarters of an hour. Six fluid drachms of chloroform were used. After delivery, the uterus contracted strongly, and expelled the placenta in about ten minutes; not the slightest appearance of hemorrhage was observed. When consciousness returned, she expressed great surprise at her delivery; soon after she was seized with vomiting, that continued at intervals through the day.

13*th*.—She has passed an excellent night; pulse 84; has had no vomiting nor nausea; passes urine freely; there is no tenderness of the abdomen. She only complains of slight after-pains, and some smarting in the vagina. From this date, there was no change in the favourable progress of her recovery.

16*th*.—She feels so well that she wishes to get up. Of course, this was not permitted, but nothing occurred subsequently worthy of notice.

CASE XII.—*Premature Labour at Seven and-a-half Months; Arm presenting; Child turned; Chloroform; Great Constitutional Debility.*

M. P., aged twenty-one, a delicate looking woman, had received a letter from University College Hospital for attendance during her confinement. She had lived in London about five months; before that, she resided in the country. She had been in the enjoyment of every comfort until she was sixteen, when she lost her mother, who died insane in an asylum. Her father also died shortly after. She was thus left an orphan. When eighteen, she married, and since that time had to struggle with poverty. She was twice pregnant previous to her present confinement; and, in both instances, miscarried at the sixth month. She says they were "cross births," and that she never afterwards recovered her strength, which she attributed to the want of the proper diet to which she had been formerly accustomed.

January 12*th*, 1849.—Her labour pains came on at the seventh month, induced, perhaps, by additional trials. Her husband was too ill to work, which restricted still more their limited resources, and about three weeks previously her sister, whom she closely attended, died of erysipelas. This caused her great mental anxiety and depression; at length irregular uterine pains came on, which lasted for four days, but never amounted to regular labour pains. On the night of the fifteenth, however, one strong pain came on that ruptured the membranes; they then ceased.

January 16*th*.—Mr Beales, a very intelligent pupil, saw her. He found the os uteri very slightly dilated; there was some hemorrhage, which he arrested, and remained three hours with her. During this time she had no further

hemorrhage, neither had she any pains. Mr Beales then left her for an hour; and, on his return, learned that she had had another strong pain like the former. A vaginal examination being made, he found the arm presenting, and immediately sent for me.

I never met a case that seemed to be so surrounded with difficulties. The os uteri was very little larger than a shilling, just enough to allow the arm through it; the vagina was very much contracted; there was great constitutional debility; and hemorrhage had only ceased a short time before. Great caution was therefore necessary in the attempt to turn the child. About a drachm of chloroform was poured upon the sponge of the inhaler, and applied to the mouth: in two minutes it took effect. She struggled a little at first to avoid inhalation, and then remained quiet; she was not quite unconscious, but, very soon after inhalation, the pulse fell from 90 to 80; the instrument was consequently withdrawn, and only applied at intervals for a very short time. I proceeded to dilate the vagina which made her restless, cry out, and mutter incoherently. The vagina yielded much more readily than I had anticipated; and, in the same manner, the os uteri offered but little opposition to the introduction of the hand, so that in about ten minutes, the child was turned and delivered. It was still-born, and showed signs of commencing putrefaction. Firm pressure applied over the uterus, soon expelled the placenta. After delivery, cold water was applied to the temples, which soon restored consciousness.

The complete anæsthetic effect of chloroform was not obtained; in fact, she inspired very little in consequence of the instrument being so soon withdrawn. She was not quite unconscious. She said she felt some pain from the instruments she supposed we were using. She was not aware that she had been rambling, but said she had a most unpleasant dream. She had been muttering and praying incoherently during her delivery.

17*th*.—She slept but little during the night in consequence of slight uterine pains. Pulse 90, weak; skin hot. Towards evening, the after-pains were more severe; urine high-coloured; pulse 96.

18*th*.—Had but little sleep; pulse 112; skin dry and hot; lochia scanty. At five P.M. there was great tenderness over the uterus; pulse 110, irregular. She was ordered calomel, camphor, and opium, of each a grain every three hours. Late in the evening Mr Beales was hastily sent for, as her friends supposed that she was dying. He found her lying motionless with numbness and coldness of the extremities; pulse 90, weak. Her bowels had been moved four times during the day. He immediately sent for me; and, in the interval, gave her some compound spirit of ammonia—applied warmth to the extremities. She was somewhat recovered when I saw her, and she was given a full dose of tincture of opium in brandy and water.

19*th*.—She slept well during the night, but talked very much in her sleep, and awoke in a fright (when well she had the habit of talking in her sleep, and occasionally had night-mare); pulse 112; breasts full and painful.

20*th*.—She passed a comfortable night, but did not feel so well this morning. In the afternoon she was again becoming incoherent, with dry tongue, and great tenderness on pressure over the abdomen. She was ordered six leeches to each inguinal region. Calomel and opium, a grain each every second hour, and a table-spoonful of the following mixture at the same time.

℞ Mist. camph. ʒviiss.
Spirits ammon. co. ʒiij.
Liq. opii. sed. ʒi.—fl. mixt.

At ten o'clock P.M., Mr Beales was again hastily sent for. He found his patient tossing about the bed in convulsions, muttering to herself. She had been purged sixteen times within the last two hours. A mustard-poultice was applied to the back of the neck, and to the soles of the feet, after which she became conscious; the pills were omitted; the mixture continued, and to have sago with brandy.

21*st*.—She had a quiet night, but during the day, was purged sixteen times —accompanied with great prostration. She would take nothing, wishing, as she said, "To die undisturbed." A starch enema was given her, and, by the careful administration of stimulants, she gradually recovered.

From this date she slowly improved; the diarrhœa was checked by astringents; and by means of stimulants, and proper nourishment, which was then charitably afforded her, she completely recovered.

This case is given more at length because of the unfavourable symptoms that afterwards presented themselves, and as it seems to illustrate a class of cases that the writer believes is not uncommon in this vast metropolis, where the extreme of wealth, of comforts, and of enjoyment, meets the extreme of poverty, privation, and despondency. A single daily meal of toast and tea (bread and water) is perhaps, as in this instance, the only diet; but a much more powerful cause of depression even than this is the intense mental anguish caused, not only by the deprivation of the comforts to which they had been accustomed, but by the chilling influence of indifference to their wants and the prospect of the workhouse. The writer has seen destitution—Irish destitution—but never yet has he witnessed more painful instances of wretchedness than in this, what might be called "London destitution." The effect of chloroform is also worthy of attention. This patient inhaled a very small quantity, scarcely sufficient, in an ordinary case, to diminish pain, yet here it seemed to influence the action of the heart; its pulsations fell from 90 to 80. Pain was not quite removed, but greatly lessened, by it, and the shock of the operation which otherwise would have been great—perhaps fatal—was altogether removed. Chloroform seemed also to induce a more ready relaxation of the vagina and os uteri. Great caution was required in its administration; but, without it, the writer feels assured that the chances of a successful issue would have been greatly diminished, if not altogether dissipated; the patient's recovery, no doubt, was protracted, but, nevertheless, it was complete.

Case XIII.—*Shoulder Presentation; Second Child; Chloroform.*

M. H. having a letter from University College Hospital, was taken in labour of her second child on the evening of August 31, 1849. She slept at intervals during the night, and, on the following morning, the pains became more regular and frequent; the membranes were ruptured in the middle of the day, soon after Mr Humble saw her. He remained with her for some time, and having made an examination, found the arm presenting. He immediately came for me. I returned with him, and found the right elbow protruded through the os uteri, which was dilated to about the size of a crown; the arm was a good deal swollen. The woman was very much taken by surprise when she was informed that it was necessary to deliver her. Her first labour was severe and protracted, but she was delivered by the natural efforts; she thought that this must be similar, and that time would do everything. She was of a violent and impetuous temper, and complained loudly when a vaginal examination was made. No persuasion, therefore, would induce her to submit to what was necessary for her relief; she became quite violent, and would suffer nothing to be done. At length, when she found that I was about to leave her, she consented to inhale a little chloroform, "just to taste it." About a drachm and-a-half was poured on the inhaler, and applied to her mouth; it had no effect. I then sprinkled about two drachms on a handkerchief, and place it before her mouth and nose. She tossed her head about to avoid it, but still it took some effect, as she was quieter although still quite conscious. The inhaler was then resumed, and she breathed it better, so that at length her breathing became slower and deeper, approaching to stertor. I then withdrew the inhaler, and proceeded to deliver her, requesting Mr Humble to apply it to her mouth only at intervals. The left hand was passed readily through the vagina, and the arm brought down; there was some difficulty in dilating the os uteri, but as the hand entered the cavity of the uterus, it met with great resistance, the uterus being so

strongly contracted about the body of the child; the knee was seized, but no reasonable effort could move it; the leg and foot were brought down as far as they would go; and a loop of tape, being slipped over the arm, was pushed up to and over the foot. The hand was then withdrawn; and being very much fatigued, I was obliged to rest. After a few minutes I again introduced the left hand, and succeeded in pushing back the protruding arm. Having twisted the tape in my right hand, I seized the foot with two fingers of that hand, and brought it down gradually to the vulva. The left hand guided, by this leg, was passed to the breech, and being hooked in the fold of the groin, brought it down, but still with difficulty; the child being turned, the body and remaining arm were more easily extracted. The head was tightly grasped by the cervix uteri, and very difficult to remove; at length the child was delivered still-born.

It was very difficult to bring this patient under the influence of chloroform; more than an ounce was used, in drachm and two drachm doses; a good deal was wasted on the handkerchief. She was partly conscious almost to the last; she rambled a good deal; at length quite suddenly lay quiet, no more chloroform was given her, what she had taken having produced a sufficient effect. She did not move nor offer any resistance during the whole of the operation, that occupied rather more than an hour. She remained asleep for about a quarter of an hour after she was delivered; and when she awoke, she was not conscious of what had been done. She said, that "she felt nothing;" she was much surprised to hear that her child was born, but equally disappointed to learn that it was dead. The most remarkable feature in the case was the powerful resistance of the uterus, which presented as much opposition to the operation as if no chloroform had been used. In this, as well as in other points, it forms a remarkable contrast to the preceding case.

In ten of these operations, chloroform alone was used. In two (Cases X., XI.), the Dutch liquid first, and then chloroform; in one (Case IX.), the Dutch liquid alone; the effect of both were similar, only that the Dutch liquid was slower in its operation, and the latter was followed by vomiting in two instances. The quantity of chloroform administered varied. In one case (XII.), a drachm was consumed. In another (Case XIII.), an ounce; its effect also varied. When the patient was fully under its influence, she was perfectly quiet, breathed deeply and slowly, and gave no expression of any kind; more usually, the effect was not so complete. Consciousness merged into incoherency; when not disturbed, the patient lay as if asleep, but when excited by the operation, she muttered complaints, and sometimes cried out distinctly, as she would have done, if she had not taken chloroform. Yet, after the operation, when she regained her self-possession, she recollected nothing about these exclamations, and said she felt no pain. The tonic contractile power of the uterus was not impaired in a single instance. In all, the placenta was separated by the contractions of the uterus alone. In none was there the slightest appearance of hemorrhage. Ten of the children were born living; one was putrid; one was extracted by the crotchet; and one delivered still-born by turning in a case of unusual difficulty. Chloroform (although given in its full dose) did not, in any manner, affect the infant. The subsequent recovery of these patients appears to the writer to be by far the most important advantage from the use of anæsthesia. In some of these cases, the

operations were extremely difficult, and calculated to cause, at least, a protracted recovery. Nevertheless, not a single unfavourable symptom presented itself after delivery, in any of the cases but one (Case XII.), which had no reference to chloroform; and in all, with this exception, the recovery was rapid.

CHLOROFORM IN NATURAL LABOUR.

The next class of cases to which the writer directed his attention in observing the effects of chloroform, were cases of natural labour. In these instances chloroform was given very moderately, and in the majority of them only when the intensity of suffering became so great as to overcome the patient. The mode of its administration was by applying the inhaler that the writer is in the habit of using, to the mouth alone; the sponge generally contained a drachm of chloroform. When the patient felt her breathing affected, the inhaler was withdrawn, and re-applied as soon as she became accustomed to the vapour, and inhaled quietly; the nostrils were sometimes closed to increase its effect. When the pain ceased, the inhaler was taken away, and again applied when it returned. Occasionally, when the patient was much under its influence, two or three pains were suffered to proceed without chloroform, which was not given until they again became intolerable.

Case I.—*Natural Labour; Second Child.*

In June 1848, a lady, whom I had attended before, was taken in labour of her second child. The previous night she suffered very much from her pains; they continued active the whole morning, and when I arrived the os uteri was nearly dilated, and the head presenting. At first I did not contemplate using any chloroform. Labour was allowed to proceed of its own accord; however, in the second stage, the pains were becoming so severe, that I thought myself justified in endeavouring to relieve their intensity. After a few inhalations, she experienced decided relief, and was perfectly conscious of the difference it made in the severity of her pains. My patient was willing to inhale as much as I would permit her to use, and asked for the inhaler the moment that she felt a pain. Not wishing, however, to bring her too much under the influence of chloroform, lest it might interfere with the action of the uterus, I did not always consent; the pains were sometimes allowed to return to their full intensity before it was renewed. The second stage was completed in four hours, about half of which time she was moderately under the influence of chloroform. In the last half-hour, during the expulsion of the head, she inhaled it more freely. She was conscious at the time of her delivery, but felt no pain. The placenta was expelled in five minutes after her delivery. Her recovery was uninterrupted.

Case II.—*Natural Labour; First Child.*

A lady, in excellent health, and of rather robust conformation, was taken in labour of her first child, August 2, 1848. Her pains began after dinner, at first slowly, but soon increased in frequency and severity. In the middle of the night she sent for me. The os uteri was then dilated to about the size of half-a-crown, its margin thin and dilatable, the membranes entire, and the head presenting. There was nothing to call for interference in any way, so labour was suffered to proceed without interruption. The severity of her pains increasing, she became very boisterous, so much so that sometimes her pains were cut short, just as they commenced. Under these circumstances, I wished to try

the effect of chloroform ; she willingly inhaled it, and felt so much relief that I could hardly get the inhaler from her hand. Not wishing, however, that she should inhale more than I pleased, I did not renew the chloroform, but left the inhaler with her as long as she amused herself with the belief that it was doing her good. However, she was soon undeceived by the increasing intensity of her sufferings, and earnestly entreated for more chloroform; more was added, but only on condition that I should have the inhaler in my own hands, to which she consented. At the commencement of every pain she inspired a small quantity of chloroform, which, without removing, always rendered the pain less acute and more tolerable. Sometimes chloroform was omitted for two or three pains, and renewed again. Thus, at the end of about ten hours from the time that chloroform was first administered, she was delivered of a living boy. The placenta was expelled in about ten minutes. Six drachms of chloroform were consumed. The lady rapidly recovered.

Case III.—*Natural Labour ; First Child ; Chloroform.*

In the beginning of February 1849, a lady was taken in labour of her first child. She had passed a very restless night, and towards morning regular labour pains set in. I saw her about noon, she was suffering from short cutting grinding pains, which she was quite able to bear. These pains continued more or less during the day, and towards evening, as the dilatation of the uterus approached completion, they became more severe and frequent. As yet I did not interfere. The lady possessed great fortitude, and although suffering very much, had sufficient resolution to command her feelings. However, as night arrived, the pains were becoming perfectly intolerable—she was beginning to lose all command of herself ; and then, for the first time, I proposed to administer chloroform. Her husband (a medical man) was quite willing that I should do so ; but her next relative, who was already a little imbued with anti-chloroform prejudices, demurred. A consultation was held in the next apartment, and it was agreed that I should be left to my own discretion. Labour had advanced so far that the head was beginning to descend upon the perineum. I proceeded to administer chloroform in the same manner as in the former cases. My patient experienced immediate relief. She still felt her pains, but thought nothing of them. In about two hours from the time chloroform was first inhaled, she was delivered of a living girl. The placenta was expelled in five minutes. About four drachms of chloroform were consumed.

This lady progressed very favourably until the fifth day, when inflammation of the mammæ set in, which ended in suppuration ; her recovery was consequently retarded ; but, with this exception, nothing took place to cause uneasiness.

Case IV.—*Natural Labour ; First Child ; Hysteria ; Chloroform.*

I was requested by a medical gentleman to attend his daughter in her first confinement. Before marriage, she suffered a good deal from hysterical symptoms, headache, pains in the back and mammæ, with some irregularity of the catamenia. Since marriage, her health improved ; but with pregnancy her former symptoms were slightly renewed. On the night of November 2, 1848, she was taken in labour, and I was sent for the following morning. The labour was in its first stage; the pains frequent and rather severe, but still not beyond what she could reasonably endure ; matters were suffered to proceed without any interference. About noon, however, the powerful bearing pains of the second stage were increasing in their severity, and, as in former cases, I found the patient beginning to lose all control of herself ; the pains themselves, also, were interrupted, and becoming irregular. Chloroform was therefore administered ; but this lady did not experience the same relief as in the cases related : she seemed to suffer as much pain as before ; and the intervals between the pains were increased in length. I was unwilling to increase the dose of chloroform, lest it might paralyse the uterus—consequently no more was given her. Labour went on, her pains increasing sometimes to agony.

She was delivered about three o'clock P.M. of a living boy. The placenta was expelled in about a quarter of an hour. This lady recovered in the usual time.

CASE V.—*Natural Labour; First Child; Chloroform.*

A lady, who was rather slightly made, but healthy, requested my attendance on her in her first confinement. She was most anxious that chloroform should be given to her. My patient and her husband were fond of literature, and read even ephemeral productions with avidity. Chloroform did not, therefore, escape their notice; and they fondly hoped that the anticipated labour would be only "balmy sleep," from which the cries of the child were to awake her. I was very anxious to remove this impression, but was not very successful. The premonitory pains of labour came on, November 27, about two o'clock P.M. I saw her at seven in the evening, very cheerful, in anticipation of soon becoming a mother. Slight pains returned at lengthened intervals, which she was quite willing to endure without chloroform. However, as midnight approached, they increased so much in intensity that I could withhold it no longer. Chloroform was administered in the same manner as in the former cases, and with evident relief; the pains were subdued, but not removed. My patient still continued to cry, and bear down with her pains, which (as I afterwards learned) very much surprised her husband, who was present in the room, and not at all prepared for such a result. Labour, however, proceeded very favourably to its conclusion; and she was delivered of a girl at about three in the morning. Chloroform was used for three hours, and about six drachms consumed.

This lady's recovery was most favourable, and she suffered considerably less than she would have done without chloroform; but this was not appreciated by the lady or her husband. Both made up their minds that chloroform was to remove all pain, and, therefore, when pain was still felt, it caused great disappointment. Had I given the full dose, their expectations would have been realised; but, as it was, chloroform lost all its credit, and was clearly proved, in their minds, of no use.

This case, also, may explain the source of some objections that are freely circulated against chloroform,—"That there are but few women who at the present time really undergo the process of etherisation during labour, who are really made insensible to any part of their sufferings: a little management and persuasion will make them imagine they are, and the comparative differences in the character of their deliveries will still further induce them to think so." [1]

CASE VI.—*Natural Labour; First Child; Chloroform.*

A Russian lady, whom I was engaged to attend, sent for me, June 18, 1849. I saw her about three o'clock in the morning, and learned that her pains began the previous evening, and that she suffered severely during the night. I found labour so far advanced that the dilatation of the uterus was nearly completed, and the head entering the cavity of the pelvis. She was very intolerant of her pains, and gave the most agonising expression to her distress. I at once gave her chloroform, and a total change took place; the pains were quieted, and she was much calmer. Previously she cried out bitterly while the pain lasted, and in the interval was still restless, watchful, and complaining: now she bore down with them quietly, and when they ceased fell off in a doze. She was again roused by the pains to a renewal of straining effort, and again slept. The pains came on at intervals of ten minutes, and in about two hours after my arrival she was delivered of a living boy. The placenta was separated and expelled in ten minutes after. About four drachms of chloroform were used. The dose was rather more then I generally administer in these cases. It produced sleep that was only interrupted by the action of the uterus. This lady stated that she felt no pain from the time chloroform was given; the straining efforts seemed to be involuntary, at least she felt no pain then, although she bore down with a deep groaning expiration.

[1] Gream—Misapplication of Anæsthesia, p. 18.

CASE VII.—*Natural Labour; Third Child; Chloroform.*

A lady, who had previously resided in the country, came up to London, pregnant of her third child. She had heard very contradictory opinions about chloroform, but was resolved to use it, if I thought it right. Labour commenced May 5, 1849, in the afternoon, severe pains coming on at intervals of about fifteen minutes. In the evening they were more frequent and prolonged, and towards night assumed the character of bearing pains; the os uteri was about one-half dilated. Her pains were rapidly increasing in their severity; and, although my patient possessed great fortitude, and could well command her feelings, she could not patiently endure these pains. I then gave her chloroform. She experienced immediate relief, and was quite conscious of the difference it made: the pains were not removed, but were muffled by the chloroform. At first I thought that the action of uterus was influenced by it, because the intervals of the pains were lengthened; but this was soon proved to be only a temporary effect. The os uteri was soon completely dilated, and she was delivered of a fine boy within two hours from the time she inhaled chloroform. During the last expulsive pains the full effect of chloroform was induced: she lay asleep while the child was being born, and did not awake for five minutes after her delivery. The placenta was expelled within that time. She recovered more rapidly than after her former labours. About three drachms of chloroform were used.

CASE VIII.—*Natural Labour; First Child; Chloroform.*

A lady, whom I was engaged to attend, was taken in labour of her first child, September 3, 1849. Her pains did not become active until night, during the whole of which she was kept awake by sharp grinding pains. She bore them, however, with great fortitude, and made little complaint. Towards morning they increased considerably in severity; and having made an examination about seven o'clock A.M., I found the os uteri more than half dilated, the membranes entire, the cervix, rather rigid, but quite free from tenderness. Labour was suffered to proceed for some time longer, without interference. Strong pains returned every three minutes, becoming more and more severe. At eight o'clock I commenced to use chloroform. The intensity of suffering was immediately diminished, she experienced great relief, the intervals between the pains were lengthened to five and to ten minutes—that interval was spent in the most perfect repose, and freedom from the aching that previously occupied it. Although the intervals were thus increased, nevertheless the dilatation of the uterus advanced much more rapidly than I expected; it was completed about nine o'clock, and soon after the head descended to the perineum. As the head pressed, the patient groaned with the pain; when it ceased, she slept until the next pain woke her up. When the head was passing the vulva, she seemed to a certain extent unconscious. The consciousness was that of a sleeping person, who will change their position, and avoid what may render them uncomfortable, without having the least recollection that they had done so. Thus my patient bore down and groaned with her pain, but the moment it ceased, slept. The head remained on the vulva about ten minutes, and then passed out with the same groan, but without any of the usual expressions of agony. The moment the head was born, she remained asleep, which was not disturbed by the passage of the body and limbs of the child. The uterus contracted perfectly after the birth of the child, and expelled the placenta in about five minutes. In ten minutes after the child was born she awoke, and heard it cry. She was not conscious of its birth.

The child was born a few minutes before ten o'clock A.M. When the first vaginal examination was made, about three hours before, I did not expect that labour would be concluded until the afternoon; the cervix and os uteri presented the character that I know requires a considerable time before it is completely dilated. I was, therefore, very agreeably surprised to find the dilatation advance rapidly, after the patient inhaled chloroform—it was completed in less than an hour; and in another hour the child passed through, and was expelled from the pelvis. Four drachms of chloroform were used.

Summary of Cases.

No.	Nature of Labour.	Chloroform. Quantity.	Chloroform. Time.	Chloroform. Effect.	Results to Mother.	Results to Child.	Special effects.
1	Forceps case.	uncertain.	30 minutes.	full.	well.	living.	Unconscious; no effect on uterus.
2	...	3 drachms.	25 ...	...	...	...	Unconscious; rambled; no effect on uterus.
3	...	4 ...	20 ...	...	...	...	Ditto; uterus doughy after delivery; placenta expelled; no hemorrhage.
4	Long forceps.	6 ...	1 hour, 10 m.	...	...	...	Unconscious; no effect on uterus.
5	Forceps.	3 ...	20 minutes.	moderate.	...	...	Not quite conscious; no effect on uterus; child, apparently still-born, recovered.
	...	3 ...	30 ...	full.	...	...	Hysterical; cried out with the pains, but recollected nothing about it; no effect on uterus.
7	...	uncertain.	30 ...	...	...	...	Unconscious; rambling; cried out with the pains—said "she felt none;" no effect on uterus.
8	...	1 drachm.	15 ...	none.	...	...	Twins; chloroform excited the pat., and was withdrawn.
9	Forceps.	Dutch liquid, ʒij	30 ...	full.	...	...	Unconscious; no effect on uterus; had vomiting after.
10	Artfic. os ut.	Dutch liq. ʒiij. chloroform, ʒiv.	2 hours.	...	...	born liv.	Perfect unconsciousness; no effect on uterus; child died next day.
11	Perforation	Dutch liq. ʒss. chloroform, ʒij.	45 minutes.	...	...	dead	Unconscious; slight effect on uterus; vomiting.
12	Arm presen.	1 drachm.	20 minutes.	imperfect.	...	dead	Child putrid; rambling; slow recovery, from great previous debility.
13	Elbow presen.	8 drachms.	1 hour.	full.	...	...	Very slow in effect; unconscious; ut. strongly contracted.
1	Natural.	3 ...	4 hours.	slight.	...	living	Conscious of pain, but tolerable.
2	...	6 ...	10 ...	...	...	...	Ditto.
3	...	4 ...	2 ...	...	...	...	Ditto.
4	...	2 ...	1 ...	none.	...	...	Hysterical; chloroform withdrawn.
5	...	6 ...	3 ...	slight.	...	...	Conscious of pain; relieved.
6	...	4 ...	2 ...	rather full.	...	...	Conscious; felt no pain.
7	...	3 ...	2 ...	...	...	...	Conscious; but asleep when child was born.
8	...	...	2 ...	...	...	...	Conscious, but not of the child's birth; intervals of pains lengthened.

In all these cases of natural labour, the patient was conscious; she slept in the intervals between the pains, and was only disturbed by their return. In the two last cases they continued to sleep, or I should rather say to be unconscious, while the child was being born. They groaned, and seemed to aid the expulsive effort; but gave no expression of suffering, and did not know when the child was born. In every instance, the uterus contracted after delivery with its usual power, and expelled the placenta. The subsequent recovery of each patient was rapid, and remarkably free from all those after-pains and restlessness that is so often observed after delivery. In one case alone mammary abscess occurred on the fifth day; but there was quite sufficient cause for its appearance, without the aid of chloroform.

(*To be continued.*)

ARTICLE II.—*On Cauliflower Excrescence from the Os Uteri.* By EBEN WATSON, A.M., M.D., Member of the Faculty of Physicians and Surgeons of Glasgow.

(*Read before the Medico-Chirurgical Society of Glasgow, on Sept.* 11, 1849.)

IN the year 1809, Dr John Clarke communicated to a society then existing in London, "for the improvement of medical and chirurgical knowledge," a paper, in which he described, under the graphic name of cauliflower excrescence, an affection of the uterine neck not formerly adverted to by writers on the diseases of women. The peculiar site of this excrescence, according to him, was "some part of the os uteri;" its early symptoms were not very distressing, hence he had never found it less in size than a blackbird's egg. It had a broad base and a granulated feel, and was insensible to the touch. Its tendency was to involve the rest of the os uteri and the outer surface of the neck. Its growth was generally slow; but in one, and that the most rapid of his cases, it had filled the vagina in about nine months after it was first noticed. During all stages of its existence there was in every case a profuse discharge, at first "like fluor albus," but in a short time clear and watery, so that it hardly coloured linen. Occasionally, however, it became bloody, and contained shreds of white brainy matter, if the patient had exerted herself. The odour of the discharge when watery was faint; but it became very offensive after the escape of blood, whether accidentally or at the catamenial periods, which were seldom affected by the disease. The patients did not generally complain of pain, except from the weight of the tumour when it was large; and after death the excrescence, whatever its former size had been, was found to have almost entirely disappeared, leaving but very slight traces of any morbid state of the uterus.

Such is a brief outline of the disease originally described by Dr

Clarke, to which we shall, in the sequel, restrict the name of cauliflower excrescence. Nor will it seem needless to make this remark *in limine*, when I remind the society of the vagueness with which these terms have often been employed by surgeons. "If," says Dr Gooch, "the surface of the tumour, or even the neck of the uterus, is a little irregular; if blood follows an examination, and the patient states that she has a watery discharge—by which she means little more than that it is colourless—all which are common occurrences in diseases of structure in this organ, the case is sure to be set down as cauliflower excrescence. If these suspicions were accurate," he adds, "this disease would be the most common of the diseases of this organ, yet *the fact is that it is the most rare.* Where we see one case of cauliflower excrescence, we see ten, or even twenty, of common polypus, and fifty of carcinoma, or malignant ulcer of the uterus."

While, however, I believe Dr Gooch to be amply borne out by facts in this opinion of the rareness of the disease, I think he has failed in his endeavour to simplify the subject by asserting the identity of the cauliflower excrescence of Dr Clarke with the vivaces of Levret, Herbiniaux, and some other French authors of an earlier date. In this idea he has been followed by several later writers on the subject; and yet there are certain very remarkable differences which distinguish the two diseases. "Vivaces," says Herbiniaux, "grow from ulcerated surfaces, and cause frequent hemorrhages, which are ultimately fatal. Commonly they remain always *in the uterus*, seldom descend into the vagina, for they have no stalk; the orifice of the uterus is commonly more or less open, and within it can be felt the excrescence; it is soft and less even than a polypus; it often distends the uterus so much that the abdomen is externally unnaturally large, and pressure on the belly causes pain." Such are the words of Herbiniaux, as quoted by Dr Gooch himself, and how different is the description they contain from that previously given of cauliflower excrescence, which always grows from some part, but not an ulcerated part, of the os uteri; which at first is not soft, but firm and consistent; which tends to cover the external surface of the uterine neck, and therefore always projects into the vagina; which leaves the orifice of the uterus undilated, its body unenlarged, and the abdomen free of pain on pressure; which, in fine, gradually exhausts by the profuseness of a watery discharge, and very rarely causes death by hemorrhage.

On examining as extensively as I could the works of the most accredited authors connected with this subject, I have only been able to find the histories of twenty-one cases of true cauliflower excrescence. Two were given in Dr Clarke's original essay; five in the work of his brother, Sir C. M. Clarke, on the diseases of females; and four by Madame Boivin and M. Dugés, in their well-known treatise. Drs Gooch, Ashwell, Simpson, Colombat, Duparque, Lever, Montgomery, Professor Syme, and Mr Safford Lee, have

each recorded a single case of this disease. In addition to these twenty-one cases, I am able to add other two, which have occurred in my father's practice. One of them happened many years ago, and only an outline of it has been preserved. The other is of recent date, and I have myself kept a careful record of its progress from time to time.

The former of the two cases referred to was that of a lady, to whom my father was called when in labour, on account of the supposed abnormality of the presentation. He found the vagina filled with long granular masses, something like fingers, depending from the margin of the os uteri. The head presented, and the child was born alive; but the mother sank, and died a few months afterwards, exhausted by the discharge, which became much increased in quantity immediately after the labour. On inspection, only a few hair-like fibrils were discovered around the os uteri, with the exception of which, the uterus and all the other organs were perfectly natural.

The subject of the second case is a lady, of fair complexion and of stout habit, somewhat past the middle period of life, and the mother of several children. All her labours were natural, and at the full period of utero-gestation.

During the autumn of 1847, she twice had hemorrhage from the womb, and after the last time she had a constant watery discharge, which was ever liable to become increased in quantity, and even coloured with blood, after exertions formerly considered trifling. But, notwithstanding the constancy and profuseness of the discharge, she continued in the active management of a large family and household for about twelve months, without asking medical advice regarding her state. It was not till the 2nd of August 1848 that she acquainted my father with the circumstances just narrated. He then examined her both with the finger and with the speculum, and found a reddish tumour filling the upper part of the vagina. The finger discovered it to be granulated externally, and pretty firm; but the os uteri was not felt, it not being thought prudent to use much freedom with the tumour at this time. Considerable hemorrhage accompanied and followed this examination. In a few days, however, it was repeated, but still more cautiously, by my father and also by Dr James Wilson, who was consulted at his request. Both these gentlemen agreed that the lady was the subject of cauliflower excrescence, and that it would be proper to take means for its early removal. The recumbent position, and frequent lavements of cold water, were for the present enjoined, as well as low diet, and the occasional use of some gently laxative medicine.

On the 29th instant, Professor Simpson (of Edinburgh) and Dr Wilson met with my father and myself at the lady's house. On careful examination, we found that the finger, introduced into the vagina, could be passed beneath the tumour; that the latter could be pressed a little forward; and that then the os uteri could be distinctly felt behind it. The posterior lip was quite healthy, and the growth was confined to the anterior lip of the uterus, which seemed to be considerably enlarged. All were unanimous as to the propriety of removing the tumour, with the lip to which it was attached; the patient, previously well prepared for such a decision, at once submitted, and it was agreed that Dr Simpson should excise the whole mass, close to the body of the uterus.

On the same evening, therefore, at half-past five o'clock, our patient began to inhale the chloroform, preparatory to having the operation performed. She was with some difficulty brought under its influence; but when that had been accomplished, she was placed on her back (with her hips on the edge of the bed), her thighs were separated, and the labia majora were held asunder by means of two bent spatulæ. The operator then fixed the teeth of a long vol-

sellum firmly in the upper part of the tumour, and made traction till the whole of it appeared externally. The anterior lip was then severed from the body of the uterus by means of strong curved and blunt-pointed scissors. On this being done, the uterus immediately slipped back into its place. It was, however, a second time pulled down till the wounded part reappeared, and a suspicious corner of the lip, which had previously escaped the grasp of the scissors, was lopped off, in order to insure complete removal of all the morbid tissues. The hemorrhage during the operation was not so profuse as might have been anticipated.

The vagina was now stuffed with lint, and the patient restored to her original position in bed. She awoke from the anæsthetic stupor about a quarter to seven o'clock; after having inhaled 2½ ozs. of chloroform.

A short interval of sleep followed. She then became very restless and uneasy, with a feeling of constant faintness. Her pulse was slow and feeble. A good deal of blood had trickled from the vagina, but not sufficient to account for the symptoms. About eight P.M., however, part of the vaginal plug was withdrawn, and more efficiently replaced. No further bleeding of any consequence occurred during the progress of the case; but the patient continued to sink, and in another hour no pulse could be felt at the wrist or in the neck; the first cardiac sound was inaudible, the extremities and surface generally were cold, and the expression of the face was very anxious. She had excessive thirst, but no particular pain; and, except in asking for drink, her speech was incoherent. Her restlessness was extreme, and every few minutes her whole frame was shaken by violent retching.

The two evident indications to be fulfilled at this time were, to rouse the vital energies of the patient, and to stop the vomiting. We therefore endeavoured to increase the external heat by frictions and warm applications; while numerous internal remedies—such as brandy and water, sherry, champaigne, soda water, ice, creasote, hydrocyanic acid, small doses of opium, and also of nitrate of silver—were each and all administered, either singly or in various combinations, as thought fit, from time to time; but no good effect followed. The catheter was introduced about midnight, and a small quantity of urine was drawn off.

So great was the dislike expressed by the patient to all the fluids offered her, except cold water, that they even seemed to excite, instead of allaying, the vomiting. We determined, therefore, early on the morning of the 30th, to allow her small quantities of her favourite beverage—pure cold water—and to content ourselves with frequently injecting into the rectum small doses of laudanum, in weak brandy and water. She retained these well, and the vomiting gradually became less frequent and less violent. About six o'clock she had a short sleep, after which she felt much better. Still the retching continued at short intervals, the coldness of the surface remained the same, and the radial pulses were imperceptible. The catheter was again introduced about this time, with the same result as formerly.

During the day (30th) our patient slept in the intervals of retching, which were now of about half an hour's duration. The injections were continued as formerly, and she was urged to swallow some strong beef-tea, which she occasionally retained for a considerable time. By such means her strength became gradually restored, but her pulse, though perceptible from about noon, could not be counted with accuracy. She passed her urine freely during the afternoon, and never afterwards required the introduction of the catheter. During the night she slept pretty well, and vomited only three or four times; but was sometimes incoherent.

Next morning (31st) she felt much better. Her pulse was 160, and of pretty good strength; her skin rather hot and dry; and her tongue furred. She complained much of the distension of the vagina. Part of the plug was, therefore, withdrawn; and her bowels were gently moved by means of a common lavement.

On the 1st of September, our patient expressed herself as feeling almost quite well. She had slept quietly during the previous night, and had not vomited for twelve hours; but she still occasionally felt sick and squeamish. The odour of the confined vaginal discharges being very offensive, the whole remaining part of the plug was withdrawn, and the vagina washed with tepid water, which gave great relief. The discharge was not abundant, and consisted of muco-purulent matter.

From this time our patient steadily recovered; and on the 26th of September the wound of the uterus had nearly cicatrised. She had now no watery discharge, or any other symptom of her former complaint; but she still had a feeling of soreness and dragging in the iliac regions, when she sat upright in bed. Otherwise, however, she was quite well, and was therefore allowed to be removed every evening to the sofa. After this, her strength improved rapidly, and her menstruation became re-established; in fact, our patient soon considered herself perfectly well.

Since the performance of the operation twelve months have now elapsed, and yet there is no re-appearance of the disease in the uterine neck. Nor do I say this merely from the absence of symptoms indicating its return; for she has occasionally been examined per vaginam by my father and myself, and once (about three months ago) by Professor Simpson. When I last examined the patient (now fully a month ago), the following was the state of the parts:—To the touch, the remaining portion of the anterior lip was smooth and roundish, but perhaps slightly harder than natural. The posterior lip remained healthy. The tip of the finger could be introduced into the mouth of the womb, and felt a slight rawness on the inner surface of the root of the anterior lip. Viewed through the speculum, the neck of the uterus seemed of natural colour, and its mouth contained a little healthy mucus. When this was wiped off, and the speculum pressed against the anterior margin of the os uteri, a portion of its inner surface was seen. We then could perceive that part of it was covered with red granulations, which bled slightly as they were touched with caustic.

The lady's general health and spirits are exceedingly good; and her menstruation, instead of occurring every two or three weeks, as formerly, now flows only at the natural periods. In the intervals she is perfectly free from discharge of any abnormal kind.

There is one part of the history of this case which is peculiar to itself, and demands some attention before proceeding further. I refer to the deep collapse, accompanied by vomiting, into which our patient sank soon after the operation, and in which she remained for upwards of twelve hours. I am, however reluctantly, obliged to consider this state as the consequence of the chloroform which she was made to inhale. In the records of other cases of excision of the cervix uteri, in which chloroform was not used, no mention is made of even the slightest shock or collapse as having followed the operation. Almost no pain, and very little hemorrhage, have in general attended its performance, and the patient has often left her bed, or, perhaps, the hospital, within a few days thereafter. In the case whose history has just been read to the society, there was neither pain nor bleeding to account for the collapse. We are, therefore, compelled to consider it the effect of the chloroform which was the only agent in operation capable of producing such a result. The everyday occurrence of a slighter degree of nervous depression, accompanied by vomiting, after the inhalation of chloroform, indicates the tendency

of the drug to act in that manner; and still further, most of the fatal cases of chloroformization have presented symptoms little different from those of rapid and intense nervous collapse.

It would be interesting if one could point out the cause of our patient's having been so seriously affected by an anæsthetic which many persons inhale with at most but trifling inconvenience. The chloroform was as pure as could be obtained in Edinburgh, and was administered either by Dr Simpson himself, or under his superintendence. The lady was strong, and had no tendency to nervous disease. There was, however, one obvious peculiarity in her case, which is worthy of remark, because it was likewise said to have occurred on a late occasion, in which death took place during the inhalation of chloroform. I refer to the fact, that *at first* our patient resisted the influence of the anæsthetic, and all at once fell into the deepest insensibility. She was retained in this state by small doses of chloroform, from time to time, during the performance of the operation; and it is not a little curious that she gradually awoke from the stupor, apparently pretty well. Two hours elapsed without the occurrence of any bad symptom, and then she relapsed into the death-like state formerly described. By assiduous watching, and a judicious use of the remedies which have been mentioned, her vital energies were gradually restored, and she then made a pretty rapid and uninterrupted recovery.

We now turn from this period of imminent danger to the patient, and of anxiety to her medical attendants, to consider very briefly some points of interest and importance concerning the class of diseases to which the cases just read to the society belong. The practical questions connected with the subject are much more easily stated than answered. They especially regard the intimate structure and relations of the cauliflower excrescence with the uterus; the treatment suitable for such cases, and the prospect of ultimate cure of the affection—involving the question of its malignancy or non-malignancy.

The cauliflower excrescence has been examined microscopically in but a few instances. There are, indeed, published accounts of only three such examinations.

In the case recorded by Professor Simpson, in the "Dublin Journal," he says, that "on submitting some very thin slices from the section of the tumour to a powerful microscope in the possession of Dr Reid, it was seen to be composed of a number of cells, arranged in some places in groups, in others in irregular lines. These cells contained each a large nucleus, and the nucleus enclosed several large nucleoli. It may be interesting to add, he continues, that none of the caudate or spindle-shaped bodies described by Müller as often existing in morbid cephaloid structures, were seen in any section examined." Mr T. S. Lee, in the Jacksonian prize dissertation on tumours of the uterus, gives the following description of part of a cauli-

flower excrescence examined by him. "When a piece of the tumour," he says, "the only remains of which were in small detached clusters, was taken and placed in water, it appeared to be made up of a number of villi, apparently attached to a central substance of a more firm consistence. It was composed of nucleated cells of large size, some circular, some oval, and others elongated oval; these contained a quantity of granular matter, and a well defined nucleus, which appeared to contain a cavity filled with a quantity of granular matter. The two together had the appearance of a cell within a cell, or a compound cell. These cells were connected by fine filaments like cellular filaments."

Lastly. Dr A. Anderson has published a microscopical examination of a cauliflower excrescence in the "Edinburgh Medical and Surgical Journal," in which he says,—"The basis of the cauliflower excrescence is a membrane of extreme tenacity, ramifying most complexly, amply supplied with blood, and possessing the power of forming, from that blood, a whitish cell-substance, which is deposited in a layer around it. I conceive, he adds, that the white substance which constitutes the bulk of the structure, is formed by the membrane as a matrix, and from the blood with which that membrane is supplied by a change in the corpuscles."

"In a morsel of the membrane, highly magnified, Dr Anderson could detect a fine fibrous structure of great delicacy, absolutely swarming with blood-corpuscles and cells, to the presence of which great part of its apparent thickness seemed due. Besides the corpuscles which retained their form, there were others apparently undergoing various changes, exhibiting every variety of shape, and mixed with nucleated cells."

In the case of excision which I have to-night read to the society in detail, part of the anterior lip was removed along with the morbid growth from it. The whole mass was about the size of an ordinary lemon, but considerably flatter. The lip had been evidently hypertrophied and elongated; partly, I suppose, from mechanical extension, and partly from the over-supply of blood which flowed through its vessels. Its mucous covering was of normal appearance, except within a few lines of the excrescence, where it was white and thickened, terminating abruptly at the base of the growth which protruded through it, very much as the fungus testis does through the scrotum. The colour of the excrescence was yellowish, and its vascularity considerable.

With the view of preventing, as much as possible, the shrinking of the excrescence, it was immediately hung in alcohol, by which means it has been pretty well preserved, and has not lost much of its original bulk. Its vascular appearance, however, has been entirely removed, and it remains of a light yellow colour. Its texture has also become more of a brainy consistence, and less apparently granular, than formerly. When cut or torn, it appears to be ramified by

tough fibres, which, on closer inspection, we shall afterwards find to be portions of a fine and complexly folded membrane.

A smooth vertical section was made through the centre of the whole mass, and by viewing the cut surface through a good magnifying lens, it could easily be seen to consist, at the upper part, of the ordinary fibrous and areolar tissues of the uterine neck, while towards the lower portion these tissues were mixed with white pulpy matter, which a little farther down constituted the cauliflower excrescence. When a small portion of this pulpy matter, *taken from the margin of the growth*, was placed under the microscope, it was found to be a small pouch or bag of very fine membrane, adhering to the inner surface of which were numerous blood-corpuscles, nearly equal in size, and similar in form. They were seen in irregular clusters, chiefly along lines ramifying after the manner of blood-vessels. I did not observe any cells in the marginal portions of the excrescence, but I saw several in morsels taken from its centre, and some of them had more than one nucleus; but it ought to be remarked, that in no section did I see spindle-shaped or caudate bodies, such as are sometimes seen in cancerous growths.

The important *practical points* to be learned from these accounts of the intimate structure of the cauliflower excrescence, which, it will be remarked, agree in such a manner as to corroborate each other, are chiefly three,—viz., 1st, That the shape of the excrescence is, in some cases at least, nearly pyramidal; the base of the pyramid being uppermost at its junction with the uterus; 2d, That in all cases, the marginal portions of the growth are soft and friable, its centre being slightly more consistent; and, 3d, That its attachment to the uterus is irregular,—not by a smooth base, but by little roots which insinuate themselves between portions of healthy uterine structure.

These deductions from the preceding rigid observations, have an evident and important bearing on the radical treatment of this disease. I do not deem it necessary to speak of the palliative treatment of cauliflower excrescence; for the simple reason that, in all but a mere fractional number of cases, the affection has become too deeply rooted, and too urgently dangerous to life, before medical assistance is asked, to admit of even a hope of relief from such measures. Strangulation or excision are the only methods by which a cure can be attempted; and the great practical difficulty is to choose between them. Now, the preceding observations regarding the structure and relations of the excrescence, tend greatly to show the superiority of the latter over the former proceeding. They show how, by it alone, one can safely and thoroughly extirpate the roots of the growth; while they also show that the ligature is either very partial or very dangerous in its action; how it either slips down upon the more solid portion of the excrescence, and only removes the soft, pulpy matter external to it; or, having been placed around part of the uterine

structure at the base of the growth, becomes an almost surely exciting cause of metritis, probably complicated with peritonitis, and brings the patient's life into the most imminent peril. It is a well-known rule in applying the ligature, in cases of common polypus of the uterus, to place the cord so low upon the polypus as to leave its root unstrangulated, and thus to avoid the danger referred to. The vitality of such tumours is by no means great, and they therefore die above as well as below the ligature. But it is far otherwise with cauliflower excrescences; for when part of them has been nipped off by the ligature, they have invariably grown again with rapidity. In seven cases, the records of which I have examined, the ligature was applied around the cauliflower excrescence. In one of them it occasioned very distressing symptoms, and would doubtless have soon caused death had it not been removed on the sixth day after its application. The patient survived the operation for about four months. The other six cases were more successful; a temporary cure was effected in each case, but relapse occurred in all, and five out of the six perished within periods varying from four months to two years after the operation. In the sixth case the disease returned in about six months after the application of the ligature, but was then excised by Dr Montgomery of Dublin, who examined the patient twenty-one months after the operation, and found her free of the disease. In nine cases in which the excrescence was at once excised, no fewer than six were cured. It is but candid, however, to state, that regarding one of these cases we cannot speak with much certainty; seeing that she was dismissed from the hospital eleven days after the performance of the excision by Mr Syme, and had not been re-examined by him when he published his "Pathology and Practice of Surgery." In the other five cases, however, the patient was examined by the surgeon at periods varying from six months to four years after the operation; and in all of them the os uteri remained healthy. In three out of the nine cases of excision, the disease returned: one after an interval of a single month, a second after twelve, and a third after eighteen months of comparatively good health. In none of the three cases was the operation repeated, and other treatment was unavailing, for all three died. Two of Sir C. M. Clarke's cases were treated throughout by rest in the recumbent position, cold and astringent washes, local bleeding, blisters, &c.; and a somewhat doubtful effect followed. In one of them the tumour is said to have disappeared, but we are not informed of the ultimate result. Five cases on record do not appear to have been submitted to any treatment whatever, and of these four perished during, or shortly after, labour, while one died of the uncomplicated effects of the disease itself. Perhaps these statistics will be more tangible when arranged in the tabular form, as follows.

TABLE I.—*Cases of Cauliflower Excrescence in which Palliative Treatment, or none at all, was used.*

Case.	Years of age.	Position and size of tumour.	Result of Treatment.	Ultimate issue.	Authorities.
1	26	Filled the whole pelvis.	...	Death after labour.	Dr J. Clarke in Trans. of a Soc. for improvement of Med. & Chir. knowledge, vol. iii., p. 325.
2	32	Both lips of os uteri.	...	Death after labour.	Sir C. M. Clarke on Diseases of Females. Part ii., p. 67.
3	...	Two-thirds of os uteri.	...	Death.	Ibid., p. 65.
4	32	Os uteri size of half walnut.	Disappearance of symptoms.	Remained well 2½ years afterwards.	Ibid., p. 105.
5	40	Os uteri size of hen's egg.	Relief.	Doubtful.	Ibid., p. 108.
6	...	Circumf. of os uteri.	...	Death after an abortion.	Dr Lever's Fothergillian prize essay, p. 144.
7	38	Whole circf. of os uteri.	...	Death after labour.	Dr Watson's private case-book.

TABLE II.—*Cases of Cauliflower Excrescence in which the Ligature was applied.*

Case.	Years of age.	Position and size of tumour.	Immediate Result.	Ultimate Result.	Authorities.
1		From one-third of os uteri.	Disappearance of symptoms.	Relapse in a few months, and death.	Dr J. Clarke in Trans. of a Soc. for improvement of Med. & Chir. knowledge, vol. iii., p. 329.
2	60	Both lips of os uteri.	Ditto, and well for two years.	Relapse and death shortly after application of second ligature.	Sir C. M. Clarke on Diseases of Females. Part ii., p. 76.
3	42	Anterior lip size of orange	Well for four months.	Relapse and death	Boivin et Dugés, Maladies de l'Uterus, tom. ii., p. 169.
4	33	Nearly whole os uteri.	Metritis.	Death four months after operation.	Dr Gooch on Diseases of Females, p. 305.
5	40	Whole os uteri.	Temporary cure.	Relapse and death 2 years aft. operation	Dr Ashwell on Diseases of Women, p. 443.
6	45	Whole os uteri.	Well for about 6 mths. and relapse.	Excision performed and patient found well 21 mths. after.	Dr Montgomery, in Dublin Med. Jour., vol. 26, p. 402.
7	36	Whole os uteri.	Disappearance of symptoms.	Relapse in a few months, and death.	Dr Brown in Dublin Medical Journal, vol. vi., p. 30.

TABLE III.—*Cases of Cauliflower Excrescence in which Excision was performed.*

Case.	Years of age.	Position and size of tumour.	Immediate Result.	Ultimate Result.	Authorities.
1	30	Anterior lip, size twice hen's egg	Well for 12 months.	Relapse and death.	Boivin et Dugés, Maladies de l'Uterus, tom. ii., p. 172.
2	28	...	Well for 25 months.	Relapse and death.	Ibid, p. 175.
3	34	Anterior lip.	Cure.	Remd. well 2 years after operation.	Ibid., p. 176.
4	37	Greater part of os uteri.	Cure.	Unknown since patient was dismissed 11 days after operation.	Syme's Pathology and Practice of Surgery, p. 306.
5	33	Posterior lip. chiefly.	Cure.	Remd. well 6 mths. after operation.	Dr Simpson, in Edinburgh Med. Jour., vol., lv., p. 104.
6	...	Whole os uteri.	Cure.	Remd. well 2 years after operation.	Colombat de l'Isere—Traité des Maladies des Femmes, tom.ii.,p. 701
7	40	Posterior lip.	Disappearance of symptoms.	Relapse in about 1 mth. after operatn. and death.	Lee on Tumours of the Uterus, p. 96.
8	34	Anterior lip.	Cure.	Remd. well 4 years after operation.	Duparque—Maladies de la Matrice, vol. i., p. 145.
9	38	Anterior lip.	Cure.	Remd. well 12 mths. aft. operation.	Dr Watson.

It would seem, then, from Table I., that this affection of the os uteri, if allowed to follow its natural course, surely issues in the death of the patient; and that the results of two cases, so far as they can go, do not encourage us to trust to palliative measures in this disease. The nature and fatality of the affection both demand a more efficient treatment. Let us, therefore, compare the results of cases treated radically. We have already adduced 16 such cases; in 7 the ligature was applied, and in 9 excision was performed. The following is a summary of the results:—

Cases of Treatment by	Total Numbers.	Immediate Result of Treatment.	Relapses.	Ultimate Result.
Ligature.	7	One death from Metritis, and six temporary cures.	6	Five deaths, & one cure *by excision.*
Excision.	9	Disappearance of symptoms in all the 9 cases.	3	Three deaths & six cures, one of them doubtful.

We leave these facts to speak for themselves. The undoubted liability of this disease to return after removal of the excrescence, has often been adduced to prove its malignant nature; but the facts just stated, greatly diminish the force of this argument; for they show that such a liability exists chiefly, if not solely, after imperfect removal of the morbid tissues, and that, therefore, it merely proves that part of this tumour can reproduce the whole,—a fact which is quite compatible with benignity. It has already been shown how removal of the cauliflower excrescence by the ligature is almost necessarily imperfect, owing to the structure and attachments of the excrescence itself. Hence, in all the cases of such a proceeding, which I have been able to find on record, a relapse occurred, and the patient died. In cases of excision, on the other hand, by which operation the roots of the excrescence, and part of the healthy uterine tissues are removed along with the depending tumour, only 3 out of 10 relapsed; indeed, since one of these cases of relapse occurred within one month of the operation, I think we may safely class it with the cases of imperfect removal; and thus we should have only 2 relapses out of 9 cases of thorough extirpation. If this proportion remain true for a large number of cases, as most certainly it is correct for those to which I have previously given particular references, this argument from the return of the disease not only does not prove, but actually goes far to disprove, the malignancy of cauliflower excrescence.

In addition to these considerations, we may refer to one or two others, corroborating the conclusion to which we have conditionally come.

1*st*. The progress of the growth is, generally speaking, slower than that of any known malignant disease with which we can compare it, such as encephaloid or colloid cancer, or of uterine hematoma. The early symptoms of the cauliflower excrescence are so very insidious, that it has generally existed for at least a year before medical advice is obtained, and even in cases where no treatment whatever has been adopted, the patient generally survives fully a year longer.

2*d*. The usual age of the patient, in cases of cauliflower excrescence, is likewise different from that of the victims of malignant disease of the uterus. In nineteen cases of the former, the average age of the patient was thirty-seven, and no fewer than *eleven* were below it. Whereas, taking the nineteen first cases of cancer in Dr Hughes Bennett's work on that disease, the average age is rather more than forty-three and a-half years. Boivin and Dugés, from very extended observations, found that the ordinary ages of patients suffering from malignant disease of the uterus, ranged from 35 to 55, and that those belonging to the latter decade were more numerous than those belonging to the former.

3*d*. The cachexia attendant on malignant diseases, especially of the uterus, is generally absent from cases of cauliflower excrescence. In the last stages of this disease, the patient, doubtless, becomes pale

and emaciated, owing to the constancy and profuseness of the discharge under which she is sinking, but it need hardly be remarked that such phenomena are very different from those of that premature decay which we denominate cachexia of malignant disease. It is an interesting corroboration of this remark, that, even in the last stages of cauliflower excrescence, the patient is capable of child-bearing; and, in three of the cases formerly adduced, the patients not only gave birth to living children, but survived their labours for several months. I am not aware that the same could be said of cancer, or of any other truly malignant disease of the uterus.

4th. A not less remarkable distinction between the two classes of diseases which we are now considering, is that, during the whole course of the cauliflower excrescence, pain is almost never complained of by the patient. The discharge alone draws her attention to the disease; and when she is examined by the surgeon, it is found that the tumour itself is insensible to the touch. How different is such a state of matters from that which exists in cancer of the uterine neck! In the early stages of the latter affection, the cervix uteri is highly irritable and sensitive to the touch; and its first and last symptom is lancinating pain of almost intolerable severity.

5th. It might be supposed that the microscope would lend important aid in the solution of this problem; but I fear it must be confessed that histological science has not sufficiently advanced to admit of our relying with certainty on microscopic characters in the distinction of malignant and non-malignant tumours. The cancer-cell is the most trust-worthy sign of the former when it can be ascertained to exist in them, but this is by no means always an easy task; and even the most experienced microscopists have confessed their inability to distinguish it from certain other kinds of cells, such as from young plastic epithelial cells, or embryonal and cartilaginous cells. Little, then, can be argued from microscopical observations of the cauliflower excrescence in regard of its malignancy or non-malignancy, and even that little weighs in favour of the latter opinion. Only one out of four observers, viz., Dr Anderson, saw "caudate corpuscles, "to use his own words," like those Müller saw in cancer and other bodies of various shapes." Mr Lee says he saw "cells within cells or compound cells;" but these are by no means characteristic of cancerous growths, and, from his examination, he concludes that the cauliflower excrescence is "of simple structure, and not malignant." The whole tenor of Dr Simpson's account of the excrescence examined by him indicates his opinion to be, that it is not a malignant growth, and he particularly remarks that he did not observe "any caudate or spindle-shaped bodies," such as are sometimes found in encephaloid structures. In none of the very numerous examinations made by me of all different parts of the cauliflower excrescence in my possession, was there detected any cancer-cell, or other body indicating malignancy of structure. I only found, as formerly explained in detail, a considerable number

of normal blood-corpuscles contained in a very delicate vascular membrane, and also a small quantity of fine cellular tissue in the centre of the growth. It will be perceived, then, that these microscopical investigations, so far from containing anything like a proof of the malignancy of this peculiar affection, rather tend, on the contrary, to establish the opposite opinion.

I have thus adduced cumulative evidence, warranting a conclusion, if not of absolute certainty, at least of the highest probability, that the cauliflower excrescence is not of a malignant nature; and that, therefore, we may advance to the removal of the growth with good hopes of a permanent cure of the disease. There can, however, be no doubt that such hopes depend for their fulfilment very much on the completeness with which the tumour is extirpated; and, with due deference, I beg to state it as my opinion, that the safest and most efficacious mode of accomplishing this end is by free excision of the affected lip, or, if necessary, of the entire neck of the uterus.

ARTICLE III.—*Contributions to the Pathology of the Heart.* By HALLIDAY DOUGLAS, M.D., Fellow of the Royal College of Physicians, Physician to the Royal Infirmary, Lecturer on Clinical Medicine, Edinburgh.—(*Continued from p.* 1046.)

IT will not be difficult to show, that the symptoms which mark the progress of cases of valvular disease of the heart, arise less directly from the valvular lesions than from the hypertrophy and dilatation which so invariably supervene. Further, it appears that dilatation is the more immediate cause of the secondary evils which usually arise in these, just as in other cases. It is probable that hypertrophy has, in the first instance, a counteracting influence on the evils that might arise from the deformed or incompetent valves; but frequently it is attended by those untoward circumstances which have been termed accidental, and with which the valvular lesion is indirectly connected.

CASE IX.—*Contraction of the left Auriculo-Ventricular Orifice; Dilatation of the Heart; Hypertrophy; Death by Apnœa.*

Mrs Forrester, æt. 38, a married woman, of spare development, was admitted, February 10, 1848, with urgent dyspnœa, respirations 36, lips livid, pain, and acute tenderness of the epigastrium; and she had frequent paroxysms of violent palpitations, with vertigo; no cough. The pulse was small, hurried, and irregular; urine scanty, and slightly albuminous, sp. gr. 1·023.

History.—She stated that her health had been impaired only for six months, and that she never had rheumatism, cough, nor hæmoptysis. For three months slight œdema had occasionally existed; palpitation and dyspnœa had been only at times so urgent as to prevent her resting in the horizontal posture.

Physical Signs.—The action of the heart was tumultuous. The impulse

was double—one diffused and more forcible than the other, which was sharp, quick, and slight. The radial pulse was very small and feeble, contrasting with the extent and force of the heart-beat, with which it was not synchronous. The præcordial space, dull on percussion, extended in the vertical direction from the third left cartilage to the costal margin, and horizontally five inches. The sounds of the heart were, at first, and indeed pretty uniformly, hurried, irregular, and confused, with an undefined prolongation and harshness affecting, though uncertainly, the first sound. Subsequently, when the action of the heart moderated, there was audible, in addition to the prolongation, a soft bellows-murmur with the first sound at the apex. The veins of the neck were neither distended nor pulsating.

The hepatic dulness on percussion extended to the epigastrium and lateral region of the belly.

Progress and Treatment.—She was immediately relieved, the tumult of the heart's action was lessened, and she could rest in the horizontal posture, after leeching in the scrobiculus cordis. Dyspnœa, however, did not entirely cease, and shortly afterwards slight œdema was observed; urine also continued scanty. Diuretics acted very imperfectly and without effect on the dropsy, which already showed a tendency to a steady increase.

In the beginning of March, about five weeks before death, cough occurred, and harsh respiratory murmur, with submucous rattle in the inferior dorsal regions. The paroxysms of palpitation became more frequent, with threatening fits of syncope; the heart's action, always more or less tumultuous, was now perceptibly less forcible; and she was unable to leave bed, though she still had occasional intervals of slight comparative comfort. Dropsy soon became general, and her distress from dyspnœa, and the sense of impending death, more unceasing. During the last ten days dull percussion was well marked in both lower dorsal regions; bowel complaint, sickness, and vomiting, and attacks of complete syncope, were frequent; and she died exhausted on the 3rd April, after great and protracted pectoral anguish.

Post-mortem Examination.—There was general dropsy of the cellular tissue and cavities. The heart was increased greatly in bulk; it was globular, and distended in all its cavities; and weighed 15 oz. The left auriculo-ventricular communication was contracted, admitting the forefinger as far as the second joint; both leaves of the mitral valve were thickened and rigid. All the cavities were dilated—especially the ventricles, and the right most of all. The walls of the left ventricle were above the normal thickness; those of the right ventricle were thin, in some parts almost transparent, and membranous. The left auricle contained a dense fibrous polypus. The muscular substance of the heart was soft. The inferior lobes of both the lungs were condensed by hemorrhagic effusion. The abdominal organs were not appreciably altered, except the liver, which was slightly puckered on its surface. The kidneys were pale; the capsules slightly adherent.

Remarks.—The lesions of the heart in the preceding case may be reduced to four:—Hypertrophy, and dilatation; contracted, and incompetent mitral valve. The two former constitute one compound lesion, rather than distinct morbid conditions; and in this, as in less complicated cases (Cases I. and V.), the obstructed circulation was due to the preponderating dilatation, rather than to the hypertrophy. We have, however, to inquire, what influence the diseased state of the mitral valve had upon the progress of the case.

Our patient presented the usual effects of obstructed circulation in an extreme degree; and we have seen in other cases all these effects arising from a preponderating dilatation. If it be argued that the contracted orifice would obstruct the circulation, and that it was

the cause of that state in our patient, it must be admitted that so very great obstruction of the circulation as existed would require a correspondingly great contraction, but the orifice had a circumference of two inches; and cases exist in which the aperture was more contracted, without any such manifested obstruction of the circulation. It is a reasonable inference, therefore, that, in this case, the symptoms of obstructed circulation were due to the dilatation rather than to the narrowing of the mitral orifice.

The dilated right ventricle has been regarded as characteristic of these cases, and as arising directly and necessarily from the contracted left auriculo-ventricular communication. I must demur to this opinion, partly on the ground of this case, in which we find a *less* degree of mitral contraction complicated by the *highest* degree of dilatation of the right ventricle, and partly on the ground of cases to be hereafter recorded, in which hypertrophy of the right ventricle existed. It appears probable that the state of the right ventricle has an important influence on the symptoms and progress of these cases, and that other causes besides the contracted aperture are concerned in the development of hypertrophy or dilatation. These questions will be resumed, in commenting on other cases.

In many respects the features of this case were similar to those we have already seen in cases of dilatation. The steady and progressively unfavourable advance of the symptoms, and the great amount of the pulmonary apoplexy, were especially due to the dilatation, just as in Cases I. and IV.; and in both these respects this case contrasted with others, to be recorded, in which hypertrophy appeared to preponderate. The attacks of syncope, and the tumult of the heart's action, also depended upon the preponderating dilatation.

While, then, it cannot be denied that these effects may have been in some degree due to the contracted aperture, nevertheless it does appear that they only arose and progressed along with the dilatation of the organ, which may be assumed, from the signs and from the anatomical condition, to have had a comparatively recent origin and progress.

Tenderness of the epigastrium is more frequently present in this than in other forms of diseased heart; it is ordinarily associated with the signs of increased size of the liver, and sometimes with obstinate sickness and vomiting; and the benefit of leeching in the scrobiculus cordis is always very marked. The enlargement of the liver is for the most part temporary—probably depending on congestion; it does not usually exist after death.

The physical signs, in cases of mitral contraction, are subject to great variation; these cases exemplify, very strikingly, the general truth that particular physical conditions are indicated not so much by individual signs, as by the association of several signs; for it not unfrequently happens that the most usual and characteristic sign is wanting.

In our patient Forrester, the physical indications resolved themselves into those of the valvular disease, and those of the dilatation and hypertrophy. The signs of dilatation were in some degree obscured by those of the hypertrophy and valvular disease, but they were every day more pronounced in the progress of the case. The extent of the præcordial space dull on percussion, especially horizontally, and the progressively lessening force of the impulse, taken in connection with the general symptoms of the case, and the known tendency, in such cases, to dilatation, justified the opinion that dilatation was considerable. The state of the impulse, which is one of the most important signs of dilatation, was less available in this case, in the first instance, owing to the disproportion between the cardiac and radial pulse; the latter being unduly diminished by the mitral contraction, but the subsequent decline of its force afforded the desired information. The signs do not enable us to distinguish very precisely the cases in which the right ventricle is so largely dilated; the extension of dull percussion to the right side of the sternum affords some information, but this occurs also when hypertrophy of that ventricle exists. This difficulty is, I believe, of less importance, as dilatation of the right ventricle, is, so far as I have observed, usually accompanied by a similar state of the left ventricle; and this lesion is readily recognised from its greater influence on the heart's action and the altered state of the systemic circulation.

The state of the pulse in this case was quite distinctive of the mitral contraction. The most important of the signs of the pulse consisted in the *disproportion* between the *force* and extent of the heart's impulse, and the *smallness* of the radial pulse. The disproportion exists in every case of contracted orifice; and it is appreciable, even in cases, such as the present, in which the force of the heart's impulse was diminished by dilatation. It acquires additional value as a sign of this form of disease from the fact, that it is always permanent, continuing even when the patient is in his best state. The irregularity of the pulse and its want of synchronism with the impulse of the heart, which existed in Forrester's case, is frequently wanting; and it was, I believe, due, in her case, in a great degree, to the dilatation, and in some degree to the mitral incompetence.

It was difficult to appreciate the state of the heart's sounds; and it was still more difficult to report or state in writing their actual condition. This difficulty was owing in part to the tumultuous state of the heart's action, and in part to an undefined vagueness of the sounds themselves. When the action of the heart moderated, the murmur of an incompetent mitral valve was appreciable, and the harsh grating state of the first sound, though it was vague, uncertain, and difficult to appreciate, was very important, because there are many cases, such as this, in which the murmur indicative of the mitral contraction is imperfectly formed, and amounts merely to this vague harsh prolongation, which owes its diagnostic significance to the facts, that it is audible in the lower part of the cardiac space,

that it accompanies the commencement of the first heart-sound, or rather precedes it, and appears at first, as it were, to accompany the second sound, and that it has a *harsh* grating, and not the *soft* bellows, tone. This undefined grating state of the sounds, in connection with the disproportionate smallness of the arterial pulse, is conclusively indicative of the mitral contraction, and is not met with in other forms of disease of the heart.

It has been already stated that the contracted valves are also usually incompetent; it therefore happens, in these cases, as in our patient Forrester, that a bellows murmur with the impulse accompanies the confused or grating state of the sounds arising from the contraction. This is probably the cause of the erroneous description of the sounds, in contracted apertures, given even in standard works; and it certainly increases the difficulty of analysing the sounds in practice. There are two distinctions between the murmurs accompanying these two forms of disease;—that of regurgitation, or incompetence of the valves, accompanies the impulse and termination of the first sound,—that of contraction precedes, or accompanies, the commencement of the first sound;—the murmur of regurgitation is almost without exception of a *soft* bellows character—that of contraction, is as invariably *harsh* and grating. In practice, there are difficulties in recognising these distinctions, owing, on the one hand, to the altered state of the heart's action and the deranged rythm, and, on the other hand, to the very varying degrees of the *harshness* or *softness* of these murmurs. There is no difficulty in explaining the rationale of these morbid sounds—their rythm and character, and the causes of these—the sole difficulty is in their practical recognition; this is to be *done* only by the accustomed ear, and much aid arises from the collateral and co-existing signs.

I have very frequently found, in cases of largely dilated right ventricles, that the veins of the neck were permanently gorged, and, still more frequently, that they pulsated irregularly. This is, in some measure, due to the widening of the auriculo-ventricular communication which so invariably occurs in dilatation of the right cavities of the heart. The unaffected state of the veins in Forrester's case is therefore the more worthy of notice; for, in her case, the dilatation of the right side of the heart was very decided; and it may, perhaps, be explained by the thin and feeble state of the muscular substance of the right ventricle.

CASE X.—*Contraction of the Left Auriculo-Ventricular Orifice; Hypertrophy of the Right Ventricle of the Heart; Death by Coma.*

Mary Fyffe, æt. eighteen, was admitted on the 30th of January 1849. She was a well-developed girl, unmarried, and employed as a mill-worker. Her lips were livid; she had slight dyspnœa; and there was œdema of the face and feet. She suffered from epigastric pain and tenderness; and she had trifling cough, without expectoration.

History.—She stated that, for two years, she had been liable to attacks of dyspnœa and palpitation, with general uneasiness, which unfitted her for work.

In the intervals of these attacks she had enjoyed comparative comfort; hæmoptysis had never occurred; and she had never had rheumatism.

Physical Signs.—The radial pulse was small and irregular, and contrasted with the extent and force of the heart's impulse. The impulse of the heart extended to the entire præcordial space; and it was not proportionately forcible; —the impulse and situation of the apex was undefined. The space of præcordial dull percussion measured five inches in the horizontal, as well as the vertical diameter; and it extended an inch to the right of the mesial line. The sounds of the heart were confused. The impulse was accompanied by a bellows murmur which was less distinct upwards, from the apex over the region of the ventricles; the first sound was also prolonged, harsh, and grating.

The sounds of respiration were harsh, with some sibilant wheezing.

The hepatic dulness on percussion extended a hand's breadth into the abdomen.

Progress and Treatment.—She was leeched, and the bowels were freely acted upon, with immediate relief. The urine was obstinately scanty—10 oz., and faintly albuminous, with a density of 1·022.

On the eighth day after admission, in the morning, she complained of slightly indistinct vision, and of dyspnœa; twelve leeches were applied to the region of the heart; but she rapidly lapsed into a state of coma, which, *at noon*, was complete. The face was congested, and her lips livid; breathing was occasionally and slightly stertorous—10 in the minute, and at times interrupted for several seconds; the pulse was imperceptible. The heart's action was more forcible than previously, and regular. She was bled to 15 oz. from the external jugular vein; and, in the course of an hour, the stupor had, in a great degree, passed off; but, in the course of the afternoon, fatal coma suddenly supervened.

Post-Mortem Examination.—The volume of flesh was normal. There was slight general dropsy.

Head.—The subarachnoid veins were full, not distended; the lateral ventricles of the brain were nearly empty; and the consistence of the cerebral substance was normal.

Chest.—The heart was increased in bulk, and weighed 15 oz. The relative size of the ventricles was reversed; the right, being the chief seat of hypertrophy, formed the rounded apex of the heart. On being laid open, this ventricle did not collapse; the walls were four to six lines in thickness; and the muscular bundles within were unusually large and distinct. The capacity of the ventricle was slightly increased; and it was distended by a loose coagulum. On the leaves of the tricuspid valve, there were numerous minute fleshy excrescences. The left ventricle formed, as it were, a mere appendage to the right; its cavity was slightly larger than natural; its walls were scarcely thickened. The left auriculo-ventricular communication was contracted, so that it admitted the point of the little finger with difficulty; the margins of the orifice were ragged and calcareous. Both auricles were dilated and distended by coagula. The pulmonary artery presented several atheromatous patches.

The lungs were slightly adherent, otherwise normal.

Belly.—The spleen weighed 12 oz., and was firm and fleshy.

The kidneys were congested. The cortical portion was pale, not manifestly granular; the tubular pyramids had the usual dark colour, and were bounded at their base by an abrupt margin.

Remarks.—The leading facts in this case were the manner of death, and the hypertrophy of the right ventricle; in both of these particulars, as well as in the state of the left auriculo-ventricular communication, which was contracted in the highest degree, the case contrasts with the preceding one. In cases of contracted orifices, as in other forms of diseased heart, death occurs in various ways by

apnœa with progressive exhaustion; or suddenly, with coma, apoplexy, or syncope. Each of these modes of death has reference to other circumstances than the mere state of a particular orifice or valve, and depends to a great extent upon the condition of the muscular substance of the heart.

The powerful influence which the state of the right ventricle exerts on the progress of cases of diseased heart, is indicated, but, indirectly, by such cases as case IX. (page 1196), in which the dilatation of the left ventricle makes it difficult to determine how far the obstruction of the circulation was due to the dilatation of the right ventricle; or whether the dilatation of the left ventricle may not have been more directly influential in causing it. The present is the only case I have met with, in which so great hypertrophy of the right ventricle existed, without hypertrophy of the left to any great degree; and though we cannot attempt to prove, from a solitary case, that the coma was due to this lesion, still the case, as well as others more or less similar, in which the organic lesions usually accompanying the apoplectic state were wanting, goes to indicate that extreme hypertrophy of the right ventricle may induce suddenly fatal coma. At all events, the contrast in these two cases (IX. and X.) deserves remark; in the former, we have the conjunction of dilatation, especially of the right ventricle, and permanently obstructed circulation; while, in the latter, we see hypertrophy involving chiefly the right ventricle, associated with sudden fatal coma.

There is a well-known class of cases, to which the present one appears to belong, in which the coma is unexplained. The coma occurs in the course of some cases of renal disease associated with hypertrophy of the heart, and without any such state of urine as might explain it; and I have met with temporary amaurosis in the like circumstances. The invariable existence of hypertrophy in these cases indicates a connection between this state of the heart and the coma, though it be unexplained. In the case of our patient Fyffe, I do not see any explanation of the coma, except the hypertrophy of the right ventricle; and other cases present a corroboration of this apparent connection. The effect of the blood-letting in this case proves its suitableness; and, in the cases of renal disease above referred to, I have met with even more decided benefit from bleeding. The blood-letting must ordinarily be pushed with greater freedom than the generally feeble state of the patient, or the diseased state of the heart itself, might appear, at first sight, to admit of.

Apart from the unfavourable, and, as it may be termed, the accidental effects of hypertrophy of the right ventricle already referred to, the present case, and the contrast of its progress with that of the one (IX.) preceding, indicates that, while the dilatation of the one aggravated or developed the evils arising from the valvular disease, the hypertrophy in this case had the effect of counterbalancing the obstructive effects of the contracted orifice. The facts on which this statement is founded, are, the greater length of time during

which the patient endured, and frequently recovered from paroxysms of cardiac distress, and the completeness of the respite she enjoyed during these intervals; while, in case IX., the symptoms once fairly developed, the progress was almost uninterrupted and unfavourable;—again, but for the counterbalancing influence of the hypertrophy in case X., the obstruction of the circulation ought to have been greater than in case IX. (Forrester), because the supposed obstructing cause, the contraction of the mitral orifice, was very much greater. Accordingly, we see a very different anatomical state of the lungs in these two cases. Pulmonary apoplexy has been supposed to be intimately connected with contraction of the mitral orifice, like obstructed circulation generally; but we find, in the case in which the contraction was greatest, neither hemorrhagic condensation of the lung, post-mortem, nor, according to the statement of the patient, had she ever had hæmoptysis; while, in case IX., both conditions existed—the dilatation being greater, though the valvular obstruction must have been less.

The heart was virtually free of dilatation; for, though the right ventricle was increased in capacity, the hypertrophy was much greater in proportion. The effect of this hypertrophy of the right side was to give the heart nearly the same globular form as occurs in dilatation, but its general bulk was less; and the tricuspid valve and pulmonary artery were in a morbid state rarely met with in the right side of the heart.

Such cases as the present are apt to create confusion, in reference to the signs of hypertrophy and dilatation. Our notions of the physical signs of hypertrophy are gathered from a very different class of cases, in which the left ventricle is chiefly involved. In such cases the effects of the hypertrophy and the physical signs are more manifest, because of the greater amount and degree of the hypertrophy, its more direct influence on the systematic circulation, and, its situation being more anterior, when the left ventricle is affected, the cardiac impulse is more directly influenced. Hence, from the physical signs alone, the hypertrophy was very imperfectly indicated in this case. Indeed, so far as these signs go, they bear a close resemblance to those of dilatation. The horizontal extent of the space dull on percussion; the dulness on the right of the sternum; and the state of the heart's impulse, which was not forcible in proportion to the extent of the præcordial space:—these signs were not to be distinguished from those observed in cases of preponderating dilatation. It, therefore, becomes necessary to take into account the general condition of the patient; and in our case we saw that the circulation had not suffered any such obstruction as would have existed had the above-mentioned physical signs depended on dilatation.

The anormal state of the pulse and heart's sounds depended, just as in case IX. (p. 1196), on the contracted and incompetent condition of the mitral orifice.

CASE XI.—*Contraction of the Left Auriculo-Ventricular Orifice; Hypertrophy of the Heart; Dilatation; Chronic Bronchitis; Pleurisy; Death unexpected.*

John Glendinning, æt. 22, was repeatedly under observation from March 1845 till the time of his death, 23d November 1848. He was a young man of slender frame, but well developed, except in the chest, which had an irregular prominence in front, and he had permanent venous injection of the face.

He stated that cardiac symptoms had existed for about a year previously, so that the progress of the case extended over a period of five years. He was originally employed as a gardener; during the latter years of his illness he attended to a turnpike gate, on a road not much frequented.

He was placed under my care, in March 1845, by Dr Richard Mackenzie, with the usual symptoms of obstructed circulation—viz., intense dyspnœa, œdema of the feet and ankles, distension of the belly, &c.; and he was subsequently liable to similar attacks, though they were not usually so severe nor so enduring as this one. The leading symptoms of these attacks were *dyspnœa*, with congestion of the face, great substernal distress, palpitation, vertigo, and the sense of impending suffocation; acute *epigastric tenderness*, with extended hepatic dulness on percussion, tympanitic fulness of the belly, and vomiting; *œdema* of the feet and ankles; *scanty urine;* and *cough*, which was not severe until latterly. These attacks occurred for the most part when the weather was cold and changeable; they lasted at times only a few days, sometimes two or even three weeks.

He always derived benefit from rest; leeching in the scrobiculus cordis invariably relieved the distress and pain of the upper part of the belly; laxatives, expectorants, and diuretics, were also used with various effect; diuresis was induced very uncertainly, but always relieved him.

The intervals of the attacks were of very various duration; a second attack rarely occurred within two or three months; and during 1847 he had very long-continued respite. Latterly, even when in his best state, a slightly-laboured breathing existed permanently.

Physical Signs.—The præcordial region was unusually prominent. The impulse of the heart was visibly extended and forcible; the apex beat in the fifth intercostal space, four inches to the left of the mesial line. The veins of the throat were distended and pulsated. The præcordial space dull on percussion was four inches square, and extended to the right side of the sternum. At the seat of the apex, a strong vibration, or fremitus (purring tremor), was communicated to the hand, not corresponding precisely with the impulse. The sounds of the heart were confused and undefined, especially over the ventricles. At the apex they were superseded by a double murmur—the one harsh, grating, and vibrating; the other, soft and blowing, distinctly accompanied the impulse of the heart. At the base of the heart; the first sound was faint, the second was perfectly pure. The radial pulse was irregular, not synchronous with the heart-beat, and small—contrasting with the extended forcible beat of the heart.

During 1847 and 1848 (the last two years) the cardiac signs underwent great change. The harsh grating murmur ceased; and the impulse of the heart fell to forty in the minute; it was forcible, corresponding to the radial pulse, and each beat was accompanied by a soft bellows murmur; in the interval of these beats, a quick tumbling action was *audible*, scarcely perceptible to the hand, and not causing any arterial pulse. Latterly, the irregularity became less intermittent, the tumbling action ceased; the impulse had a more uniform force, but was still very irregular in frequency, as well as in size—about 90 or 100 in the minute.

Termination.—His death was unexpected. He had been well for some months, and on the 22d November 1848, he sent for Dr Buchanan, who found him in great distress, with the usual symptoms of his attacks; he had been unable to lie down for two nights. The usual treatment was commenced;

but he sank unexpectedly during the morning of the 23d. No precise account of the symptoms at the last could be procured from the attendants.

Post-mortem Examination.—The volume of flesh was rather small; the face was livid; there was no dropsical swelling.

Both pleuræ were extensively adherent; some recent soft lymph and serous purulent fluid existed on the right side. Both lungs were voluminous and emphysematous—the right more so; they crepitated everywhere, but were slightly gorged with blood in the lower lobes.

The heart, while still in the body, appeared of great bulk, owing to the distended state of the auricles, especially the right; it had an oblong globular form, and its rounded apex was formed by the right ventricle, which was much hypertrophied; the left ventricle also was hypertrophied, but less in proportion—weight, 20 ozs. The wall of the left ventricle had an average thickness of eight lines; that of the right was also thickened, but the increase of its substance was in a great measure due to hypertrophy of the reticulated and fleshy columns within, which were unusually prominent and distinct. The ventricles were not distended, though both contained large discoloured clots. The auricles were dilated and distended by loose clots. In the aorta and pulmonary artery there were insignificant atheromatous deposits; the sigmoid valves were normal. The left auriculo-ventricular communication admitted only the little finger; viewed from the auricle, it formed a rigid calcareous ring. The leaves of the valve were opaque, thickened, and rigid, as were also the tendinous cords.

The liver was enlarged and congested. The spleen was of fleshy consistence, and weighed 18 ozs. The kidneys were congested.

Remarks.—The chief value of this case arises from the fact that it was under observation for a long period, and the progress of the disease was carefully noted. It presents a very close resemblance to the preceding case; and appears to confirm the supposition that hypertrophy has a powerful counteracting influence upon the obstruction arising from the valvular disease.

In connection with the great degree of hypertrophy in these cases (IX. and X.), which to such an extent involved the right ventricle in both, it is to be remarked that both were liable to paroxysmal attacks of great severity; that these appeared very amenable to treatment, and an interval of very complete cessation of cardiac distress occurred; that in both the duration of the symptoms was great—in one above two years, in the other above five; that in both cases the death was sudden, though the evidence is imperfect that the mode of death in Glendinning's case (XI.) was by coma, as in Fyffe's (Case X.); and, lastly, we find in both an entire absence of hæmoptysis and pulmonary apoplexy. The agreement of these two cases in these particulars is scarcely more remarkable than is the contrast presented by the pulmonary hemorrhage and the uninterruptedly unfavourable progress of Forrester's case (IX.), which we hold depended mainly or altogether upon the dilatation.

The remarks made upon the physical signs in the two preceding cases are for the most part applicable to this case. It is farther to be observed, however, that this case presented a sign which was not ascertained to exist in either of the other cases. I refer to the grat-

ing vibration, or *fremitus*, perceptible at the apex. As appears from these cases, this sign is wanting in many instances; moreover, the history of the present case shows, that the sign occasionally ceases in a given case; and there are cases in which it is developed only when the action of the heart is excited. It is in many cases difficult to determine the precise rythm of the fremitus—*i. e.*, its relation to the sounds of the heart. It no doubt precedes the contraction of the ventricles, but it is difficult to separate it from the impulse.

This sign, when it exists, is significant of the contracted mitral aperture, so far as I have seen; though it is possible to conceive other physical conditions of the heart capable of inducing a fremitus in this situation. I have never observed it in the incompetent state of the mitral valve. In the same way, I have been in the habit of connecting the fremitus, which sometimes exists at the base of the heart, with pericarditic friction; for a distinctly appreciable fremitus in this situation scarcely occurs in other forms of disease. A fremitus at the base of the heart does exist in rare cases of incompetent aortic valves, but very rarely, and these cases are easily distinguishable. These observations have no reference to the fremitus, or "purring tremor," which exists in certain cases over the great vessels of the neck, and at times on the upper part of the sternum—the value of which, as a sign of disease, I am disposed to think has been overrated.

In the progress of the case, several of the physical signs and the action of the heart underwent changes, to which I wish to direct attention, as they are recorded in the narrative of the case, without any attempt to explain them.

The pleurisy, which during life had been latent, and was brought to light only post-mortem, was not such as to influence the fatal result very materially; though, taken in connection with the bronchitis, it gives some ground for doubting that death occurred, as in Case X., by coma, and renders it a plausible conjecture that exhaustion, influenced by these complications, and possibly inducing death by syncope, had occurred.

The complications caused some difficulty in the treatment; the chronic bronchitis was no doubt intimately connected with the paroxysms; it was always aggravated at the time they occurred, and was referred, for the most part, to unfavourable changes in the weather, and to exposure. The relief of the bronchitis was, then, always a leading object, and this was effected by ordinary means. In attempting to relieve the embarrassed state of the circulation by leeching, which always proved more or less effectual, it was found at the same time to relieve the epigastric pain and the vomiting, though, on some occasions, the latter did not yield until the further administration of bismuth, with or without Dover's powder, which always had the desired effect.

(To be continued.)

ARTICLE IV.—*Observations on the Urine in Cholera.* By JAMES W. BEGBIE, M.D., formerly Senior President of the Royal Medical Society.

ONE of the most invariable and characteristic symptoms of Asiatic cholera, is the entire, or almost entire, suppression of the urine. The examination of the characters presented by that secretion, at the period of its restoration after the collapse, is, therefore, an object of much importance. The changes undergone by the urinary secretion in the course of cholera form the subject of the following inquiry.

If the entire suppression of the urine be looked upon as a sign of most unfavourable import, its re-appearance may be regarded as in the highest degree favourable, for in very few cases will ulterior bad symptoms manifest themselves after a copious discharge of urine has occurred.

The mode of fatal termination in many cases of cholera has satisfactorily shown, that the retention of those matters in the system, which it is the province of the kidneys to eliminate, may be regarded as one great danger to be apprehended, and, if possible, avoided. That this danger is not equally imminent in all cases in which the urine is suppressed, we know, and that it should not be so, we can easily understand; for chemical examination of the fluids passed from the bowels has pointed out, that, after the kidneys have ceased to perform their function, the urea may find an exit by the intestines. After a time, however, this channel fails, and the morbid matter being retained, death in the way adverted to is rendered almost certain; or, at all events, the case presenting these features assumes a still more alarming aspect. That death, in the way of coma, attributed to this cause, should take place so much more rapidly in cholera than in any other disease, is, I think, to be accounted for in the fact, that the nervous prostration prior to the occurrence of symptoms threatening coma, has been excessive, and that consequently the action of even a smaller amount of morbid matter is rendered more speedy and certain.

These preliminary observations will suffice to show the importance of attention being given to the urine in cholera; while, I trust, that the facts now to be communicated will be considered as proofs of the interest attending its examination.

Through the kindness of Dr Robertson, physician to the Cholera Hospital, I have had ample opportunities afforded me for making repeated and careful examinations of the urine in this disease. From these examinations, tables of the various characters and phenomena presented by the urine were formed. The tables were placed at the disposal of Dr Robertson; what is now to be said in regard to the urine being deduced from these records by myself.

The urine examined was, as a general rule, that first passed by

the patient after recovering from a state of collapse. In one or two instances the urine was withdrawn by the catheter during life, and in a few by the same means after death. The characters presented by the urine I shall notice in succession, and on some of these, in concluding, offer a few observations.

1st. As to the appearance and other external characters. The amount was almost invariably small, on several occasions so small as to prevent the density of the fluid being ascertained. The appearance of the urine varied greatly, and no accurate statement can possibly be made as to the most common colour. A large proportion of the urines deposited a distinct sediment, others were muddy and opaque, but presented no evident deposit, very few were perfectly translucent. The odour was not characteristic; in a few it was ammoniacal.

2nd. The average density was considerably lower than that of healthy urine; for, though it varied greatly in different cases—the highest examined being 1·045, and the lowest 1·007—the largest number were at 1·012, and the succeeding at 1·016, 1·018, 1·020, and 1·014. Of twenty-two specimens, the densities of which were ascertained more than once, in five the specific gravity had risen a few degrees at the second examination, in nine it had fallen, and in eight it continued the same; while, in two of the nine, it is noted that, at the third examination, it had again increased.

3rd. In thirty-seven out of seventy-two specimens in which the reaction of the urine was ascertained, it was noted as acid; in nineteen it was strongly, highly, or powerfully acid; in three it was faintly acid; in seven it was neutral; in four it was alkaline; and in two strongly, highly, or powerfully alkaline. On subsequent examinations of several of these urines, the reaction was found unaltered; while in one or two of those which presented at first an acid reaction, it was found to be neutral or alkaline; and, on the contrary, in a few of those in which, on first examination, the reaction had been alkaline, it had become acid. It is worthy of note, that in at least one of the latter class the urine was in the first instance withdrawn by means of the catheter, symptoms of retention having manifested themselves; in this there probably existed a sufficient cause for the alkalinity of the urine.

4th. Of sixty-seven urines tested for the presence of albumen, by the application of heat and the addition of nitric acid, in sixteen the urine was noted as being coagulable, or decidedly coagulable; in seventeen as being highly or powerfully coagulable; in twenty as slightly or faintly, or very slightly or very faintly, coagulable. In fourteen there was no coagulability.

In a number of those in whch albumen existed, subsequent examinations were made at an interval of one or two days, with the effect of finding that at the second examination in most it had partially disappeared, and that in very few it was present on the third. In one or two instances the first urine voided was not coagulable, but a subsequent examination detected a very small amount of

albumen. In all the albumen, though present at the first examination, even in large quantity, was but transient in its duration.

5th. In twenty-eight out of sixty-eight specimens, it was noted that bile existed, or was decidedly present, or that its existence was characteristically displayed upon adding a few drops of nitric acid. In fifteen others there existed a slight trace of bile, or an equivalent term was used in expressing its presence. In twenty-five the addition of nitric acid gave no indication of the existence of bile. In several, bile was found on subsequent examinations, and in two was found to manifest its presence for the first time on the third examination. In most, though undoubtedly present, its duration, like that of the albumen, proved short.

6th. In eighteen specimens of urine the amount of urea present was ascertained. The mode adopted was the following:—A thousand grains of urine were first accurately weighed, and then evaporated to the consistence of an extract, to which alcohol was added, and after thorough digestion had been allowed, the alcoholic solution of urea thus obtained was filtered and then evaporated. To the mass left nitric acid was added, the nitrate of urea was thus formed, and being collected on a filter, was carefully weighed. In eight out of the eighteen urines, however, in which this process was adopted, it was found impossible, from its extreme minuteness, to estimate the amount of urea present; accordingly, in regard to six of the eight, it is noted that there did not exist sufficient urea to crystallise with nitric acid; and in regard to the other two, that there remained of the nitrate of urea a mere trace upon the filter. These eighteen specimens were the only ones in which I attempted to estimate the exact amount of urea contained; but in a large number of the urines examined, the usual method for arriving at an approximation as to the contained urea was practised, namely, by gentle evaporation and the addition of nitric acid. In all the urines treated in this manner the urea[1] was found to be deficient in quantity. In the ten specimens the weight of the nitrate of urea obtained was as follows:—In one, 28 grains; in one, 20 grains; in two, 19 grains; in one, 18 grains; in one, 11 grains; in one, 10 grains; in one, 9 grains;[2] in one, 8 grains; in one, $1\frac{1}{2}$ grains.

From these results it appears, that from none of the urines, the urea in which was made the subject of quantitative analysis, was there obtained of the nitrate an amount equal to that of the urea[1] contained in healthy urine.

7th. The microscopic appearances presented by the urine in sixty-four instances were noted. In two of the sixty-four no deposit of any

[1] The reader will remember that the amount of urea in healthy urine is 30 grains in 1000 of urine. To test the accuracy of my own experiments in estimating the exact amount of urea in cholera urines, I, at the same time, conducted the examination of the urine in other diseases as well as in health, and as the results were uniform, I think the table given above is worthy of confidence.

[2] This analysis was conducted by Dr Douglas Maclagan.

kind existed. In regard to the remaining sixty-two, it is noted that, in fifty, epithelium existed in greater or less amount. In fifteen of these it was present in very large, in three in very small, quantity. In twenty-four urines the casts, or epithelial moulds of the small tubes of the kidney, were present; while in almost all the specimens examined, an appearance resembling these tubes, as if lacerated or broken up, existed. Amorphous urate of ammonia was present in fourteen cases, in one or two others the urate of ammonia evidently existed in a crystalline form. Besides it, the crystalline deposits met with were—the uric acid, the ammoniaco-magnesian phosphate, and the oxalate of lime. The former was present in sixteen instances, the triple phosphate in twelve, and the latter in four; in three of these the dumb-bell was the form assumed; in only one the octohedral. In two urines blood corpuscles, and in other two pus corpuscles, existed, all in small quantity, and all in the cases of females, so that in all probability their presence depended on the existence of some vaginal or uterine discharge.

Exudation corpuscles, or the compound granular cells, were present in three urines. In one of these, the patient, at the period of her seizure with cholera, was just convalescing from a severe attack of pneumonia. These granular cells are very commonly met with in the urine, about the critical period in acute inflammations of the lung. With the history of the other two patients in whose urine these cells were found, I am unacquainted. Spermatozoa and the spermatic globules were detected not unfrequently in the urine, when drawn off, by means of the catheter, from the dead body. In one instance in which dissection was performed a very short period after death, the animalcules appeared living; the urine in which they moved was of acid reaction, and in this respect confirmed an observation I have frequently had occasion to make, that the acidity of the urine is favourable, and its alkalinity unfavourable, to their vitality. In this way the preservation of their life in urine containing pus (according to some authors the only urine in which their vitality can be continued for any time), may be accounted for; such urine is almost invariably acid; on the other hand, urine containing much mucus, in which these animalcules are never found living, may reasonably be supposed to offer as secure a retreat to them as the former, but that urine is invariably alkaline.

From these details of the examinations of a large number of specimens of urine in cholera, it appears, that besides being at first entirely absent, or greatly diminished in quantity, when the secretion is again restored, it is very materially changed as to quality. It is of a lower specific gravity than healthy urine, and though its reaction is generally acid, it for the most part contains abnormal ingredients, of which the chief are albumen and bile, with a greater or less amount

[1] According to M. Regnault, 100 parts of the nitrate should contain 48·938 of urea.

of perfect and abortive epithelium, derived from different parts, and assuming also different forms; and besides containing abnormal ingredients, one of its own principal constituents is absent, or exists in very limited amount. It is this absence or deficiency of urea, the consequences of which are known to us, and are represented in cholera, and in other diseases also, by what are now regarded as very evident signs, which is the only truly important morbid condition of the urine; the abnormal ingredients present are of interest as indicating an alteration in the quality of the urine, but only of importance when taken in connection with this. In the present paper, however, the changes alluded to merit a little more of detail. It is, I think, of interest to observe the circumstances with which the presence of albumen in the urine of cholera is attended; generally bile co-existed with it, if not throughout its entire duration, at least at its commencement, or before its disappearance: further, albumen was always associated with, and in general held a ratio commensurate with the amount of epithelium found on microscopic examination. Taken in connection with the former, its presence, as indeed that of the secretion itself, is an indication of a most favourable tendency, nothing less than that of commencing convalescence, the bile almost always manifesting its presence in the urine at the period when it re-appeared in the stools. No urine which contained albumen did not contain epithelium; and, on the other hand, no urine in which epithelium was present in any amount, was destitute of albumen: further, as the albumen disappeared, so also diminished the epithelium. This association of albumen and the cells of epithelium, is exactly what we observe in the urine of certain other diseases; of scarlatina, for example, from which, at a particular period of the disease, neither, I believe, are ever entirely absent; and though variety, to a considerable extent, exists in the amount of both present, they uniformly exist in corresponding ratio. The presence of epithelium in the urine of cholera, is the evidence of the existence of a process of desquamation, to which the mucous membrane lining the urinary passages is subject, equally with that which lines the intestinal canal, from which desquamation, to a considerable amount, occurs. When such changes are taking place in the epithelium lining the small tubes of the kidney, the office of the cells composing which is to eliminate from the blood the matters which, in the normal state of the renal function, form urine, it is not surprising, that albumen and bile, in greater or less amount, should find their way into that secretion, and that the urea it contains should be at first so deficient in quantity. In but few instances did the two former continue to manifest their presence in the urine for more than a few days at most, while, as a general rule, the lapse of the same period was sufficient for the increase in the urea to have taken place, if not to the normal standard, at least to a point not far distant from it.

Frequent observations on cholera urine have shown me, that the

uric acid is not generally deficient in quantity; this was proved by microscopic examination, as well as by the addition of a small quantity of nitric acid, when the usual change in appearance, illustrative of its presence, occurred. Under the microscope the usual form presented by the uric acid was that of small lozenges; generally they were void of colour, but occasionally possessed a deep yellow hue. The crystals of the triple phosphate were always present when the urine was alkaline—they possessed their usual form. Crystals of oxalate of lime were only observed four times; it was not ascertained whether the patients, in whose urine they occurred, presented the symptoms which are known to accompany the presence of this salt in the urine, and this inquiry was not deemed necessary, for we know that the oxalate frequently appears unassociated with any dyspeptic symptoms. The observation of Dr Prout, that, during the prevalence of cholera in 1832, the oxalate of lime was more frequently present than before or after the epidemic, has not been confirmed, at least in the case of hospital patients, during the existence of cholera in Edinburgh. From numerous observations, conducted with the intention of estimating the relative frequency of the various urinary deposits, I have found that crystals of oxalate of lime occur as frequently in the urine of hospital patients in Edinburgh, as either of the other common crystalline deposits. Oxalate of lime is very common in the urine of patients labouring under all the forms of organic disease in the abdomen, with the exception of that of the kidney. The same observations showed that no increase in the frequency of the occurrence of oxalate of lime, took place during the epidemic of cholera.

On a review of these observations, the following conclusions in regard to the urine passed during recovery from cholera, may, I think, be deduced.

1st. That the urine, besides being affected as to quantity, is materially altered in quality.

2nd. That this alteration consists in the presence of a very small quantity, or in the entire absence, of urea, and in the presence of albumen and biliary colouring matter.

3rd. That on examination by the microscope, there will be found uniformly associated and existing in amount commensurate with the albumen, epithelium assuming different forms, and derived from different parts of the urinary system.

4th. That one or other of the following deposits will probably be present—amorphous urate of ammonia, uric acid, ammoniaco-magnesian phosphate, or oxalate of lime—the two latter being less frequently present than either of the former.

5th. That the healthy condition of the urine, in so far as the increase in the amount of urea, and the disappearance of the albumen and bile, are concerned, is generally restored in the course of a day or two, if the case go on favourably.

6th. That as the association of albumen and bile, in particular, as

well as the other general characters of the urine which have been stated to exist, are by no means of common occurrence, it follows, that the careful examination of the urine in cholera is of importance both in diagnosis[1] and practice.[2]

To which I feel disposed to add another conclusion, though it is not brought fully out in the preceding observations.

7th. That the characters which have been stated to be those of cholera urine, will be always best marked in the severest cases.

ARTICLE V.—*Case of Cancer of the Tongue, existing four years—Cervical Glands unaffected—Removal by Excision—Cure.* By WILLIAM KEITH, M.D., Surgeon, Surgeon to the Royal Infirmary, Aberdeen, and Lecturer on Clinical Surgery.

MRS M'HARDY, aged sixty-six, from the Cabrach, of a pale complexion, aged expression of countenance, and slightly cachectic look, admitted July 3d 1849, with a large fungus occupying the posterior two-thirds of the tongue on the right side, ragged and ulcerous, and bleeding on the slightest touch.

The fungus commences behind in the base of the tongue, just where the anterior column of the velum-palati touches it. In breadth it all but reaches the mesial line of the tongue, and terminates anteriorly at a point distant half an inch from the centre of the tip.

Is afflicted with constant pain of a burning character in the tumour, aggravated by frequent and severe pains of a lancinating kind, darting through the ear, and extending over the whole right side of the head.

Is almost starved, owing to the presence of the fungus rendering it difficult to swallow fluids, and impossible to get solids over at all.

States that four years since she became aware one night of a warty excrescence on her tongue, her attention having been drawn to it by feeling a taste in her mouth to which she had not previously been accustomed. A succession of these warty elevations, white to look at, arose from time to time around the first one. Until six months since the progress was comparatively slow; the whole then became massed into one fungoid tumour, which enlarged from week to week sensibly.

More recently it has advanced with still greater rapidity, so that the growth of the past three weeks has equalled that of three preceding months.

She has been afflicted with pain from the very commencement of the disease —of a burning and lancinating character—shooting out at the right ear; and her sufferings have all along been much aggravated by any attempt to take food—which, on many occasions, had also the effect of causing considerable hemorrhage.

In the course of the previous treatment she had been subjected to the influence of mercury to the extent of salivation, with no advantage to the disease whatever.

The only topical remedy employed had been lunar caustic, which had been freely and frequently applied to the fungus; the effect was in every instance to aggravate both the disease and her sufferings.

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[1] I have made frequent examinations of the urine in diarrhœa, and in the so-called choleroid diarrhœa, but have never found albumen and bile associated with epithelium, and a small amount of urea, to be the characters of such urine.

[2] Dr Robertson prescribed colchicum in a case where a small quantity of urine, deficient in urea, was voided, and with the desired effect upon the urine, and with great benefit to the patient.

The most careful examination of the neck can detect but a slight thickening of one absorbent gland below the angle of the jaw, and in the course of the external carotid.

Having but one tooth on the right side of her mouth, and that the last molar in the lower jaw, it was at once extracted, to allow of free access to ligature the vessels when the disease came to be excised.

July 20th.—For seventeen days past she has been richly nourished with soups and wine, and was this day brought to the operation chair.

1st. A strong ligature was passed through the tip of the tongue rather to the left side, looped, and given to an assistant, with orders to draw the tongue well out, and towards the left side, at same time instructed to watch closely, and as he saw occasion, to allow her to swallow, should a gulp of blood require her to make the effort.

2nd. Her head was steadied against the breast of another assistant, who had charge of both carotids, with orders to limit the pressure to the vessel on the right side, unless he should receive instructions in the progress of the operation to impede the circulation in both.

3rd. The jaws were widely separated by a wedge of cork entered between the teeth on the left side.

4th. The diseased mass was then firmly grasped between the claws of a volsellum held in the operator's left hand, and the right half of the tongue swept out by one cut of a strong curved probe-pointed scissors, the diseased mass being thereby wholly and at once removed. The dorsal and sublingual branches of the lingual artery bled with great energy for a few seconds, owing to a movement of the patient having altered the position of the right carotid, but both the bleeding vessels were speedily secured and easily ligatured, when all hemorrhage ceased.

5th. To eradicate any trace of disease that might exist by possibility in the tissues contiguous, the actual cautery was then run over the whole surface of the wound; the cavity was then filled with amadou soaked with tincture of matico, and the patient removed to bed. She bore the operation well, and neither moaned or winced during the whole proceeding. About two hours after the operation hemorrhage took place, which was, however, easily controlled by a finger in the mouth until the operator arrived, who, having emptied the mouth of amadou and a clot of blood, saw, seized, and secured by ligature the dorsal artery previously tied, when the bleeding wholly ceased. The cautery had evidently come in contact with the previous ligature and burnt it.

8 P.M.—The wound has been left open, and under constant and easy inspection; not an ooze of blood has occurred since the dorsal artery was again tied.

July 21st.—Has slept well during the night; no hemorrhage; has swallowed two cupfuls of soup thickened with arrow root without much difficulty.

July 22nd.—Wound clean; feels well; swallows more easily.

July 24th.—Going on favourably. There has not been the slightest oozing of blood since the afternoon of the 20th.

July 25th.—Complains of a stitch in the left side of her chest, which she ascribes to cold caught from the draught of an open window. Apply six leeches to the pained part

July 26th.—Leech bites bled freely; pain much relieved.

July 28th.—Is well in health; pulse sixty; tongue clean; bowels regular; wound much contracted. The tip of the tongue is now drawn round to the cut side, so that the edge of the left side presents itself as a tip, affording the semblance of a short but entire tongue. Neither the burning nor the lancinating pains have ever been felt since the hour of the operation.

August 4th.—Wound granulating kindly; whole surface healthy and much contracted in extent. The powers of deglutition and of speech wonderfully perfect.

August 7th.—A white speck observed to-day of the size of a split pea, like an aphthous spot, on the right anterior column of the velum palati, quite clear of the recent wound. It is brushed over with nitrate of silver.

August 9th.—The film formed by the nitrate is thrown off, and the spot presents the same appearance. A portion of the mucous membrane under and around it pinched up and clipped off, and the parts under and around freely seared with the actual cautery at a white heat.

August 10th.—Wound suppurating kindly where it was seared.

August 11th.—Wound looks well. Complains to-day of a pleuritic stitch in her right side, affecting her breathing. Apply six leeches.

August 12th.—Pain relieved, but not gone. Apply a blister to the seat of pain.

August 14th.—Pulse quiet; pain gone; tongue healthy to look at, and all but healed; though small, it seems sufficient for every useful purpose. She speaks very distinctly.

August 17th.—Dismissed cured. To travel a distance of fifty miles home.

Remarks.—There seems to be a dread at the performance of this operation on the part of some surgeons, for which there are no just grounds; while others profess to decline the operation on account of the uncertainty of the cure being permanent. To the former I would only suggest, that choking by hemorrhage appears to me to be the only risk; the mere loss of blood need never excite apprehension. The head should be firmly lodged in a perch, such as is used on a dentist's chair, an able assistant can then moderate the bleeding by compressing the carotid of the side on which the operation is proceeding (and the disease is almost invariably confined to one side); a few seconds suffice for securing the two, or at most three, vessels that are there to be found; and during this short period a right posture of the head, with the remains of the tongue well drawn forward, alike facilitate the ligation of the arteries, and the escape of the blood over the patient's chin and breast. For taking up the vessels the spring or bull-dog forceps is far preferable to the tenaculum—its grasp is decided—its hold is secure and independent of support, so that the operator can dispense with one assistant by dropping the forceps and then running home the ligature for himself. The ooze afterwards is trifling; the wound left open, and the mouth empty, would afford the greatest security against after bleeding, by allowing it to be at once seen—and seen to. The wound is most favourably circumstanced for healing, and may accordingly be expected to give no further trouble after the first day.

I would never again apply the cautery until after the ligatures had dropped off, when, if anything suspicious appeared in or about the wound, I would assuredly use it most freely as a likely mean of destroying any remains of infected tissue that might have escaped excision, and as well calculated, by occasioning the formation of the dense cicatrix so well known after severe burning, to resist the effusion of a blastema bed for cancer cells to become developed in.

To such surgeons as deny to a patient the chance of cure afforded by excision in any case or stage of cancer, I would earnestly commend to their careful perusal the inestimable work on this subject recently given to the profession and to science by Professor Bennett

of Edinburgh. After reasoning the question of the "Rational Treatment of Cancerous and Cancroid Growths" in the most able manner—see page 236, *et seq.*, of his work above referred to—Dr Bennett, at page 245, lays down the practical rule—a rule in the propriety of which I entirely acquiesce—"as based alike on pathology and experience"—"That so long as a cancer remains fixed in a part capable of being removed, and the strength of the patient is not too much reduced, so long is the surgeon warranted to interfere."

The malignant character of the growth excised from Mrs M'Hardy's tongue may be assumed from the description I annex, penned after a careful microscopic examination made by our eminent histologist, Dr Redfern.

"The cut surface of the tumour has healthy muscular substance every where, except near the middle, at which point it partakes of the structure of the whole mass.

"The mass contains scales of epithelium of all forms and sizes, naked nuclei, numbers of fusiform, caudate, and irregular nucleated cells of all sizes, and a number of beautiful spherical and large cells, with large nuclei, and one or two nucleoli."

257, UNION STREET, ABERDEEN,
1st September 1849.

APPENDIX.

It adds a melancholy interest to the case above recited, to know that the imprudent manner in which Mrs M'Hardy was conveyed home, has been the occasion of her death. In a letter from her husband, received two days since, he states, "that she reached Rhynie safe" (forty miles per coach), "and being then transferred to an open cart, set out to travel over a hill road, a distance of ten miles further; that she continued well until she was within three miles of home, when she was seized with a severe pain in her side, which came across her heart and stopped her breathing; that after seven days of extreme suffering, she expired on Sunday the 26th August." The only treatment alluded to is, "that two blisters had been applied to her sides, but without any benefit." He adds, "she felt no pain in her head or in her tongue, which was quite whole and beautiful in the appearance."

It is grievous to think that the life of an aged individual that had passed so safely through so much, should have been sacrificed to an ill-judged parsimony. A carriage to convey her over the last ten miles of her journey in place of an open cart—or the skill of a medical adviser on the onset of inflammation in her chest, in place of their own misguided attempts at treatment, might, humanly speaking, have easily preserved the life that bade so fair to be comfortably prolonged by the successful issue of the operation.

257, UNION STREET, ABERDEEN,
6th September 1849.

ARTICLE VI.—*Case of Femoral Hernia, operated on with success after thirteen days' Strangulation.* By DAVID JOHNSTON, Surgeon to the Royal Infirmary, Montrose.

C. R., aged fifty-seven, had been the subject of a femoral hernia on the left side for a good many years. She was unconscious of its nature, did not wear a truss, and never required medical interference on its account. She had previously suffered from occasional attacks of sudden pain in the tumour, accompanied by vomiting, but these symptoms had always subsided after a few hours rest in bed. I was called to see her on Wednesday, July 23d, 1845, and found her labouring under all the usual symptoms of strangulated hernia. The tumour was about the size of a hen's egg, tense, and very painful; there was a distressing sense of constriction in the epigastric region, and vomiting continued incessantly. She stated that she had been in this condition since the accession of the attack three days before, after taking a short walk.

The ordinary means of reduction were put in force; but, after many persevering efforts, they proved unavailing. I then resolved to have recourse to operation; but the patient, although informed of its benefits, and convinced of her present danger, resolutely refused to submit to it. During the ten succeeding days, while her sufferings continued unabated, I was unable to gain her consent. The pulse rose to 150; stercoraceous vomiting ensued; the hernial tumour increased slightly in bulk, became less painful, and lost to a considerable extent its feeling of elasticity. The secretion of urine had all along been scanty, and the abdomen much distended with flatus. The treatment, meanwhile, consisted of repeated but cautious employment of the taxis during the first few days; cold was applied to the tumour; small doses of solid opium were exhibited frequently; and, latterly, an enema daily. A very small quantity of feculent matter was discharged with the first enema, but none subsequently.

On the evening of Saturday, August 2nd (being the thirteenth day after decided symptoms of strangulation had appeared), the patient, persuaded at last that nature could do no more, urgently requested me to operate. Despairing now of accomplishing the ordinary result of an operation, but hopeful that a chance of life yet remained by establishing an artificial anus, I laid open the sac through its whole length. The protrusion proved to be a large mass of omentum completely enveloping a loop of small intestine, the former gangrenous and fœtid, the latter of an uniform very dark appearance, but free either from any deposit of lymph or from adhesions. The stricture was divided; the whole mass of mortified omentum was ablated without the slightest hemorrhage accruing; and the loop of intestine returned into the abdomen. The wound was closed by sutures, and dressed with a compress and bandage. In a few hours vomiting ceased; in twenty-four hours the bowels were freely opened by a saline draught; and on the 21st September the wound had completely cicatrised. At the present date (August 1849), C. R. is engaged in her usual avocations, and in possession of good health. The protrusion occasionally recurs, but has never been productive of any inconvenience.

In this instance the function of the bowel was completely arrested, but its circulation must, to some extent, have still been carried on. It was obviously saved from destructive constriction at the expense of the soft omentum among which it was imbedded.

The case is possessed of interest, from the fact of an operation proving successful for the relief of hernia after the contents of the sac had been in a state of manifest strangulation for thirteen days. The instances, if any, are rare, of a like happy result following an

operation in a like unhappy combination of circumstances; and the issue would seem to warrant operative interference at however late a stage the surgeon may be permitted to have recourse to it.

MONTROSE.

Part Second.

REVIEWS.

Fruits and Farinacea the proper Food of Man, being an attempt to prove, from History, Anatomy, Physiology, and Chemistry, that the Original, Natural, and best Diet of Man, is derived from the Vegetable Kingdom. By JOHN SMITH. 2d Edit. London. 1849.

Report of the Second Annual Meeting of the Vegetarian Festival. Abridged from the Manchester Examiner and Times. July 12, 1849.

THE object of Mr Smith in writing this book, is to prove by argument, as it is the aim of the Vegetarian Society to inculcate by example and speech-making, that ever since the period of the deluge, the whole human race have been living upon a plan contrary to nature; that their almost universal habit of making more or less of their food to consist of the flesh of animals, has been, and is, the cause of countless evils; and that the way to set matters right in this respect, is for all human beings to abjure animal flesh, and to nourish themselves exclusively with fruits and farinacea.

Although we do not find it distinctly stated by Mr Smith, it seems to be tacitly admitted to be consistent with vegetarian laws, and, as we gather from the bill of fare of their dinner, with the practice of the Vegetarian Society, to add to the fruits and farinacea what is commonly called dairy produce. At least, we presume that "savoury pies," "bread and parsley fritters," "blanc mange," "cheesecakes," and "custards," contain more or less of eggs, milk, and butter; and we cannot help entertaining a suspicion, that without these very wholesome additions to their dietetical resources, our vegetarian friends would find fruits and farinacea *alone*, by no means so rich and luxurious a diet as they would have us believe. We do not bring forward the case of these articles, as an accusation of inconsistency against them, because, so far as we can see, their system is directed only against the use of flesh, *i. e.*, whatever is obtained by the destruction of animal life; but we must maintain, that, if they feel themselves compelled habitually to use these articles, it militates strongly against a physiological doc-

trine, to the support of which Mr Smith devotes nearly fifty pages of his book, "That vegetables contain all the elements and qualities necessary for the complete nutrition of man."

Assuredly one of the essential qualities in any system of nutrition must be, that it is one which can be persevered in without repulsiveness, as well as without injury, throughout a lifetime; and until we learn that vegetarians are able, without undue sacrifice of their comforts and tastes, to persevere in a diet, from which all animal matters whatever are rigidly excluded, we must hold that the above thesis has not been sustained, and that, *nolentes volentes*, men will be compelled to make part of their diet, at least, consist of animal substances. If, then, it be admitted that certain animal matters, such as milk, are not only physiologically compatible with, but are essential to, supplying the requirements of human beings, surely others which resemble them in their nutrient qualities may be allowed on similar grounds. There can be no chemical or physiological objection to the protein compound fibrine as it exists in flesh, which may not be urged against the protein compound casein as it exists in milk, or the protein compound albumen as derived from eggs; and, therefore, physiology and chemistry failing them, the vegetarians must fall back, for their objections against flesh-eating, upon the æsthetical and moral objections. If, therefore, we found ourselves called upon to act as counsel for the defendant, in the case of Smith and others *versus* flesh, we should begin our pleading by a special objection to the list of witnesses for the plaintiffs, and move the court to rule that, until the above strictly vegetarian experiment have been fairly tried, chemistry and physiology were not admissible at all as evidence. We have, however, no intention of adopting this method of procedure. We give Mr Smith the benefit of the whole evidence which he can produce, and we unhesitatingly maintain that he has not succeeded in establishing his proposition, that all participation in animal flesh ought to be abandoned by those, who would wish to live both well and wisely. In fact, this grand fallacy runs through nearly the whole of Mr Smith's book, that he contends against the employment of flesh, to constitute part of the mixed diet in use among all well-regulated communities, upon arguments which are applicable only to the immoderate use of flesh along with vegetable matters, or to its use without vegetables at all. Thus, for instance, when he states upon the authority of comparative anatomy, that—

> "All the human organs connected with alimentation, are evidently very different from those in carnivorous animals; and, although in some respects they differ also from the organs of herbivorous animals, they are evidently much more closely allied to those of the latter class than those of the former,"

he merely proves, what every one will most readily admit, that man, on the one hand, was not intended by his Creator to eat grass like an ox; nor, on the other, to tear and devour raw flesh like a tiger.

Whatever we may think of vegetarianism as a system, or of the arguments in its favour as stated by Mr Smith, we must concede to him considerable merit as an author. We shall not assign to him as characteristics, merely the energy of an enthusiast, and the proverbial zeal of a proselyte, but we will say for him that he is distinguished by an intense honesty of purpose, which creates in us the impression, on reading his book, that he (however erroneously) feels and believes all that he writes and contends for; that he is a man of amiable sentiments and charitable feelings, and one who has a proper, but not overstrained regard, for whatever he conceives to be taught him by the word of God. His book, as regards composition, is readable and entertaining, and displays an amount of industry in study which reflects the highest credit on him. Though not a medical man, he has perused, obviously with care and intelligence, whatever chemistry, physiology, and pathology have made known, as bearing at all on his darling subject; and, although we think his conclusions in most instances very unwarranted, and his reasoning often very loose, these faults seem to arise, not from careless reading, still less from a desire purposely to distort the truth, but rather from a want of that fundamental acquaintance with medical science, which so commonly tends to error on the part of those who write popular books in which medical subjects are discussed.

We regret that our space does not permit us to make a detailed analysis of Mr Smith's book; we must content ourselves with pointing out the general line of discussion which he follows, and selecting a few of his arguments, to justify the general statement which we have made above, that though we have read his book, we have not been converted to vegetarianism by it.

Mr Smith begins his argument with an inquiry into the original food of man, and of course maintains that our first parents in Eden subsisted solely on vegetable matter, inasmuch as the only articles mentioned in the Mosaic account as being given to them for meat, were "every herb bearing seed," and "every tree in which is the fruit of a tree bearing seed." It is true that they already had dominion over the animal as well as vegetable creatures, but there is no mention of the former being to be consumed by them as food, and therefore we have nothing to oppose to Mr Smith's doctrine in regard to this matter. But whilst we admit it as a historical fact, we are by no means prepared to allow, that the manner of life of our first parents living in paradise, "in native worth and honour clad," was recorded in the Scriptures for the purpose of being adopted as a physiological rule, for the guidance of their fallen posterity in after ages.

The chairman of the Vegetarian Festival, however, the record of whose speech stands at the head of this article, has no doubts on this point. He says—

"The free will of man might make it necessary that the flesh of animals

should be allowed, but if they would live as near the order of creation as might be, they must adhere to the teaching which was first established by the Creator himself, since the facts of scientific research, confirmed by experience, showed, that what was good at creation, was most reasonable in 1849."

Now, if this doctrine be tenable as regards habits of feeding, it appears to us to be equally so as regards habits of clothing; and if it be reasonable in 1849 to refrain from animal food, because we have reason, on *negative* evidence, to believe that Adam and Eve did so in paradise, *a fortiori* we ought to follow their example in dress, seeing that we are *positively* informed that they both went naked from the creation to the fall. To follow out, therefore, the vegetarian chairman's rule of "living as near the order of creation as may be," we apprehend that we ought not merely to abjure our chops and beefsteaks, but put our breeches in the fire, annihilate our "Nicoll's registered Paletot," and sally forth, stethoscope in hand, to see our patient's to-morrow, *in puris naturalibus* (that being "as near the order of creation as may be"), instead of being attired in "customary suits of solemn black." We do not stop to inquire, whether, in the present easterly wind (*horrescimus referentes*), this would be a good sanitary measure, or whether, in our present fallen mundane condition, it would be regarded as being exactly decent; but this we maintain, that it is every whit as reasonable to imitate Adam and Eve in the matter of dress as in that of food, if we are to be guided by the rule of living, as regards the corporeal man, "as near the order of creation as may be."

Mr Smith, in pursuing his history of the original food of man, reminds his readers that it was not till immediately after the flood, that animal food was permitted as an article of diet; that then the average period of human life, which previously had been about 900 years, was reduced to 400, and when Jacob lived it had gradually declined to 150 years. He then says—

"This abbreviated period of human existence, may not have been the effect solely of animal diet, but it doubtless had a considerable influence."

We have no objection to an inquiry, within proper limits, into the secondary causes by which God executes his decrees, either as regards the duration of human life, or any other subject of his providence. But in the total absence of any declaration on the subject, to assign this as in any respect a cause for the abbreviation of human existence, is, to our thinking, not merely an unphilosophical but a most unwarrantable bare-faced assumption. Before being doubtless on this point, Mr Smith ought to have been able to bring out, with some degree of probability at least, not merely that the shortening of man's days up to Jacob's time, bore some relation to the extent to which the use of animal food then prevailed, but he ought to have ascertained that when human life was further limited to three score years and ten, the use of animal food had increased in a corresponding ratio. We need hardly say that facts on these points are entirely wanting.

Mr Smith next enters upon the consideration of the natural food of man, and draws comparisons, from comparative and human anatomy, in favour of vegetarianism, which, as we have before observed, are not warranted by his premises, his arguments being good enough against the undue use of flesh, or the eating of it raw, but certainly not against its moderate use, when cooked, along with vegetable matters. According to him, likewise, our senses of sight, smell, and taste, all naturally prompt us to eschew animal food, and adopt vegetables. Here is a kind of *resumé* which he gives us on this point—

"It seems reasonable to conclude, that the following relations between fruits and man, are the results of special design. The general height at which fruits grow, is adapted to his stature and upright position; their elevation and shape to his hands as organs of pretension; their consistency to his teeth; the liveliness and beauty of their colours to his organ of vision; their agreeable fragrance to his organ of smell; and their grateful and delicious flavour to his taste."

This is as good an example as we ever met with, of the process commonly called taking a broad margin. The general height and elevation of fruits specially adapted to our stature! They vary from a strawberry, which a tottering infant may pick off the ground, to the banana, which is only to be acquired by an active nigger shinning up a tree forty feet high. Their consistency adapted to our teeth! He who eats a strawberry may do so without teeth at all; he who tries his grinders on a cocoa nut will soon find that he had better attack it first with a saw or a hammer. Their fragrance attractive to our smell! A melon is certainly overwhelmingly fragrant, but the more important fruits of the cereals are not fragrant at all. Mr Smith feels the force of this, and says—

"Many roots, nuts, corn, rice, and esculent vegetables, seem to be equally proper for man as articles of diet; and it may be urged that the organ of smell fails to direct him in the choice of them. But it must not be forgotten that the instincts of man are no longer in their original state; and the sense of smell may now have lost much of its primitive sensibility and discrimination."

Whether our noses are less or more sensitive than those of the patriarchs, we cannot pretend to say; but if the instincts of man are now no longer in their original state, what becomes of all those arguments derived from the original state and early history of our race? They are truly full of interest in a theoretical point of view, and as showing how much man has fallen away from his primitive condition; but, unless Mr Smith means to maintain that vegetarian diet may succeed in restoring man to the model of his antediluvian ancestors, and people the earth with a race of Methuselahs, they cannot serve as practical rules for his guidance in his present state of being.

Into the argument from the moral and sensitive feelings of man, it is not our province to enter. We pass on to the remaining and

largest portion of Mr Smith's book, which is devoted to proving that vegetable food is not only the original and natural, but the best food of man. As this, however, is discussed in not less than seventeen chapters, we must be content with offering a single comment on one or two of his statements.

To his very first chapter, where he maintains that vegetables contain all the elements and qualities necessary for the complete nutrition of man, we have already objected that this has never fairly been tried. Even if the stories of anchorites and holy hermits living on parched peas and water be true—even if it be a *scandalum magnatum* that these worthies now and then their

> "flesh did mortify
> With a dainty slice of a warden pie"—

we take the liberty of doubting the goodness of their *physique* as men, whatever we may think of their *morale* as devotees, and therefore do not admit that their nutrition was complete. Furthermore, even if, on such a diet, they were all as vigorous as Friar Tuck (who, however, understood the philosophy of alimentation better than to adopt it), we should hold such examples worthless; because, to have a perfect diet, we must have one which is at once good and agreeable, and not one which infers the severe ordeal of a lifetime's penance. Mr Smith, we must mention, does not cite these instances of vegetarianism; but the experience of communities and individuals, quoted by him, do not in the least establish his proposition; because we have good reason to believe that, in these instances, milk was used; which must be rigidly excluded, before we can test the capabilities of a purely vegetable diet to subserve the purposes of nutrition. Mr Smith may rest assured, that our railways could never have been completed had the "navies" enjoyed no better diet than potatoes and water. We hold, then, that the experiment of an absolute vegetable diet has never been fairly tried on ordinary men.

When Mr Smith gets upon chemico-physiological ground, he takes some bold strides in directions where we cannot follow him. Thus, in order to make out a good case for the nutritive qualities of vegetables, he adopts the hypothesis that starch may be converted into the protein tissues of our bodies, and he effects it in this way:— 1st, he assumes that nitrogen is absorbed to a greater extent than it is evolved in the process of respiration (a proposition by no means established); 2d, that when absorbed it can form combinations with other elements, chiefly, it would appear, by its combining with nascent hydrogen in the capillaries and intestinal canal! and then he makes protein from starch in the following manner:—

> "If we admit that the nitrogen of the atmosphere combines, in the digestive process, with the elements of food—of which there can be little doubt—

then four equivalents of starch [C^{12}, H^{10}, O^{10}] may be converted into one equivalent of protein, C^{48}, H^{36}, N^{6}, O^{14}, and four of water, with a separation of oxygen."

This is an erroneous kind of reasoning, by no means peculiar to Mr Smith. A great deal of this sort of philosophy runs through the writings of the chemical physiologists and pathologists of the present day. We have no objection to these chemical equations, as affording a probable solution of a fact already established by experiment; but we have no experimental proof whatever to show that fresh blood or muscular tissue can be formed in an animal fed upon starch. When this has been done, we may, perhaps, adopt this hypothesis, as an ingenious way of explaining it; but, in the total absence of experimental fact, we can regard it as nothing more than an exercise in arithmetic.

Mr Smith having, in a chapter on "Vegetable Food consistent with Physical Strength and Activity," treated us with some very astounding narrations of personal vigour, and having, under "Climate and Temperature," made a concession, for which the Esquimaux must be grateful to him, that—

"If farinaceous articles of diet cannot be procured in high latitudes, or if the persons residing there have not been accustomed to that kind of food, then, undoubtedly, animal oils and fat are the only substances that can be substituted for them,"—

and, having shown forth the manifold advantages accruing to mankind from vegetable food; next holds out to us, *in terrorem*, the evil effects of animal diet in causing certain diseases. He not being a medical man, this matter rests solely on quoted authorities, and the way in which he selects examples which he supposes favourable to his views, and disposes of those which do not agree well with his ideas, will be amply illustrated by two examples:—

"Abernethy says—'Animal substances are changed into a putrid, abominable, and acrid stimulus,' which was verified by Sir Edward Berry, who prevailed upon a man to live on partridges, without vegetables; but after eight days' trial he was obliged to desist, in consequence of strong symptoms then appearing of an incipent putrefaction."

This will be admitted, on all hands, to be a very fair proof, if proof were needed, that man is not a hawk, and should not attempt to feed in the same way as that feathered biped. That it proves anything more, we entirely deny.

"In that usually fatal disease, diabetes mellitus, a diet, consisting almost exclusively of animal food, is considered by most practitioners absolutely necessary; and all vegetable productions containing starch or sugar are most scrupulously forbidden. But if fruit and farinacea are the natural and best food of man, they must be equally proper, when judiciously selected, in health and in diseases of every description. Medicines which are unnatural to man in a state of health, are, doubtless, of great use in disease; but food is surely intended to supply nutriment to the system, and to support respiration; and these purposes will always be best effected by such a diet as bears the closest

relation to the structure of the digestive organs. The general use of animal food in diabetes must, therefore, be attributed to an imperfect knowledge of the cause and nature of the complaint."

We believe that no one will deny our very imperfect knowledge of the cause and nature of diabetes, and that we have need of all the enlightenments which sound theory and experience can throw upon the subject. Science and theory have not done very much for us, and experience not much more; but certainly the latter has shown us that, if anything do good, it is an azotised diet, under which, in a considerable proportion of cases, we, like Dr Watson, dare not say the disease is cured, but the exhaustion of the vital powers is delayed or arrested. It is a little too much that Mr Smith, after quoting a detached sentence or two from Prout, and Bernard's experiments on the existence of minute quantities of sugar in the liver, should undertake to teach us how to treat diabetes, in the following sentences:—

"As in dyspepsia, a well regulated diet of animal food, and a sparing supply of vegetable matter, will be much more beneficial to the diabetic patient than the indiscriminate use of all kinds of food of which he may be tempted to partake; but, on a mild farinaceous diet, exclusive of all animal food, there would be a much better prospect of recovery, although the saccharine state of the urine might for a while be increased by it. The presence of sugar in the urine, is only a *symptom*, not a *cause*, of the disease; and as an azotised diet may diminish the amount of sugar without curing the complaint, so a purely farinaceous diet may, by its mild and nutritious qualities, restore both organ and function to health, though the symptoms should at first appear to be aggravated."

In a subsequent chapter, on "Vegetable Diet favourable to Longevity," we have a statement made of so curious a nature that we cannot resist extracting the passage. The Mr Graham here named, is an American, author of a series of lectures on vegetable diet, from which Mr Smith quotes largely:—

"It is said by Mr Graham to be 'well known, also, that human blood, formed from animal food, will putrify, when taken from the living vessels, in a much shorter time, than that formed from pure vegetable aliment; and that there is always—other things being equal—a much greater febrile and putrescent tendency in the living bodies of those who subsist mostly on animal food than in those who subsist mostly on pure vegetable aliment. Hence, if two healthy robust men of the same age—the one subsisting principally on flesh meat, and the other exclusively on a diet of vegetable food and water—be suddenly shot down and killed, in warm weather, and both bodies be laid out in the ordinary manner, and left to the action of the elements and affinities of the inorganic kingdom, the body of the vegetable eater will remain two or three times as long as the body of the flesh eater will, without becoming intolerably offensive from the process of putrefaction."

We are not informed where this curious observation was made; for made it surely must have been, from the precise manner in which it is stated. We can only hazard a conjecture, that Mr Sylvester Graham may, at one time or other, have been resident at the court of the King of the Cannibal Islands, and have had the *entrée* to the

royal game larder, where he could observe the progress of the various samples of human *gibier*, as they gradually attained the *haut gout* requisite for his Anthropophagous Majesty's fully enjoying a good dinner. It is added, that "Majendie fully confirms this statement;" but, as the particular passage is not quoted, we cannot append to the above experiment the *ipsissima verba* of the French physiologist's corroboration.

We would gladly set before our readers some more specimens of Mr Smith's statements in favour of vegetable diet; but we have, we believe, quoted enough to justify our avowal, that he has not succeeded in seducing us into vegetarianism.

The reasons assigned for a system of action, however, may be but indifferent, and yet the system may be good in itself, if it only be unquestionably established to produce good effects in the end. The proof of the system lies in its results; we, therefore, quote, in conclusion, one passage more from Mr Smith, both because it shows what vegetable diet *can* do, and because it brings us back once more to the Vegetarian Society, of which we had almost lost sight, in our devotion to Mr Smith. In reporting the first meeting of the Vegetarian Society, he gives us, among other instances of the good achieved by it, the following:—

> "The wife of another gentleman at the meeting had abstained from flesh, and all intoxicating drinks, for thirty years; had given birth to fifteen children during that time; fourteen out of the fifteen she had suckled; and yet remained hale, young, and happy-looking."

Now, to us, this result of vegetarianism wears rather a formidable aspect. We speak as *pater familias*, blessed with a fair allowance of children, who, on a moderate mixed diet of vegetable and animal matters (certainly never eight days' partridges), have enjoyed an amount of vigour, and exemption from disease, for which we ought to be, and hope we are, grateful. We have, therefore, no direct inducement, from our own experience, to fly from the use of animal food; and if the above be due to vegetarianism, we have no temptation to rush into it, for we tremble to think that the result of its adoption by the wife of our bosom might be, in the course of a few years, an increment of our "little responsibilities" to the tune of a round dozen or so. We have often heard of fish and mollusca as being rather anti-Malthusian in their effects, but they must yield the palm to vegetables.

Mr Smith's arguments, therefore, have not converted us to vegetarianism. We willingly concede to him, that a great many persons in this country, and a still greater number of Europeans in tropical climates, eat a great deal too much flesh; and had he contented himself with showing the abuses of animal food, instead of arguing for its total exclusion from our tables, he might, with a little less zeal and a little more judgment, have done some real good by his book. We fear that its extreme doctrines will effectually prevent it from bringing about any such result.

Dissertation on Scientific Nomenclature, Medical and General; exhibiting the Defects, Anomalies, Errors, and Discrepancies of its present condition, with Suggestions for its Improvement. By R. G. Mayne, M.D., Surgeon to the Leeds Lock Hospital. 8vo. Pp. 82. London, 1849.

We confess we took up this book without much hope of finding in it matter for approbation. We have been agreeably disappointed. We have read it with attention, and now unhesitatingly recommend it to our readers for a careful perusal. It was our strong conviction of the chaotic state of medical nomenclature, which led us to despair of Dr Mayne's efforts for its improvement becoming anything else but a waste of his time, learning, and talents. The end he has in view is most desirable, and though we think the utmost exertions of one individual can go but a short way towards its accomplishment, yet we feel satisfied that our author has begun his undertaking in the way best calculated to attract other labourers into the same field. We therefore fondly anticipate that the correction of medical nomenclature may, ere long, become a prominent topic of interest, and a subject of cultivation, with the medical profession at large.

The book before us is an octavo of about eighty pages, exemplifying some of the principles on which Dr Mayne proposes to publish an Expository Lexicon of the terms in medical and general science. It consists of two parts: the first relates to terms of science having certain terminations,—as *ides*, *ideus*, *adæ*, *idæ*, *ida*, *odes*, &c.; the other treats of some examples of terms belonging to medical nomenclature, among which are anasarca, gastrocnemius, psalloides, emprosthotonos, opisthotonos, pleurosthotonos, platysma-myoides, ephemera, narcotics, antispasmodics, tonics, fibrine, stearine, salicine, &c. This little work, then, is a mere specimen of what the author intends to do in his larger work—and this specimen we uphold, as giving ample promise of Dr Mayne's fitness to make a valuable addition to the literature of medical nomenclature. We urge him to proceed with his task.

We should willingly have entered to some extent into many of the topics suggested in the pages of our author, but time presses, and doubts arise how far a lengthened discussion of such subjects would be suitable for this publication. Our remarks will hardly extend beyond his first section, namely, on terms ending in ides—but even these, we trust, will serve to give our readers some idea of the magnitude and importance of the subject, and of the manner in which he proposes to perform the task which he has taken upon himself.

Referring, then, to Latin terms in *ides*, as clinoides, deltoides, and hyaloides, our author shows satisfactorily that these are in general inaccurately pronounced, as if the *oi* were a diphthong, and

that the only proper pronunciation is as if the word were written clino-īdes or clinoīdes, &c. All such words in their Greek form are unquestionably written as *σφαιροειδής*, like a sphere, are declined as adjectives, and signify, not having the exact figure of the object from the name of which they are derived, but making an approach to its figure. We entirely agree with our author that adjectives of this form are not synonymous, as some think, with those derived from the same root, ending in *us* or *eus*, as sphæricus, or deltoideus. The latter can have no other signification than "of or belonging to a deltoid body," and in anatomy is only applied correctly to things standing in such relation to the deltoid muscle as its vessels, nerves, &c. Such adjectives, however, as sphæricus, conicus, &c., have a less definite signification—since these not only denote "of or belonging to a sphere, a cone," &c., but also "having the figure of a sphere, a cone," &c. These differ from the corresponding words in *oides*, inasmuch as they express, not an approach to the figure referred to, but its actual type. Our author inclines to think the term ossa sesamoidea, an example of the substitution of an adjective in *eus* for the neuter pleural of sesamoides. There we must dissent from him; and it is enough in defence of our opinion to say, that his assumption is unnecessary. We know how many dictionaries he can quote in support of such a statement; but on a point like this, the real question is not what grammars and dictionaries teach, but what is the actual usage of the original authorities in medical science who employed such terms. If Galen, as our author says, makes use of the term *ὀστέα σεσαμοειδεα*, there is an end of all difficulty—the term "ossa sesamoidea," of modern times, beyond doubt, represents Galen's expression, whatever view dictionary-makers may have since taken of it, and his adjective can be nothing else but the neuter plural of *σεσαμοειδής*. The early Latin authors, in adopting technical terms from the Greek, used the actual Greek words, written in the Greek character. By degrees these terms were changed into a Latin form, and were written in the Roman character, the inflection of the words, however, being kept as near as possible to the Greek model. Thus, Celsus, speaking of the eye, says, "quod a vitri similitudine *ὑαλοειδες* Græci vocant"—where the Greek term is plainly declined so as to agree with the accusative case neuter of the relative *quod*. And in the sentence immediately preceding, the same author, referring apparently to what anatomists now term the hyaloid membrane, says, "quam Herophilus *ἀράχνοειδῆ* nominavit;" where the Greek word is also inflected in the manner proper to the language. At a later period, for example in the fourth century, Servius writes, "cupressus et ipsa conoides est; nam a rotunditate in acumen levatur," on which a commentator remarks, "sed melius Græce scribitur." We attach no value to the rule quoted by our author from the Eton Grammar, that Latin adjectives in *ēs*, or *ĕs* have seldom or never the neuter in the singular number, and very rarely in the plural. However true this may be

of the words employed by classic authors in general, it is wholly inapplicable to terms of science, and we would recommend our author, in his future researches, to trust as little as possible to grammars and dictionaries, the authors of which are seldom sufficiently conversant with the ancient works on technical subjects. Let him have recourse to the best classical authorities as exhibiting the actual use of scientific terms in their original purity. It must be confessed, however, that to throw the expression, "ossa sesamoidea" back on a classical form, is but a small point gained when so many similar words in anatomy are left in an almost hopeless state of inaccuracy.

The radical error undoubtedly lay, as our author has pointed out, in regarding the word in *ides* as a substantive, from which an adjective in *eus* might be properly formed without any change of meaning, when joined with a new substantive. For those who wrote in Latin without understanding the Greek inflections, this was a natural, if not an unavoidable, error. And hence, doubtless, has arisen the present form of so many of our technical terms, particularly in anatomy. The difficulty of restoring anatomical nomenclature to accuracy, even under words ending in *ides* alone, looks very formidable when we run over such terms as—tunica choroidea, membrana hyaloidea, tunica arachnoidea, processus odontoideus, cartilago thyroidea, cartilago cricoidea, flexura sigmoidea, processus mastoideus, processus pterygoideus, processus coracoideus, processus styloideus, cavum cotyloideum, cavum glenoideum, valvulæ sigmoideæ, cartilagines arytenoideæ. When, however, the difficulty, as it exists in reference to words of this form, is fairly examined, it is found to be hardly so great as it seems in the distance. In the first place, as Latin is no longer in much use for description in anatomy, no more is required than the uninflected form of the correct expression, such as tunica choroides, membrana arachnoides, processus odontoides, membrana hyaloides, &c.; secondly, the adjective in *eus* is quite proper where the constituent elements or appendages of the parts referred to are concerned—for example, arteria sigmoida, arteria mastoidea, musculus pterygoideus, ligamentum styloideum; thirdly, there are a few models of this form of expression which have never ceased to be in universal use, witness os hyoides, os ethmoides, os sphenoides; lastly, the plural number, which in this form of word alone creates an insurmountable difficulty, is required in a few cases only. When the word is neuter, the neuter plural of the correct form of the adjective coincides with that of the incorrect form, as we have seen in the case of σεσαμοειδεα. Of the rest, cartilagines arytenoideæ, and valvulæ sigmoideæ must be left in an inaccurate form, unless anatomists will consent to say cartilagines arytenoidees, and valvulæ sigmoidees.

As the adjectives in *eus* are undeniably of a Latin form, the com-

mon pronunciation at present of such terms as deltoideus, mastoideus, cannot be defended on any ground whatever. And thus it may be found as easy to change flexura sigmoidēa into flexura sigmoidēs, as to train the organs to pronounce flexura sigmoidĕa.

Fortunately the English form of the terms under discussion, now so generally adopted, creates no difficulty—choroid, hyaloid, arytenoid, sigmoid, serve for all cases, genders, and numbers. And while we agree entirely with our author, that in the Latin form all such words are not correctly pronounced, otherwise than by a distinct enunciation of the two vowels *oï*, hyaloïdes, &c., we cannot suppose that he wishes us to imitate the same rule in English. We allow that the conversion of the *oï* of these words into a diphthong in English is a corruption; but it is already an unalterable rule of the English language—in it *oid* is a monosyllable, signifying likeness, as in the words rhomboid, conchoid, cycloid, epicycloid, coraloid, paraboloid, prismoid, ginglimoid, conoid, spheroid. Were we called on, in modern languages, to renounce whatever can be proved to be a corruption, there would be an end even to that short-lived stability which words are allowed to attain.

We agree with our author entirely in what he says as to the proper etymology and signification of such words as phlegmonodes, erysipelatodes, impetiginodes, icterodes, though we cannot allow that the members of the medical profession in general confound these with terms in *oides*, like the lexicographers whom he quotes.

Before we part with our author, we wish to show our good will by tendering to him one or two pieces of advice; and, had circumstances permitted us to go over the whole book with the same minuteness with which we have treated of the earlier part of it, perhaps we might have had more advice to offer. In the first place, we advise him to leave alone the names of the classes and orders of the Linnæan system. As a reformer of purely medical nomenclature, "satagit rerum suarum." Besides, the Linnæan system is rather a matter of history than of present use in botany. We recommend to him, also, to leave the spiritus asper (familiarly known as the letter *h*), in connection with the letter *r*, in the place where lexicographers have placed it. We assure him that any labour he may choose to bestow, will be thrown away in the attempt to persuade the whole of Europe to change the present mode of spelling such words as rheumatic into hreumatic, rhetoric into hretoric, rhapsody into hrapsody, catarrh into catarhr, hemorrhage into hemorhrage. If he adopts such a mode of spelling in his lexicon, he will surely gain for himself the character of a wild speculatist. We beg of him to eschew it.

We will make but one remark on the first section of the second part of the book, entitled "Terms Peculiar to the Nomenclature of Medical Science." The first term spoken of is anasarca. We en-

tertain no doubt that anasarca or hyposarca was originally two-words, *ανα σαρκα, υπο σαρκα*, and the proof is, that the corresponding Latin word, manifestly a translation, is "inter cutem," which afterwards became a declinable word from an assumed nominative "intercus."

Pathological and Practical Observations on Strictures, and some other Diseases of the Urinary Organs. By FRANCIS RYND, Esq., A.M., M.R.I.A., Fellow and Member of Council of the Royal College of Surgeons in Ireland, &c. &c. London, 1849. Octavo, p. 195.

THIS work bears the impress of being the result of very considerable experience in the important class of diseases of which it treats, and though there does not appear to us to be much that is new in the present treatise, yet the author's descriptions of disease, illustrated as they generally are by appropriate cases, are so clear and truthful, that we think the treatise is one which will prove acceptable both to the student and the practitioner. The former will find in it ample and perspicuous details of the pathology and treatment of some of the most important diseases of the urinary organs, and a safe and judicious guide to practice; whilst the practical surgeon will recognise the faithfulness of the descriptions of disease contained in it, and may draw some useful hints from it as to the treatment of special cases. The chapters on Rupture of the Bladder, Retention, Extravasation of Urine, Urinary Abscess, and Fistula in Perineo, are well worthy of perusal. We can scarcely suppose, however, that the author means to claim, in reference to the treatment of fistula in perineo, the degree of originality which his words would imply; for, at page 144, after stating his opinion that all operations for fistula are futile, unless attention has been previously paid to the state of the urethra, he goes on to say:—

> "The difference, then, between the principle I advocate and that I have more generally seen followed is, that I operate on the urethra in the first instance, in order that the impression made on it may guide and direct my proceedings with respect to the fistula; whereas others divide the latter in the first instance, and take the chance afterwards for dealing with the urethra concurrently with the healing of the wound."

We think that the principle insisted on by Mr Rynd—that of primarily and principally directing attention to the state of the urethra, before having recourse to any other operative procedure for the cure of fistula in perineo—is one which has been long and generally recognised by the profession, being advocated by most modern writers on surgery, both British and foreign. Though, of course, our author must be better acquainted than we are with the

practice of Dublin surgeons, we may observe that the same principle was distinctly pointed out by the late Mr Colles of that city. In the lectures of that eminent surgeon, lately published, after stating his opinion, that the passage of the urine over the fistulous track is not the principal obstacle to the healing, he says (Lecture XXIX., p. 53, vol. ii.) :—"The fact is, it is the morbid condition of the urethra that keeps it open." So that, whilst the principle is one that cannot be too often or too strongly inculcated, it is at the same time one which we had thought was universally followed in practice.

As a whole, we have no hesitation in heartily recommending Mr Rynd's treatise to our readers, as containing much valuable and instructive matter.

Part Third.

MEDICAL NEWS.

DISCOVERY OF A PECULIAR CORPUSCLE IN THE VOMIT, DEJECTIONS, AND SWEAT OF CHOLERA PATIENTS; AND IN THE ATMOSPHERE OF INFECTED LOCALITIES.

In the beginning of July last, Mr Brittan of Bristol, and Mr Swayne of Clifton, were deputed by a committee of the Bristol Medico-Chirurgical Society to examine microscopically the evacuations from two cholera patients, and to make reports and drawings of the appearances met with. The drawings were presented at next meeting of committee, and attracted attention from their representing bodies which each observer considered characteristic of the evacuations in cholera. In the "Medical Gazette" for September 1849, Mr Brittan has given the following account of his observations, which he has illustrated by drawings and a series of tables. "I examined, in conjunction with Mr Swayne, two specimens of rice-water dejections, and on comparing our drawings we were struck with the peculiar appearance of certain bodies depicted in each. On further prosecuting this investigation, I found these bodies to be constantly present in the rice-water evacuations of the cholera patients, and offering the same characteristic appearance that distinguished them from anything I had before observed. In order to ascertain if they bore in their size or quantity any relation to the severity of the symptoms, I endeavoured to obtain specimens passed by the same patient at different periods, as well as to complete the observation by an account of his condition at the time. The result was, I became convinced that a certain relation does exist between the size and number of these bodies, and the time elapsed after the seizure, taken in connection with the severity of the symptoms. That is to say, they are small and clearly defined in the matter vomited; they become larger and more compound in the dejections; and as the disease progresses favourably, where I have had the opportunity of examining, they vanish as the symptoms disappear, and the motions regain their natural appearance. I have also found that in very rapidly fatal cases these bodies are sometimes to be met with only in very small quantity, or are altogether absent, though this observation must be qualified by the remark that it is not always possible, or at all events has not been with me, to obtain portions of every motion passed, and that these bodies might have been present in those not examined. On every opportunity that I have had of examining the intestines of those who died from cholera, these bodies have been

found adhering to the mucous membrane in shreds of white matter, and very abundant; and the inference is, that in these very rapid cases they are in the intestines, though not given off in the evacuations. It must also be recollected that but a small quantity is saved for investigation, and but an infinitesimal part of that even ever comes actually on the stage of the microscope. My observations were made on cases taken just as they came under my notice, and without selection; some in the cholera hospital, some through the kindness of Mr Ralph Bernard, in the Bridewell; they extend in dates from July the 9th to July the 30th. Whilst collecting this series, I examined and compared the specimens with others obtained from patients free from cholera. I found that in healthy solid motions these bodies did not exist, nor could I meet with them in the fluid stools of typhus and other diseases, but that they were present in the cases of *severe choleraic diarrhœa* so prevalent in districts where the disease abounds; and I was thus led to the necessary inference, that these bodies were peculiar to the evacuations of cholera patients, and must have some essential relation to the disease.

"Having been thus led to consider these bodies (which, from their characteristic appearance, I have termed annular bodies), in some manner essentially connected with cholera, I wished to ascertain whether it might be as cause and agent, or effect and product; that it could not be the latter seemed evident at once from the fact that they were unlike any of the known healthy or morbid elements of the body, or secretions, and as they were found in the vomited matters apparently in an early stage of development, it seemed probable they were introduced from without, and would be met with in the atmosphere, &c., of places where cholera was rife. Accordingly, with the view to test the truth of this supposition, on July 19th, with the kind assistance of, and an apparatus suggested by, Dr Bernard, I condensed about ʒj. of fluid from the atmosphere of a room in a house from which five patients had been removed the day previous to the cholera hospital, and found in it bodies *of the same appearance* as those in the stools. I soon afterwards repeated the experiment, with the aid of Mr Ralph Bernard, in a cell in the bridewell, which had been unoccupied for some time, but adjoining cells the occupants of some of which had been seized with cholera, one of whom died the day before. Here, also, the same result was obtained. The same experiment was then tried in situations free from cholera, but with a negative result: the fluid here obtained was destitute of these bodies, and contained only small portions of hyaline structureless matter, also observed in the first. I have since repeated these experiments, aided by Dr William Budd, several times with the same positive and negative results, and therefore feel justified in stating that the same will follow similar investigations made elsewhere, if the necessary care be taken, and a glass of sufficiently high power (I used a Ross's 1-12th) be employed. The only question remaining is, as to the identity of the annular bodies thus shown to exist in the atmosphere of cholera districts, and in the vomited matters and evacuations of cholera patients. Most of those to whom I have shown the specimens entertain no doubt on the subject, and all seem to concur in their identity of form. This is all that is possible in respect to matters of such extreme minuteness, and we must, I imagine, be satisfied here, as in similar cases, to form our opinion on circumstantial and corroborating evidence. And when we consider that this form is in itself of too definite a character to be one of mere chance; that the sizes are progressive, accurate measurement showing those in atmosphere to average from the 10,000th to the 3,000th of an inch in diameter; those in vomit, from the 8,000th to the 5,000th; those in dejections, from the 6,000th to 500th; whilst they are met with in the same specimen of dejection in all the intermediate stages of palpably the same object;—the inference is, it appears to me conclusive, that the annular bodies of atmosphere, vomit and dejection, are but the three stages of development of one and the same body, of whatever nature it may be. Those in the stools are found in almost all these stages, some being nearly as small as those in the atmosphere, others intermediate, evincing a commencement of compound character, whilst the largest again fre-

quently seem parent cells containing young cells in their walls and interior, or broken up into a number of young cells more or less separated from each other. I have seen some much larger, occasionally entire, but more frequently broken with a sharp irregular fracture, the morsels presenting, in some measure, the same characteristic annulus as the parent cell did. Their form is too peculiar to need further comment, the light ring round them giving a peculiar cupped appearance, which is unmistakeable, especially to one who has seen the drawings.

"Having thus given a detailed account of the mode in which I was led to the discovery of these bodies in the atmosphere and evacuations, it might be expected that I should enter more fully into a description of their nature; but, as I have stated in the commencement of this report, my object is simply to lay before the profession the facts as I have found them, that they may receive the attention and examination which I believe they deserve, and be tested and proved by a repetition of my own experiments. To this end I shall be happy to give any information to those desirous of prosecuting the research, fully satisfied that the more they are examined the more fully will they be established, and the more important will be the results that may flow from their knowledge, and with the earnest hope that we may through them obtain, if no more, at least one common ascertained fact on which the profession may be agreed, and by which our inquiries into the cause and effect of this and other allied diseases may be directed in the right path.

"On this account also, I have studiously avoided giving any opinion at all on the facts brought forward, lest I might by so doing distract attention from them, and because I would wish them to stand alone as a fixed and demonstrated truth, from which others, as well as well as myself, may draw their inferences."

After the above was placed in the printer's hands, Dr B. was furnished by Mr Quekett with the following, in a letter of date September 20th:—"I have carefully examined the specimens procured by you from the air of cholera districts, choleraic vomit and evacuation, submitted for my opinion on Friday, September 14th; as also a specimen obtained from the atmosphere this day, and have no hesitation in stating that in my judgment they are successive stages of development of the same body, which I believe to be of a fungoid nature."

In the same number of the "Gazette," Dr Cowdell, of Dorset, announces that he and Mr Corme of that place have found, in the clammy sweat accompanying the last stage of collapse in cholera, "minute organised bodies, closely resembling other bodies admitted by naturalists and microscopists to have a protophytic organisation." Dr C. makes the simple announcement, and promises details in a future number.

Mr Busk, surgeon to the Dreadnought hospital ship, made a communication to the Microscopical Society, on October 17th, on this subject:—"With regard to the whole of the bodies seen in the air and water, and to a great proportion of the bodies found under the other circumstances, they were too indefinite and varied in their appearance and characters to be for one moment referred to the same species. The large bodies found in the matters passed from the system were most constant; and after a careful examination of specimens from Drs Brittan and Swayne with their drawings, and with specimens procured by himself, he believed they were of the following nature:—1st. A true fungus, very rarely seen; being a species of Uredo, and identical with that found in bread affected with smut. This fungus is very common in bread; and as it is not easily digested, its presence is readily accounted for.—2nd. Bodies of less size than the last, but under high magnifying power and defective illumination looking very much like them,—and consisting of small particles of the inner membrane of the grain of wheat. These bodies may be easily seen in the coarser kinds of bread; and drawings were exhibited giving their character before being taken into the body,—which corresponded precisely with the specimens obtained from cholera patients. The presence of these bodies was also accounted for.—3rd. Bodies of lighter colour and more delicate. These were found to agree precisely with grains of starch from wheaten flour;—and

drawings of both were exhibited. From these observations, Mr Busk was led to conclude that the fungus theory derived little or no support from the facts brought forward by the microscopists of Bristol :—although he did not deny that subsequent investigation might lead to the discovery of something which had hitherto eluded observation."

Since the announcement of Mr Brittan's discovery, a series of microscopical observations has been undertaken in Edinburgh by Dr J. H. Bennett and Dr Robertson, in order to test the accuracy of his conclusions. As these investigations are still in progress, we cannot yet present our readers with the details ; but we are authorised to state—

1st. That the "annular bodies" described by Messrs Brittan and Swayne have been frequently found in the "rice water" evacuations of cholera patients.

2nd. That in the majority of cases examined, they have not been seen.

3rd. That they have never been found in the moisture condensed from the atmosphere surrounding cholera patients in Edinburgh, nor in the water used in the city.

4th. That they cannot be confounded with starch globules, as they resist the action of iodine.

5th. That cholera evacuations often exhibit other *and less equivocal vegetable* bodies, such as sarcina, forulæ, and branched fungi.

6th. That a magnifying power of 250 diameters is quite sufficient to exhibit the "annular bodies" when present.

Now that this discovery has been made, we shall shortly have reports of observations by microscopists in all quarters, on whom we would strongly impress the necessity of giving accurate details of the physical characters of the "*cholera corpuscle?*" and of the changes it undergoes from the action of various re-agents—iodine among the rest. Mr Brittan's observations are deficient in these respects. The most important fact in these observations is, the presence of this corpuscle in the atmosphere of infected places—that is, in apartments in which cholera patients are confined, while it is absent in others. That this corpuscle, however, acts as a cause, or has any connection with the propagation of the disease, by no means follows ; for, as in certain states of the mucous membranes their surface is known to present the conditions necessary for the growth of various kinds of confervæ, so in cholera may the state of the mucous membrane of the alimentary canal, *induced by* the disease, present the conditions necessary for the development and growth of the corpuscle in question, which would thus be but one of the secondary results of the disease, and not, as Mr B. seems inclined to suppose, one of its causes, if not the specific one.

ABSTRACT OF OBSERVATIONS MADE IN THE CHOLERA HOSPITAL AT LEYDEN, DURING NOVEMBER AND DECEMBER 1848, AND JANUARY 1849. BY DR F. J. J. SCHMIDT.

From the 7th of November 1848, to the 3d of January 1849, 146 patients were received into the hospital—82 males, and 64 females. Forty-nine of the patients were under fifteen years of age. The youngest admitted was two years and a-half old, the oldest seventy-two. With the exception of ten persons, who were suffering merely from ordinary diarrhœa, all were affected with genuine Asiatic cholera. The greatest number were admitted towards the end of November, and the first half of December. All were of the lowest class; workmen, labourers, beggars, &c. ; and all, except three, inhabitants of Leyden. It could not be ascertained that any of them had been attacked during the epidemic of 1832.

Many of those who were not carried off by the cholera, were attacked with consecutive diseases—by far the most frequent of these being typhus.

Few of the patients could give any clear account of premonitory symptoms ; and very many described their health to have been perfectly good just previous to the attack. Excesses of various kinds could not be traced as the exciting cause, nor was it observed that more patients were admitted after Sundays and

festivals. Exposure to unusually wet weather seemed to have more influence. An instance of this was furnished by a beggar, who went out on a very rainy morning in good health, wandered about all day, and was brought dripping wet to the hospital, at nine in the evening, in the worst state of collapse; he died early the following morning.

None of the numerous hospital attendants were attacked with the disease.

There was much variety in respect of the duration of the illness. The shortest periods were eight and nine hours; both fatal cases. The occurrence of hiccup during the attack, was usually a fatal sign; a fact in direct opposition to the statements of several German writers.

In two women, who were suckling at the time of their being attacked, the mammæ became completely collapsed during the acute stage. With the first signs of reaction they began to swell, soon became turgid with milk, and, when reaction had fully set in, were so distended and painful, that it was necessary to draw off the milk several times. In both patients this was done twice before the secretion of urine had been re-established, and in one of them before the fæces had begun to be coloured with bile. The milk was very thin, of a light green tint, and became of a more decidedly green colour in the course of a few days. Chemical analysis demonstrated the presence of uric acid in it before the secretions of urine had set in. The colouring matter of bile could not be detected by the ordinary tests, so that the cause of the greenness was not ascertained. In the last-named patient the milk first secreted was found to be composed as follows:—

1000 parts contained—

Water, - - - - - -	917·007
Solid matter, - - - -	82·993
Casein, - - - - - -	8·714
Butter, - - - - - -	2·919
Sugar of milk, Extractive matter, Salts, - - -	71·223
Urea, - - - - - -	0·127
	82·993

The milk, when fresh, was neither acid nor alkaline; its specific gravity was 1028·3, healthy milk having a specific gravity of 1028 to 1042.

The bodies of all the seventy-three patients who died were examined, at from twelve to thirty-six hours after death; none at a later period. In all cases where death had occurred in the acute stage, it was noticed that a remarkable elevation of temperature took place immediately after death. Bodies which, long before that event, and up to the last breath, felt as cold as corpses, and which the thermometer proved to be several degrees below the temperature of the surrounding air, showed, a few minutes after death, a very sensible increase of warmth, whether they were laid in a cold place and covered merely with a sheet, or remained covered up in blankets. In either case this increased warmth was retained for several hours, and they did not become as cold as ordinary corpses until the expiration of twelve or sixteen hours. It is worthy of notice that the bodies were generally found to a great extent free from the traces of previous acute or chronic diseases. This remark fully applies to the old subjects. In no one instance were decided tubercles found; and only in very few cases trifling earthy deposits in the upper lobes of the lungs, surrounded with condensed tissue. Carcinoma did not once occur in any organ.

Cranium and Vertebral Canal.—In the cases of pure cholera (uncomplicated with typhoid symptoms), the universal congestion of the veins and arteries, described by some authors, was by no means constantly observed. This condition was only noticed in the vessels and sinuses at the base of the skull. In only about one-fourth of the cases were the membranes of the brain much injected, and its substance hardly ever; so that a section of it exhibited no more than the ordinary number of bloody points. In about half the cases the arach-

noid was thickened, whitish, and opaque, and easily separable from the convolutions, the result of serous exudation. The quantity of fluid in the ventricles varied greatly, and seemed to bear no proportion to the duration of the disease. The same remark applies to the consistence of the brain itself.

The spinal cord and its membranes were examined in two bodies only; nothing remarkable was found in either case.

Thorax.—The larynx, trachea, and bronchi, the pharynx and œsophagus, presented nothing worthy of note. The only morbid condition found in the lungs was "œdema," which existed in the majority of cases of pure cholera where much dyspnœa had preceded death. The rarity of tubercular disease has been already noticed.

The heart and great vessels usually contained a large quantity of blood, the condition of the latter varying considerably. In most cases of pure cholera no fibrinous coagula were to be found, but the cavities of the heart were distended with thick blood, in which large black clots had formed, extending into the great vessels, and easily giving way when an attempt was made to draw them out. Occasionally these clots were mixed with small shreds of pure fibrin. The arteries more remote from the heart were entirely empty; the veins contained more or less blood, dark of colour, and of ropy consistence.

Abdominal and Pelvic Cavities.—The absence of all unpleasant smell on opening the abdominal cavity, which many have asserted to be characteristic of cholera subjects, was by no means universal. In several cases the odour was most disagreeable. The abdominal viscera for the most part presented the general condition and aspect which have been often described. In adults the distended pale stomach, the turgid gall-bladder, projecting beyond the margin of the liver, the inflated ascending and descending colon, the contracted transverse portion, and the deeply injected small intestines and omentum, were strikingly evident. The quantity of matter contained in the alimentary canal depended, of course, on the greater or lesser amount of diarrhœa which had taken place; like the dejections during life, it was colourless, and, where the illness had run but a short course, mixed with undigested food. Tympanitis was rare in the small intestines, but more frequent in the ascending colon. The stomach usually contained, in addition to a larger or smaller quantity of the matters above-mentioned, a considerable bulk of air. In by far the greater number of cases, its mucous membrane might be termed normal, of a light reddish colour, often covered with a tenacious mucus, easily wiped off. The larger scattered glands could be noticed as white prominent points in only two or three cases. General or partial injection of the mucous membrane was not observed; but occasionally, small, circumscribed, round extravasations, resembling petechiæ in appearance.

Duodenum.—In this intestine, as in the other small intestines, vascular injection was rarely noticed. Brunner's glands were frequently found enlarged, from the size of a pin's head to that of a lentil. This condition, however, did not exist in patients who had died typhoid.

Ileum and Jejunum.—In this portion of the alimentary canal, the greatest variety of appearances was manifested, from extreme anæmia to a high degree of vascular injection; the glands in some cases indistinguishable, in others offering the most marked specimens of "infarctus," and these varieties having no apparent relation to the intensity or duration of the disease. In children, the mucous membrane was usually pale and bloodless, but frequently of the normal reddish colour. In other cases there was a considerable injection of the vessels along the free portion of the intestine; the highest degree of injection, in which the mucous membrane assumed a deep red, from the multitude of gorged capillaries, was extremely rare. In two or three instances only did circumscribed extravasations—petechiæ—occur. The above remarks, as to the vascular injection of mucous membrane, apply equally to the typhoid cases, but in the latter the contents of the intestines were, of course, different in their character. In a very few instances no trace of glands could be seen throughout the whole course of the intestines. The glandulæ solitariæ and Peyer's glands, were some-

times barely discernible, as in ordinary post-mortem examinations; at one time they would be found a little raised above the level of the mucous membrane, at another the glandulæ solitariæ would be as much developed as those in the duodenum, while the number, extent, and "infarctus" of Peyer's glands presented the appearance described by authors as "plaques intestinales." This enlargement was by no means equal in both sets of glands; the largest "plaques" being sometimes seen where there was not a trace of solitary glands, and *vice versâ*. In patients who died typhoid, the glands were usually found normal in appearance; ulcerations of Peyer's glands being nowhere met with.

Large Intestine.—The partly distended and partly contracted state of the colon has been already noticed. Its contents were in all respects similar to those of the small intestines; the mucous membrane was in most cases normal, and the glandulæ solitariæ rarely enlarged. Except in a few bodies, where there were traces of old dysentery, morbid appearances were found only where bloody stools had preceded death; in such patients the mucous membrane was more or less injected, sometimes so deeply as to be here and there black, the contents of the bowel being also mixed with bloody exudation.

The *Liver* was always unchanged in structure and colour (with the exception of a few specimens of fatty degeneration, and of earthy deposits); the vessels filled with dark blood. The biliary ducts were almost always greatly enlarged, filled with thick green bile, which was infiltrated into the surrounding parenchyma of the liver. The distension of the gall-bladder was considerable, but varied in different subjects. The bile itself offered every variety of appearance.

Neither in the *Spleen* nor the *Pancreas* was any change observable.

The only circumstance worthy of notice in the *Kidneys*, was the large quantity of mucous fluid which could generally be made to exude from the pyramids by pressure. Sometimes this fluid was to be found also in the calices and pelvis of the kidney, and when this had been washed away, a few drops might still be squeezed from the uriniferous tubes. Under the microscope this fluid appeared to consist wholly of pus cells and epithelium, without any trace of crystals. It had no urinous odour.

The *Bladder* always presented the appearance described by those who have written on the subject, being extremely contracted, quite empty, and showing no trace of vascularity, with the exception of a circle of fine vessels around the neck.

Nothing remarkable was detected in the *Genital Organs*, *Nervous* or *Muscular Structure*. The muscles were usually hard or dark-coloured, having much the appearance of smoked meat.—*Kliniek*, vol. 4, p. 525.

APPOINTMENTS.

Edinburgh.—Professor Syme was elected, on the 17th inst., to be President of the Royal College of Surgeons, his predecessor, Dr J. Argyll Robertson, having, we understand, declined the usual re-appointment to the Chair, on account of the state of his health.

The appointment of Mr Syme is highly creditable to the Edinburgh College, as certainly no one else is better fitted to occupy such a position in a College of Surgeons.

St Andrews.—Dr Day, of London, has been appointed to the Chandos Professorship of Medicine, vacant by the lamented death of Dr John Reid.

London.—Dr Risdon Bennett has recently been appointed Physician to St Thomas' Hospital, in place of the late Dr Burton;—and

Dr T. B. Peacock has been appointed Assistant-Physician in the room of Dr Bennett.

Our list of Books Received has been unavoidably postponed.

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Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*Further Observations on Chloroform in the Practice of Midwifery.* Part II. By Edward W. Murphy, A.M., M.D., Professor of Midwifery, University College, London.

The cases that have been detailed, although not very numerous, are, the writer trusts, sufficient to enable him to form a reasonably fair estimate of the value of anæsthetic agents in the practice of midwifery, and to arrive at some judgment on the *vexata questio*, that is at present distracting the profession. Resting his opinions upon direct personal experience of the effects of chloroform, he has sought, as far as possible, to correct them by the experience of others. Wherever that experience has been faithfully detailed, he has availed himself of it, and he would add, has as carefully shunned all statements or assertions not supported by well-authenticated facts.

Whether anæsthesia be employed at all in midwifery or not, may be a question of *a priori* discussion to be decided by *a priori* arguments. The writer will not here discuss the propriety of relieving the pains of labour, believing it to be the physician's duty to relieve pain and suffering whenever he can safely do so. But questions that concern the value of chloroform for that purpose—Whether it be safe or dangerous to use? whether it contribute to the patient's recovery, or is followed by results detrimental to her health? how it operates on the constitution of the patient?—All these questions, which are to be decided by observation, require facts for their solution, and whether positive statements are confidently made, or ominous hints are cautiously whispered that have no support from facts brought forward, the writer feels it his duty to pass them by as unworthy of

attention. There is no medium in the use of chloroform in obstetric practice. It is either a blessing or a curse;—it is either a consolation to soothe the sufferings of the parturient woman in the hour of her severest trial, or it is an insidious and dangerous agent that should be at once prohibited. The question is too important to admit of being mystified by eager praises, or by clamorous anathemas. We wish to know the truth, and truth can only be known by facts honestly stated. The cases that have been brought forward illustrate the general effects of chloroform in parturition, and will afford some reply to the queries that have been proposed.

Does chloroform paralyse the uterus? Does it deprive it of its tonic contractile power? The writer's experience is in the negative. It did not do so in a single one of the cases he has recorded. In one case of natural labour (Case VIII.), the intervals between the pains were increased, but action of the uterus was more efficient than when they were more frequent. In another (Case IV.), the same happened, and labour seemed to be retarded; but the moment that chloroform was withdrawn, the pains returned with more force and effect than before it was used. If such be paralysis, its effect was quite evanescent; the uterus not only regained its power immediately, but the power seemed to be increased. The patient was hysterical—had some apprehension of chloroform from what she had heard; and it is at least doubtful whether the action of the uterus was controlled by mental causes, or by an irregularity in the action of the agent.

Those cases that required operations afforded stronger evidence, because it was necessary to soporize the patient more completely; the dose of chloroform was more powerful and more likely to affect organic muscular fibre, yet even here it did not do so. In the case of perforation (Case XI.), the patient was as completely under the influence of chloroform as in a surgical operation; she was perfectly still, without sensation or motion, yet the uterus made several expulsive efforts during the operation; and, after delivery, it contracted perfectly and expelled the placenta. In the case of artificial os uteri (Case X.), the patient was ætherized to the same degree, and the uterus contracted equally well. In Case XII., of arm-presentation, the tonic power of the uterus evidently was feeble: the patient had no regular labour. She had altogether only two or three strong pains at very long intervals; the action of the uterus then ceased; there was some hemorrhage also before her delivery; a very moderate dose of chloroform produced a powerful effect: it lowered the rate of the heart's action; turning was performed, while she was under its influence; and, after her delivery, the uterus contracted with sufficient power to expel the placenta without the least hemorrhage. If chloroform paralyse the uterus, it ought certainly to have done so here. The next case (XIII.) of elbow-presentation, is a striking contrast to the preceding one. This patient was strong and healthy; the former was weak and debilitated. In the present case,

a good deal of chloroform was inhaled before any effect was produced; but when it did act, its effect was complete. The writer was obliged to perform one of the most difficult operations of turning he ever recollects to have undertaken; the patient did not mutter a groan; and when, after delivery, consciousness returned, she knew nothing and felt nothing of what had just been done to her. Yet although chloroform so completely narcotised the patient, the chief difficulty of the operation was the powerful resistance of the uterus; it was strongly contracted about the body of the child, and did not relax in the slightest degree. This case is certainly an exception to cases generally of turning under chloroform; but as an exception, it is well worthy of attention, because it proves, in a strong and healthy patient, how perfectly independent the uterus may be of anæsthetic influence. In all the remaining forceps cases, the action of the uterus was in no way interfered with. The conclusion, therefore, that is derived from these cases, taken collectively, contradicts the statement that chloroform paralyses the uterus. In the examination of this question, however, the experience of other observers must be taken into consideration; and, with this view, the writer confines himself to those alone who have advanced carefully observed facts. In an inquiry of this nature, loose assertions and imperfectly related cases are worse than useless, because they are only calculated to make facts appear much more contradictory than they really are, and to confuse rather than convince the patient inquirer after truth. Dr Denham[1] gives a very interesting report of fifty-six cases of labour, with whom chloroform had been used, in the Dublin Lying-in Hospital. Forty-four of these were cases of præternatural, instrumental, or complicated labours; twelve were natural labours. In the latter class, chloroform was used for a longer time, and its effect on the action of the uterus was more easily observed. The degree of etherisation is not precisely stated. In the majority of these cases, the intervals between the pains were increased, and the pains are described as "weaker" after chloroform than before; nevertheless the action of the uterus went on, and the labour was soon happily concluded. In some, the pains returned with frequency and force, the patient being still under the influence of chloroform. In others, the same effect took place immediately after chloroform was withdrawn. One patient (Case X.) "was put under the influence of chloroform, but the pains soon became weak, and at the end of an hour *ceased entirely;* the chloroform was now left off, soon after which the pains increased to such a degree, that *we began to dread rupture of the uterus*, especially as one of her former labours had been instrumental. She was therefore again put under the influence of chloroform, when the pains subsided, and she was safely delivered by the forceps; child living, no hemorrhage; recovery good." In another case (Case XI.), "the pains gradually diminished, and

[1] Dublin Journal for August, p. 107.

at the end of two hours *had almost entirely ceased.* The chloroform was now discontinued, when the pains again *returned with considerable force,* and the child was born in half an hour from the time she ceased to be under the influence of it." A third case (Case XII.) occurred in the practice of Dr Shekleton, the present master of the Dublin Lying-in Hospital, and is sufficiently remarkable to quote more at length. "A lady, pregnant of her first child, had been in the hands of an ignorant midwife for twenty-four hours." Dr S. was sent for.—"On going up the avenue leading to the house (he says), I distinctly heard the lady's screams, and fancied she was on the eve of getting well. On examination, however, I found it was a face case, the chin partly protruded, and looking towards the pubis; the features of the child were so swollen and distorted by the rude fingering of the midwife, that, except for the chin, they could not be recognised; the vagina was hot and tender from the same cause; and the perinæum hard and rigid, partly owing to the advanced age of the lady, and to her habit, perhaps, of hunting regularly during the season. The pains were powerful, and evidently advancing the labour, though slowly. Instrumental assistance, from the state of parts, was out of the question, and I determined to try the effect of chloroform to mitigate, if possible, her agonising screams, which accompanied every pain. She was speedily put under its influence, talked at first of hounds and horses, and laughed immoderately; and it decidedly put an end to her expressions of agony, but *the labour pains nearly ceased,* and for two hours and a half, that I kept her in a state of anæsthesia, *the child never advanced.* I therefore finally abandoned it altogether; and with the happiest results, for *the pains speedily resumed their efficacy; the screaming never returned;* and the child was born alive in three hours from the time the chloroform was relinquished."[1] These three last cases are quoted by Dr Denham to show where chloroform, "if persevered in, may be positively injurious" in suspending labour. In the nine cases previous to these, it was highly beneficial; and although, in some cases, the intervals of the pains were lengthened, and the pains *seemed* weaker, labour was rather hastened than otherwise. The writer hesitates to believe that even these three cases prove that chloroform was injurious; the action of the uterus was certainly suspended while the patient was under its full influence, but so far was the uterus from being paralysed, or its power impaired, that it returned with increased force the moment that chloroform was withdrawn, and in the last case evidently with considerable advantage; the "pains speedily resumed their efficacy; the screaming never returned." If these instances, where the intervals between the pains are lengthened—where they seem weaker, or are suspended—be considered proofs that the uterus is paralysed, there is certainly no danger whatever in the paralysis; it is but momentary, and disappears with the cause

[1] Dublin Journal for August, pp. 116–118.

producing it. Such an effect, consequently, cannot be advanced as an objection, in the sense in which it is offered. The objection is meant to imply much more than a mere suspension of labour: it signifies a diminution of that contractile energy of the uterus upon which the safety of the mother depends—that loss of tone which results in hemorrhage, in retentions of the placenta, and other ill effects, such as we observe after a very protracted labour, or in very feeble habits. If chloroform brought the uterus into the state that we find it when it is exhausted, relaxed, difficult to excite its contractions after delivery, requiring the aid of our best directed skill to prevent hemorrhage taking place—if such were its effects, there could not be a more dangerous instrument of mischief, or one that should be more strongly condemned; but no such effect is proved to be the result of its action. The writer has carefully looked for it in every case that came under his notice, and did not find a single instance where the uterus lost its contractile power. In the whole of the fifty-six cases reported by Dr Denham, there is not one that gives evidence of such an effect. He quotes two cases where hemorrhage occurred. In one case (XIII.) of natural labour, in which the pains had nearly ceased after the administration of chloroform, it was discontinued. "Soon after this, they again returned, and the child was born in two hours; the placenta soon came away; but, *at the end of six hours*, there was some hemorrhage, but *not to any extent.*"[1] The other case (XLV.) was one of twins; "*labour tedious, from inertia*, the result of over-distention of the uterus. . . After being twenty hours ill, she was delivered of both children, while in a state of anæsthesia. An interval of an hour was allowed to elapse between the birth of the first and second child, during which she became conscious, but without a return of uterine action sufficient to expel the second child. She had a smart attack of hemorrhage after the placenta came away; her recovery, however, was good, and she went home well on the ninth day."[2] No just reasoner would attribute the hemorrhage that occurred in either of these cases to chloroform; and, consequently, if this be admitted, the facts brought forward by Dr Denham prove, that, although chloroform may "interfere with the activity of uterine contractions," may "interrupt or suspend the expulsive and contractile power of the organ," it does not *impair* its energy, nor produce that loss of tone which is followed by such dangerous consequences. Why chloroform sometimes suspends the action of the uterus, is a different question, and one that is full of interest; but, in our examination of it, we must not confound suspension of power with loss of power.

Dr Channing, who is a warm advocate for etherisation, gives, with his opinions, the whole of his facts. He details 78 cases of labour that had been delivered under the influence, either of ether or chloroform; 43 of these were cases of natural labour; 29 inhaled

[1] Dublin Journal for August, p. 118. [2] Ibid., p. 130.

ether; 14, chloroform. There is only one instance (Case IX.) of suspension of pains where ether was administered, and none where chloroform was given; on the contrary, the action of the uterus, in place of being diminished was generally increased—in some instances, remarkably so. In case XLIII., "the pains were of great severity; the os uteri dilated to the size of a dollar; chloroform was inhaled imperfectly at first; no effect was observed; at length she inhaled freely, and became calm; there was no effect whatever observed from chloroform, but quiet. . . With regard to the progress of the labour, the facts were of great interest: *the pains rapidly increased;* the show, which at first was very small indeed, became abundant, and of the true character; the os uteri rapidly dilated; the waters came away; and the child was born alive twenty-five minutes past three o'clock, about an hour and twenty minutes after the first use of chloroform."[1] In the next case (XLIV.), chloroform was given in the second stage of labour; "a sound sleep, accompanied with loud and deep snoring, occupied the intervals of the pains; and the pains were not at all noticed by the patient. *I note this especially, because of the rapidly increased force of the contractions, while under the fullest effect of chloroform.*" In case XLV., "the womb acted with increasing energy in every succeeding contraction"[2] after chloroform had taken its effect. In case LVIII., "at times unconsciousness and insensibility were complete, and it was during these *that the uterine action was strongest and most efficacious.*"[3] Dr Channing relates five cases where hemorrhage occurred. In the first (Case XVIII.), it took place *before* chloroform was administered; none occurred afterwards. In the second (Case XXXVII.), it happened *an hour after delivery*, not externally, but into the cavity of the uterus. In the third (Case LXVII.), the patient *had hemorrhage in her previous labours* after the birth of her child; the same happened in this, the placenta was adherent and obliged to be removed; but there was no difficulty in securing a perfect contraction of the uterus; the womb was in fact already contracted upon the placenta, closely adapting itself to the inequalities it presented. The fourth case inhaled sulphuric ether; the child *and placenta were safely delivered*, but soon after she looked pale, and felt faint; on examination, the womb was found very large, and filled with firm coagula. Dr C. at once emptied it of the coagula; "*it contracted promptly and firmly*," and no further hemorrhage took place. In the fifth case, the patient, who was in labour of her fourth child, inhaled about two drachms of chloroform in half drachm doses. "During the whole time there was no loss of consciousness, and only just so much insensibility as to render labour comfortable;" the walls of the abdomen were unusually thick, very pendulous, and so loose and uncontractile, that Dr Channing could fold them very easily over

[1] Etherisation in Child-birth (Channing), p. 233.
[2] Op. cit., p. 234. [3] Ibid., p. 256.

each other; the uterus, however, *contracted firmly, and expelled the placenta*, but hemorrhage soon took place; and the loss in a short time was very great. By careful management the patient recovered. Upon this case, Dr Channing further remarks, "A year ago or more this woman had been seized in the unimpregnated state with severe uterine hemorrhage, and grave inflammation of the uterus. Then, again, sometime before labour, she complained of a singular sense of sinking in the præcordia which led to strong demands for air and fanning; there was thus *a hemorrhagic tendency present*, and she had eaten a hearty breakfast of meat and potatoes while labour was present.[1]

These are the whole of Dr Channing's cases of hemorrhage, and in none of them is there any clear evidence that the cause of hemorrhage was paralysis of the uterus, the result of etherisation. Further testimony upon this, as upon other points, is supplied by Dr Channing's correspondents. Dr C. sent round a circular containing eleven queries on several important points in connection with the use of ether or chloroform,—among these the 7th was—Special effects, *physical*, moral, or intellectual? 8th. *Apparent danger*, and how manifested? 9th. *Results to mother* and child, both immediately after delivery and later?" Forty-five medical gentlemen replied, quoting, collectively, 471 cases of labour. Some allude, and in particular Dr C. Ware, to the special effect of etherisation in "diminishing the force of the pains, and retarding the labour;" but not one makes the slightest allusion to any apparent danger, and all report most favourably of the patients' recovery. Taking, then, the whole range of this experience, the writer presumes that upwards of 600 cases[2] of labour, in which either ether or chloroform has been used, is quite a sufficient number to determine one special effect of etherisation. The question before us concerns the physiological action of a new agent. Does it annul pain? Does it paralyse the uterus? The one effect should be just as observable as the other in every case that it is used; and if we are satisfied that hemorrhage is the consequence of atony of the uterus, flooding more or less, should be noticed almost as frequently as absence of pain; and yet in these 633 experiments of slight, moderate, and profound etherisation, there is not one single case in which hemorrhage has been clearly proved to be the result either of chloroform or of ether. It is true that hemorrhage has occurred where chloroform has been administered, just as it has happened where such an agent was never thought of; but the very circum-

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[1] Etherisation in Child-birth (Channing), p. 321.

[2] The following cases are referred to:—

Dr Channing, - - -	78 cases.
His forty-five correspondents, -	471 ...
Dr Denham, - - -	56 ...
The writer, in present paper, -	21 ...
In a former paper, - -	7 ...
	633 cases.

stance that these cases are not in greater proportion than in ordinary cases, whereas it ought to be pointedly the reverse, is the strongest proof that etherisation has no such effect. If, then, this be admitted, a question naturally springs up from it that can only be answered by careful and accurate observation. If the uterus be not paralysed by chloroform, why are the intervals of the pains lengthened, the pains themselves weakened, and the labour sometimes suspended by it? The writer would prefer leaving this question unanswered, because he has no facts from his own experience upon which to found an opinion; but he would suggest as an important and interesting inquiry—the influence of the mind on the action of the uterus. Every practical accoucheur is aware of the power that the parturient woman has by mere mental effort to hasten or to retard her labour. We are daily witnesses of the manner in which she calls to her aid the whole of the respiratory muscles to assist the action of the uterus; we have also evidence of the reflex action of those muscles in aiding the uterus irrespective of the will;[1] there are, therefore, many interesting points to inquire into when we examine the independent action of the uterus. Take away the will, and you remove with it, in many cases, straining efforts, constantly and fruitlessly repeated; this condition is changed for one of perfect repose; the intervals of uterine contractions are lengthened,—they *seem* weaker, or perhaps are suspended until the stimulus of the child excites reflex actions, when they are renewed; this may occur while the patient remains completely soporised, and the patient may be delivered in that state. In another case, the patient may struggle to prevent the pain, and her apprehensions alone will often do so; but let her be unconscious of suffering, and the uterus that before was impeded and embarrassed in its action by the will, acts with increased power when this disturbing cause is removed. Thus may be explained those opposite and apparently contradictory statements—that chloroform weakens and suspends the action of the uterus, and, on the other hand, that it increases its force.

The writer, however, merely throws out these observations to explain the views of the subject that appear to him so interesting, rather than with the intention of giving an absolute opinion upon the question itself. Its importance will be appreciated when it is considered in how many instances, as in Dr Shekleton's case, the removal of the influence of the passions as a disturbing cause, will restore the uterus to its proper order of action, although it may be for a time suspended.

In those cases where chloroform is given in moderate doses, so as merely to diminish pain, but not to destroy consciousness, the action of the uterus is not interrupted, the intervals between the pains are not generally lengthened, and if such should happen, it is because the patient sleeps more profoundly between her pains, and is not kept awake by the aching that often lingers after the last pain. In-

[1] The influence of the reflex nervous function on the uterus, and the effect of chloroform in disturbing it, is at present under examination.

tervals so lengthened are always valuable in hastening the progress of the labour.

With regard, then, to the effect of chloroform on the uterus, the writer concludes :—

1st. That chloroform does not impair the contractile power of the uterus, neither does it interfere with the action of the uterus, unless it be given in very large doses, or that the patient be highly susceptible of its influence.

2nd. That the full anæsthetic effect of chloroform may be produced without paralysing the uterus. The intervals between the pains may be lengthened, or the pains may be suspended, without any loss of power. On the contrary, in such instances, the moment that chloroform is withdrawn, the uterine contractions instantly return with increased force and efficiency. The renewal of the uterine action may occur when the patient is under the influence of chloroform.

3rd. Whether the action of the uterus be temporarily suspended by chloroform—whether it be increased or not interfered with—in every case where the patient had previously suffered agonising pains, and her labour was making an unfavourable progress, chloroform has produced a most salutary change in restoring the proper action of the uterus by which labour was brought to a happy conclusion.

4th. Those effects that indicate want of power in the uterus, viz. great protraction of labour, hemorrhage, retained placenta, &c., have not been proved to be the results of chloroform; on the contrary, where some of these conditions have been observed, there were causes present quite adequate to explain them independently of chloroform.

5th. That degree of etherisation which removes the intensity of pain, without interfering with consciousness, does not interrupt the action of the uterus.

The next question—"Whether the safety of the child is hazarded by the administration of chloroform?" needs only a brief answer in the negative. A sufficient number of cases have been recorded to put this question to the test of proof, because it is very obvious that, if such were its effect, it must be at once noticed by the great increase of infant mortality; and yet, in 540 cases of natural labour, in which either chloroform or ether had been used, not a single child's death is reported; in 179 of these chloroform was inhaled. In 79 cases where different operations were performed, 20 being perforations, the deaths of children were 28, or, deducting perforations, only 8. Eight deaths in 59 cases, or 1 in 7, is rather less than the average infant mortality in operative midwifery, which is about 1 in 4 cases.

The following table is formed from the cases reported by Dr Channing, Dr Denham, and the writer :—

Natural Labours.

Name.	Cases under		Total.	Children		Mothers	
	Ether.	Chloro-form.		Living.	Dead.	Living.	Dead.
Dr Channing, -	35	14	49	49	...	49	...
45 Correspondents,	325	146	471	471	...	471	...
Dr Denham, -	...	12	12	12	...	12	...
The Writer, -	...	8	8	8	...	8	...
Total, -	360	180	540	540	...	540	...

Operations.

Name.	Cases under		Total	Children		Mothers		Cause.
	Ether.	Chloro-form.		Living.	Dead.	Living.	Dead.	
Dr Channing, Forceps c.	7	2	9	9	...	9	...	
" Perforations,	3	...	3	...	3	3	...	
" Turning,	3	4	7	5	2	7	...	
Dr Denham, Forceps,	...	18	18	17	1	18	...	
" Perforations,	...	12	12	...	12	10	2	Perito-nitis.
" Turning,	...	11	11	8	3	10	1	Ileitis.
The Writer, Forceps,	...	10	10	10	...	10	...	
" Perforations,[1]	...	5	5	...	5	5	...	
" Turning,[2]	...	4	4	2	2	4	...	
Total,	13	66	79	51	28	76	3	

The third is the most important question—"Whether any ill effects subsequently manifested themselves by which the safety of the patient was compromised?"

So far as the writer's experience is concerned, he cordially adopts the language of Dr Denham: "I must say that I have never met with a single untoward circumstance affecting the health or life of either mother or child, that would, in the slightest degree, deter me from giving it were it thought desirable or necessary. I have never witnessed any of the dreadful evils described by some writers as consequent upon its use. I have never seen the blood blackened, nor the brain poisoned, nor has it ever in my hands induced convulsions, partial paralysis, or still more formidable consequences."[3] Dr Channing observes: "I have spoken now of the treatment of the untoward effects of etherisation, let the circumstances be what they may. What is said can have only a very indirect relation to child-birth, since I know of no case of labour in which the various affections so distinctly referred to have ever been

[1] Including cases in former paper.
[2] Including cases in former paper.
[3] Dublin Journal, August, p. 120.

noticed; on the contrary, they have been confined almost exclusively to minor operations in surgery, as tooth-drawing, nail-drawing, &c."[1] The writer has not met with a single case where the inhalation of chloroform was followed by coma, convulsions, or other ill effects, described as the necessary consequences of its use; and, notwithstanding what has been so boldly asserted, he has not the smallest hesitation in saying, that *there is not a single case of labour on record where chloroform or etherisation was the cause of death.* All those deaths which have occurred from the use of this agent were the results of its administration, or rather mal-administration, in surgical operations,—cases in which the risk of accident is greatly increased, because a more powerful dose is always required than need ever be given in midwifery practice. It is necessary that the patient be not only insensible to pain and unconscious, but perfectly still; the patient is brought to the very verge of the precipice, and a very little want of caution would very easily throw her over it. In the practice of midwifery they need not be brought near to it. Deaths, however, are said to have "resulted from the employment of anæsthesia during delivery," which cannot be passed over in silence, not only because of the importance of the question, but that to omit them might be interpreted as a tacit admission of their accuracy. Mr Gream relates a very melancholy case—a death from chloroform—in the following words:—"A most melancholy death resulted in the case of a lady of early age, who gave birth to twins, some miles from London. The first was born, and before the other was expelled, chloroform was administered to her, but she survived the birth only for a period less than an hour."[2] There is no account of the manner in which chloroform was administered, nor the quantity given, nor the symptoms that preceded the fatal termination; there was no post-mortem examination, such as took place in other cases of death from chloroform: we have simply to put two facts together—that the lady took chloroform before the birth of her second child, and that she died—*not immediately*—but in less than an hour after its delivery. From these two facts we are required to infer that her death must be from chloroform. Mr Chevalier relates a case that may be compared with this—a delivery that occurred long before chloroform was thought of:—"The last case," of sudden death, says Mr Chevalier, "occurred lately in a lady who died shortly after having been delivered of twins. I was informed that she had for some time entertained apprehensions that she should not survive her delivery. After the birth of the second child, she appeared a good deal exhausted, and as the discharge of blood *was very moderate*, the accoucheur thought it best to defer the extraction of the placenta. She recovered a little, but, about two hours afterwards, grew suddenly faint, breathed short, and died in about half an hour."[3] A post-mortem

[1] Channing on Etherisation, p. 131.
[2] Misapplication of Anæsthesia.—Gream, p. 59.
[3] Med. Chir. Trans., vol. i., p. 160.

examination of this case was carefully made; "all the viscera were free from disease, the uterus contained the placenta, *with a small quantity of blood.* But all the cavities of the heart were in a state of relaxation, and completely destitute of blood." Had this lady taken chloroform, the writer can imagine the triumph with which it would have been brought forward as a strong instance of its destructive properties, not alone paralysing the voluntary muscles, not merely paralysing the uterus and retarding labour, but here actually paralysing the heart itself, and thus destroying life. As it is, the only difference between the two cases consists in the facts—that one lady took chloroform—the other did not.

Another case, quoted by Mr Gream, was that of "a young lady who had become a wife only within the year. She died at about the end of the second week from her delivery, never once having rallied from the effects of chloroform."[1] Here also is a similar obscurity; we are not given a single symptom to guide us into a knowledge of these fatal effects, not operating, as chloroform is known to do, almost instantaneously, but occupying weeks to develop its mischief. Were there no other causes than chloroform in action that might explain the result? Two other cases have come to Mr Gream's knowledge, as having occurred in the suburbs of London—both under the hands of very skilful practitioners:—"In both cases the effect was direct—in one, death resulted," not immediately, but "very shortly after delivery. In the other, life was prolonged for many days; but the remedies used were of no avail, and the patient sunk without ever having rallied from a state of utter prostration."[2] These cases are equally imperfect as the former, no account of them, in fact, has been given, because the gentleman who quotes them "regrets, by any direct allusion to these cases, to increase the feelings of remorse that have since been entertained at having listened to the encouraging accounts given of the effects of chloroform, and acted upon them without considering the dangers and waiting for experience to prove or disprove their liability to the destruction of life."[3] But the writer takes leave to say that this is no excuse. Any conscientious practitioner would feel it his duty, however painful it might be, to give the fullest report of such a case, in order that it might be fully investigated, proved, or disproved. He is bound to prevent other practitioners being led away, like himself, by these "encouraging accounts," by telling them faithfully, candidly, and clearly, the dangers that he incurred by the use of chloroform. If the writer is asked to believe in such cases, so totally opposed to his own experience and to that of others, he demands, at least, a faithful and exact narrative of them; but it can hardly be expected that he should give credence to such loose statements, that look much more like the "lenes susurri" that are meant to whisper down the character of a valuable remedy,

[1] Misapplication of Chloroform.—Gream, p. 59.
[2] Op. cit., p. 61. [3] Op. cit., p. 61.

than those honest objections which would instruct us in forming a true estimate of its worth. In such deaths from chloroform

Credat Judæus Apella. Non Ego.

Other deaths from convulsions, from ruptured uterus, and puerperal fever, are quoted as deaths from chloroform, simply because chloroform happened to be administered. The writer has carefully perused these cases, and can find nothing in them different from similar cases that have been reported, but in which no anæsthetic had been given. He feels, therefore, the more confidence in repeating, that there is not a single well-authenticated death from anæsthesia in the practice of midwifery. In making this assertion, however, he by no means wishes to imply that such an accident is impossible. On the contrary, when the powerful nature of the agent is considered—when we reflect on our imperfect knowledge of its properties, and the want of uniformity and precision in the mode of its administration—we can only attribute its comparative safety in obstetric practice to the very small quantity of the anæsthetic that is necessary to produce the desired effect—a limit which the instinctive caution of the accoucheur will prevent him ever exceeding. All the deaths from chloroform have occurred either during surgical operations, or from the reckless experiments of individuals on themselves, and these have been instantaneous. While, therefore, the writer is far from denying the possibility of death from chloroform in the practice of midwifery, he cannot admit the accuracy of those that have been thus brought forward; and he would add, with great respect to those gentlemen who relate these so-called "deaths from chloroform," that if not accurate, they should never have been advanced as evidence. To do so, is an act of injustice equally to the profession and to the public. To the former, because, deceived by such statements, they yield themselves up too readily to the prejudice that always exists against novelties, and decide without investigation against what they are told is so dangerous; while those who do venture to examine this new agent, who dare to use it, and who are bold enough to declare in its favour, have these false facts thrown in their teeth as proofs of their reckless indifference to human life. To the latter it is equally unjust, because, on the authority of such bold assertions, a means of relief is withheld from them, which they have a full right to expect is only denied them because of the certainty of its danger; but when there is proof amounting to demonstration that this relief may be afforded, not only without danger of a fatal result but with positive advantage to themselves, the opponent who rests his assertions on such factitious evidence incurs a serious responsibility—he not merely deceives himself, but leads the public, who are ignorant on these matters, altogether astray.

In making these observations, the writer is very far from wishing to stifle a fair and impartial inquiry into this question. So far from this, he is convinced that such an inquiry is precisely that which is

so much wanted. Enthusiasm is warring against prejudice, and the anxious observer of the conflict can see little or nothing through the clouds of sophisms and false facts that surround it. Sooner or later these will descend to their proper level, and the truth will be revealed; but it is obviously desirable to hasten that period, and to discourage assertions on either side that bear not upon them the stamp of perfect impartiality. The enthusiast who witnesses new and important phenomena, leading to most valuable results, may perhaps be pardoned if he is a little dazzled by the bright discovery, and fails to observe the spots on its disc. But he who will not use chloroform—the opponent who will not examine for himself—who will not look, or perhaps who, blinded by his prejudice, cannot see, should not be believed when he exclaims that all is darkness.

Having made allusion to those cases that are not "deaths from chloroform," the writer would briefly refer to those that are. Certain deaths have taken place, about which there cannot be a doubt that chloroform was the cause, and it is desirable to examine in what manner it becomes fatal. In other words, we should know and study the precise action of chloroform on the constitution, in order to determine the limit between danger and safety. The researches and experiments of Dr Snow, Mr Nunneley, Mr Wakley, and M. Gosselin, afford abundant information and assistance in such an inquiry. All the deaths that have been caused by chloroform—the deaths at Newcastle, Cincinnati, Hyderabad, Boulogne, Lyons, Westminster, Guy's Hospital—all agree in one fact, the deaths were sudden, and happened when the patient was completely narcotised by the vapour. When the degree of narcotism is not so great as to cause death, and the vapour is withdrawn, the patient soon recovers completely from its effects, and is quite as well as before. This is also proved by numerous experiments on the lower animals, some being destroyed in less than two minutes, others being brought nearly to the fatal point of narcotism, but recovering completely the moment the vapour was removed. With one or two exceptions, that may be explained by the manner in which chloroform was inhaled by the animal, there are no examples of protracted recoveries or of lingering deaths. The same conclusion may be inferred from the records of surgical practice. Chloroform has not lost favour in surgery, and if its use were followed by depression and prostration of the powers of the constitution after surgical operations, this fact alone would be a capital objection to its administration in such cases. No surgeon would use a means to save his patient the pain of an operation that would render the operation itself unsuccessful; but so far is chloroform from being proved to have such an effect, that the evidence is quite the other way. The statistics of surgical practice, the testimony of Mr Curling, Mr Miller, and others, demonstrate that chloroform is useful, not merely as a means of saving the patient pain, but of promoting his recovery. The nature of the vapour and its action on

the tissues, leads us to the same inference as to its operation on the constitution.

Chloroform is very volatile and very insoluble, as Dr Snow has fully explained in his valuable monographs on the subject. Mr Nunneley's experiments point out its local action. When applied to the skin, or any of the tissues, its immediate effect is, "to induce redness, some tumefaction and paralysis, with loss of feeling of the part which has been exposed to its influence." Bearing in mind these three facts, its action may be understood. The vapour of chloroform, diluted with atmospheric air according to the strength of the dose, when drawn into the lungs, is rapidly diffused over an immense extent of mucous surface, surrounded on all sides by innumerable blood-vessels and nervous fibrillæ. It first acts as a stimulant, exciting cough, hurried breathing, a sense of choking, cerebral excitement, and even convulsions; but this effect is almost instantly followed by the sedative influence of the vapour. When chloroform is perfectly pure, the stage of stimulation is scarcely noticed, being so rapidly succeeded by anæsthesia; but if impure, it is the reverse,—which proves the importance of attending to the quality of the chloroform that is used, if we would wish to measure accurately its effects. In the lungs it is brought into immediate contact with the ultimate ramifications of the nerves and blood-vessels that surround the vesicles, and passing through these, it is taken into the pulmonary circulation. Mr Nunneley[1] "is inclined to think the action is primarily in all cases, and principally, if not entirely, upon the nerves;" and if so, the sedative effect is instantly communicated to the cerebro-spinal axis, and from thence reflected over the whole of the sentient nerves. Such, however, is not the effect when locally applied elsewhere—the loss of sensation seems quite confined to the part, or to progress but slowly over the body; the extent of nervous surface exposed in the lungs, as compared with other parts, may, however, account for this difference. Dr Snow has shown, by experiments very carefully conducted, that the vapour of chloroform is absorbed into the blood, and that the quantity absorbed bears a fixed proportion to that which is inhaled. He gives a formula, also, by which the quantity of this and other vapours contained in blood may be ascertained; and he proves that those fluids which are least soluble in the blood, and the most volatile, act with the greatest power. Chloroform possesses both these properties. It is very volatile, and is therefore rapidly diffused. It is only slightly soluble in the serum of blood, and consequently the blood dissolves much less than it absorbs. This will explain the rapidity of its action; because a certain portion, being free, is immediately conveyed from the lungs to the heart, and from thence throughout the whole circulation. It will also account for the slight change that takes place in the blood, in

[1] Transactions of the Provincial Medical and Surgical Association, vol. xvi., pp. 11, 353.

proportion to the effect produced. In surgical operations the colour of the blood is unaltered. In animals that have been promptly killed by it, the lungs retain their scarlet colour; nor is it until the respiration is impeded by the vapour, while the heart is acting and distending the lung, or that the animal is exposed a very long time to its influence, that we perceive any real change in the colour or consistency of the blood. The rapidity with which its effects disappear may also be understood on the same principle. The moment that chloroform ceases to be inhaled, it quickly escapes from the circulation and is exhaled, leaving only a small portion behind to separate more slowly. In this respect chloroform differs essentially from alcohol, and may be contrasted with it. Alcohol being very soluble in blood, mixes with it freely, and circulates at the rate of the blood itself, hence it is comparatively slow in its operation; when inhaled, it produces little or no effect. We all know its effect when taken into the stomach—its powerful action as a stimulant, but having no anæsthetic effect, unless taken in large quantities. The two hydrocarbons may be placed side by side.

CHLOROFORM.	ALCOHOL.
Slightly soluble in serum.	Soluble to any extent.
Very slightly stimulating.	Highly stimulating.
A most powerful sedative even in small doses.	No sedative effect, unless when taken in large quantities.
Its effects rapidly disappear.	Its effects continue for hours.
Most powerful when inhaled.	Least powerful when inhaled.
Comparatively slight effect when administered by the stomach.	Effect most powerful when taken into the stomach.
No alteration in the appearance of the brain in cases where it has caused death rapidly.	Apoplectic congestion of the brain where it has been fatal.

Such are the differences in the effects of these fluids, and yet we are told that the insensibility of chloroform is only another term for intoxication. The amount of solubility in the blood seems to be a most important item in estimating the effect of anæsthetic agents. Their stimulant action appears to be in direct proportion to their solubility, their sedative power to be in inverse proportion. If we assume that a free undissolved portion of chloroform passes rapidly from the lungs to the heart, and thence throughout all the vessels of the body, its effects on the constitution may be understood. First on its arrival at the heart, a safe dose will only act as a stimulant, or perhaps not at all. In some cases where the patient is previously excited by apprehension, and the heart, in consequence, pulsates rapidly, chloroform may reduce these pulsations by controlling the mental excitement; but this is very different from the slower and *feebler* heat that is the result of its sedative effect. This symptom is most important to attend to, because sudden death is the consequence if the heart's action be suspended—the syncope of chloroform is fatal; and if too concentrated a dose be inhaled, the excess

of free chloroform may be so great as at once to paralyse it. The brain gives evidence of its effect in loss of consciousness, the medulla oblongata in slow deep stertorous respiration, the spinal marrow in more or less perfect loss of sensation and motion, and in the dying activity of reflex action. The different degrees of its influence on the vital functions have been fully pointed out by Dr Snow; but that effect which has the most important relation to our present subject is the independence of sensation and consciousness. Mr Nunneley observes, as the result of several experiments, that "the animal, after recovering from sopor, is often conscious long before there is any muscular power or even much sensation; indeed, after a moderate dose, or when the dose is not sufficient to induce a complete state of anæsthesia, consciousness remains where there is no power of motion and but little sensation, as everybody who has inhaled any of these substances knows."[1] This effect has been frequently noticed by the writer, and seems to him to be one of its most valuable actions in obstetric practice. A moderate dose will diminish if not remove sensation, without destroying consciousness. The patient is afforded relief from the extreme intensity of her sufferings, without being put to sleep, and also by a dose of chloroform that may be given without the slightest risk. The reverse of this sometimes happens under more full doses—consciousness is lost, but sensation remains—a metaphysical difficulty certainly, but which, nevertheless, seems to be true. In one of the cases related, the patient complained of her back, placed her hand upon it, seemed to feel the full force of her pains, but, after her delivery, knew nothing about it—she said "she *felt* no pain." When death takes place, the influence of chloroform on the vital functions may be either progressive or simultaneous. Loss of sensation, motion, and consciousness, may be followed by stertorous, laboured, feeble respiration, and this by the gradual cessation of the heart's action; or they may occur all at the same moment, the pulse, respiration, and consciousness, ceasing together. Hence the post-mortem appearances in man and the lower animals vary, and are influenced very much by the rapidity of the effect. After the most powerful doses the lungs are collapsed, the heart flaccid and empty, or the right side *moderately* distended by the cavæ, and the left ventricle contracted by the rigor-mortis. The brain is natural, but when death is less instantaneous the lungs are congested, ecchymosed, emphysematous; the right side of the heart and cavæ distended, sometimes enormously; the sinuses and membranes of the brain filled with blood.

This brief review of the action of chloroform on the animal economy leads us to the same inference as to its effects,—That the constitution is quickly brought under its influence, even to a nar-

[1] Transactions of the Provincial Association, p. 366.

cotism that may be fatal, but that it rapidly recovers itself when the vapour is withdrawn. This evidence it is hoped is sufficient to prove that chloroform does not necessarily cause depression or prostration of the vital powers. In the practice of midwifery the writer never has had occasion to regret the use of chloroform, because the patient's recovery was retarded or her health impaired; on the contrary, its chief value in his experience has been the rapidity with which the parturient woman's health has been restored when she was freed from the *depressing effects* of protracted and severe suffering. We may dispute about the necessity or the propriety of relieving the pains of childbirth, but none will question the propriety of promoting the patient's restoration to health, in whatever way we find that it may be best accomplished.

Dr Channing gives stronger evidence, because it is derived from a more enlarged experience of anæsthesia and its effects. He gives not only his own results, but those of his correspondents, every one of whom were requested to answer the important queries before alluded to. Whether they observed, "7, any special effects, physical, moral, or intellectual?—8, any apparent danger?—and to state the (9) results to mother, both immediately after delivery and later?" In reply to these questions, they mention as special effects unusual excitement in some cases, interruption and suspension of the action of the uterus in others; but the invariable answers to the two last questions were, "no apparent danger," "mother and child well." They allude to differences of effect while the patient was under the influence of the agent, but are perfectly silent as to any subsequent mischief. These witnesses were not all advocates of etherisation; on the contrary, some were evidently doubtful of the propriety of adopting it, and therefore their testimony may be received with more confidence. With a similar object, the American Medical Association appointed a committee to make inquiries on this subject, and to report upon it. They state that "the anæsthetic agents, ether and chloroform, have now been used in perhaps 2000 cases of midwifery, and, so far as the committee have been able to learn, without a single fatal, and very few, if any, untoward results. The committee, in a very extensive correspondence with physicians in various parts of the country, have found an entire unanimity of opinion among those who have tried these agents as to their favourable effect, both in advancing the progress of labour, and in relieving the sufferings of the patient."[1] It might, perhaps, be advanced, as an additional proof of the safety of chloroform, that its opponents have not given a single case accurately detailed in which subsequent unfavourable symptoms were clearly proved to result from chloroform. There has been no lack of general statements such as we have quoted, but there is no connected history of symptoms that can at all aid us in tracing out the mischievous influence asserted, nor can

[1] Medical Gazette, Dec. 1, 1848.

we perceive the relation of cause and effect in the same clear manner that is observed when the patient is under its immediate influence. There is the same difficulty in assigning chloroform as the cause of those unfavourable symptoms, as there is in attributing to it the deaths from puerperal fever and ruptured uterus.

Our knowledge of the properties and effects of this agent can only be acquired from facts accurately stated, but when those facts are mystified in general expressions of certain unfavourable results, they are valueless. It might, therefore, be asserted that the opponents of chloroform have no such facts to bring forward, and cloak such as they have in an ominous veil of mystery, so as to increase our dread of this agent, but the writer is very far from wishing to do so. Chloroform has no immunity from those accidents to which other and similar poisons are liable. Opium, aconite, belladonna, and hydrocyanic acid are known as powerful poisons, and most useful remedies; they are known, also, to disagree with certain constitutions, and to produce protracted and distressing symptoms; such is the case with almost every remedy in use: a few grains of blue pill has caused a profuse and exhausting salivation; the writer has known a desert spoonful of castor oil excite violent catharsis. Chloroform, therefore, cannot be an exception to this general principle; it must disagree with certain constitutions, and if no case had ever been quoted to prove its injurious properties, we should nevertheless feel assured that some such cases must arise when it is more generally used; but it would be just as absurd to decide against chloroform on the strength of such cases, as it would be to prohibit opium, mercury, or castor oil, because they sometimes act violently on peculiar habits. The writer will not deny that in certain temperaments, in constitutional idiosyncrasies, chloroform may produce most troublesome and unpleasant symptoms long after it has been inhaled, but these cases must be considered by themselves. They must not be put forward as one of many examples of a common effect. It is very desirable that we should know everything about such cases; we should be informed precisely what the symptoms were, how soon they appeared, the quantity of chloroform administered, the manner in which it was given, its immediate effects, and, above all, the previous history of the patient, both in respect to her general health and constitutional peculiarities. We have a right to demand a faithful account of them, not merely for the purpose of satisfying a doubt of their accuracy, still less with the object of intimidating the practitioner from the use of chloroform, but in order to guide him in the administration of this agent, and to prevent the uncertainty that arises from contradictory statements. At present all is confusion. We are told that chloroform causes convulsions, and cures convulsions. It is said to excite the hysterical patient to the degree of delirium, and even insanity—and that it tranquillises her. Chloroform causes prostration and depression of the vital powers after delivery; chloroform has a precisely opposite

effect, because it prevents the depressing effect of long-continued and severe pain. How is the unbiassed practitioner to decide between these contradictory statements, but by a careful examination of the facts themselves, by comparing these facts with his own observations, and deriving from the whole the truth? How can he do so if the details of such facts are not placed faithfully before him?

The disordered action of these ethers is an inquiry quite as interesting as their anæsthetic properties, but the time has not arrived when it can be dispassionately entered upon. At present, everything connected with etherisation is either branded with "Hic niger est," or adorned *à la couleur de rose;* the facts are therefore suspicious. There are some questions, however, well worthy an attentive examination, which the writer can only propose, without venturing upon their solution. Does chloroform produce any *permanent* effect on the nerves? Mr Nunneley[1] remarks, as the result of his experience, —"The persistence for days of the partial loss of sensation and motion in a finger which has been immersed in an anæsthetic substance (as I have had in my own finger). * * * And what is curious, I have sometimes observed it intermittent, returning for four or five evenings (corresponding with the time of immersion), far more perceptibly than in the intermediate periods."[2] Perhaps such may be its effect on the nervous centres in some cases after inhalation, causing periodic headache, and other disagreeable sensations. If this be true, what constitutions are most liable to this irregular influence? Susceptibility to the influence of chloroform differs remarkably in different constitutions. It would seem to be inversely as the strength of the individual. Mr Nunn's patient, in Westminster, was a strong labouring man. "Half an ounce of chloroform was used on a handkerchief, with the effect of causing only excitement; insensibility was not induced."[3] A lady called upon Mr Nunneley while suffering from a paroxysm of tic-doloreux, to have immediate relief. "I rubbed upon the face," says Mr N., "not more than twenty minims of chloroform, with success, but while doing so, another lady who had accompanied her, and *stood near*, complained of feeling the effect of it. On leaving my house, instead of proceeding to some shops, as intended, the head of the second lady became so confused and giddy, that she was obliged to make the best of her way home again; indeed, she walked so unsteadily, that she afterwards told me she was quite afraid of being supposed drunk. She laid down and slept for several hours. This lady was one of a family of the most highly hysterical diathesis I ever met with."[4] Another equally important question is the effect of chloroform on the blood. It has been proved that the full anæsthetic effect of chloroform, and even death, may be

[1] Transactions of the Provincial Medical and Surgical Association, vol. xvi., pp. 11, 293.
[2] Ibid, p. 361.
[3] Dr Snow on fatal cases of inhalation, p. 12.
[4] Transactions of the Provincial Medical and Surgical Association, vol. xvi., pp. 11, 374.

induced without the colour of the blood or its consistence being altered; but in some fatal cases the blood was dark and fluid. In animals that have been completely soporised, not once, but several times, the same was observed. In each successive experiment, the same dose of chloroform more easily soporised the animal, until the last dose promptly destroyed it. An interval of several hours, and sometimes days, elapsed between each experiment, sufficient to remove every atom of chloroform which might be in combination; yet, in the post-mortem inspection, the blood was darker and more fluid. Its coagulation was looser and softer, and every organ was highly congested.[1] It may be choloroform—it may be the imperfect oxydation of the blood—that produces this alteration; which ever be true, the question is well worthy an attentive examination.

While on this part of the subject, the writer is very reluctantly obliged to make some allusion to one effect attributed to chloroform, which, although this agent might be perfectly free from danger or risk, nay, although it might be administered with considerable benefit in practice, nevertheless, if it were true, would constitute an insurmountable objection to its adoption. The objection is best explained by the following passage from a pamphlet on the subject:—"As to dreams that occur in the second degree of inhalation, I may mention, that in a former pamphlet, after having stated that there was evidence of sexual excitement in some of those who had been etherised, I wrote thus—'The facts alluded to I do not think it right to relate in detail. It is sufficient to say that they are unfit for publication in a pamphlet that may fall into the hands of persons not belonging to the medical profession. I will assure those readers that they would create disgust unequalled by any other occurrence in life; that the bare thought of their relatives or themselves (if females), having been subjected to such revolting influences, would make them ever afterwards shrink from the observation of the world.'" It would have been well if the author of this paragraph had exercised the same reserve in making allusion to such a subject, as at first he had done in withholding his facts. But his assertions being questioned, we now have the assertion and the facts both together. "The facts recorded presently, regarding these dreams, will make manifest the truth of my former observations." In search of these facts, however, we have to wend our way through a physiological discussion on the seat of the sexual passion in the brain, and the reflex action upon it of the generative organs, through the reviews of journals, where allusion is made to sexual excitement in surgical operations, until we arrived at just two facts in support of the statement. One, a case of Baron de Bois, taken from the "Lancet."[2]

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1 *Vide* Mr Nunneley's experiments, 9, 10, 11, 12, op. cit., p. 179.

2 The case is as follows:—"After the expulsion of the fœtus, the mother thanked me mildly for the relief I had afforded her. When I asked her how she felt—if she remembered what had passed? she answered that she felt very

The patient did not use a single indecent expression, but said that "she dreamt," and after a cross-examination by the Baron as to the nature of the dream, in which he was obliged to have recourse to the intermediate communication of a respectable person of her own sex, she acknowledged that she dreamt "that she was with her husband." Another instance is related by an anonymous correspondent. In one instance only (a midwifery case), the woman appeared to mistake my manual interference in assisting her in labour, for a liberty taken with her person, and she screamed out, "Don't, you beast, do that! don't be such a beast!" This was, of course, distressing and unseemly. "One lady hummed a polka air, and assured me she was very fond of me, but this was only for a few seconds.!" [1] These are the only two cases (and collected from all possible sources) in which anything approaching to sexual excitement occurred in midwifery practice, and are the sole evidence advanced to prove the revolting influence of etherisation in childbirth. The reader can judge how far such testimony would authorise the insinuation conveyed in the paragraph quoted as to the general effect of chloroform.

The writer should not have alluded to such a subject if he had not made this asserted fact the object of his closest attention, and finding it to be perfectly without foundation, he feels bound to give the strongest denial to the statement founded upon it. It may be possible that in the lowest, the most degraded, the most unfortunate class of society, instances may be found in which, when the restraint of reason is removed, when the barrier is withdrawn that keeps in check the pollutions of the contaminated mind, a foul and loathsome stream of filthiness may issue forth, well calculated to create disgust. But that chloroform produces such an effect in any, the very lowest, rank of well ordered society, the writer believes to be utterly untrue. He can most conscientiously declare, that he has not met with a single instance in which anything approaching to indecency of expression or manner has been observed. Patients have occasionally become incoherent, and in their ramblings have talked of their domestic concerns—of their amusements; they have sung—they have prayed, but not one made use of a single offensive expression. Dr Denham gives similar testimony—"Another who required a very large quantity to affect her, became very loquacious, and spoke of domestic trials, and of many matters. But in no case did he hear an

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well, and that she dreamt. What did you dream of? was my question; but the patient turned her face aside with a smile, the peculiarity of which having drawn my particular attention, I renewed my question, but on her again having refused to let me know the nature of the dream, I had recourse, in order to ascertain it, to the intermediary communication of a respectable person of her own sex, and who was present at the operation of inhaling ether. To the same question being renewed, she answered, she had dreamt that she was beside her husband, and that he and herself had been simultaneously engaged going through those preliminaries which had led her to the state in which we now beheld her."—Misapplication of Anæsthesia, p. 36.

[1] Misapplication of Anæsthesia, p. 40.

immodest remark, or one calculated to offend the most delicate ear, nor did we ever observe the slightest approach to sexual excitement. We are therefore inclined to receive with caution the disgusting details and indecent stories told of patients while under the influence of chloroform."[1]

Having thus reviewed the whole of the facts bearing upon the third query respecting the safety of the patient, and the subsequent ill effects of chloroform, the writer has not the least hesitation in asserting, that there is no danger, present or future, from its judicious use. No proof whatever is given to establish the injurious consequences that result from the inhalation of chloroform. It is only by bringing forward exceptional cases to the exclusion of all the rest, that such assertions can be supported; cases in which a constitutional idiosyncrasy may render chloroform mischievous, in the same manner as the most popular remedies are sometimes known to act as poisons.

The safety of chloroform in the practice of midwifery, the writer believes to consist in the very small quantity of the vapour that is required to accomplish the desired object. Its obstetric use differs in this respect essentially from that in surgical practice.

In natural labour, the full anæsthetic influence of chloroform may certainly be safely induced, but it does not appear to the writer necessary to go at all so far. It seems to him sufficient to diminish the intensity of the pains. This may be accomplished without putting the patient to sleep, without even disturbing, in the least degree, her self-possession; perfect consciousness may remain, and yet the agony of suffering be completely subdued; when chloroform is given for this purpose, the quantity is so small, that no interruption to the action of the uterus could be produced; whenever this has happened, the dose has been sufficient to soporise the patient, but even here such an effect was only temporary, and passed away with the chloroform, when the uterine contractions returned with more power and efficiency than before it was inhaled. In order to subdue the pains of natural labour, the writer generally pours about a drachm of chloroform on the sponge of a small inhaler.[2] This is applied to the mouth alone, by which means atmospheric air is freely mixed with the chloroform, a certain portion passing into the mouth with the vapour, while the customary tract for respiration remains perfectly free. The quantity of chloroform really admitted into the lungs, diffused as it is in so large a quantity of air, must be very small—so small as not to excite the least sense of suffocation in its passage. If this quantity produce no effect, the dose is increased until it become sufficiently pungent to render the inhalation rather difficult. Its strength should always be tested before being given to the patient: a single inspiration will be sufficient to prove this. If the very moderate quantity first used should act with a power disproportioned to the dose; if it

[1] Dublin Journal, August, p. 138.

[2] The inhaler of Steevens and Pratt.

excite the patient, produce incoherency, or interfere with the action of the uterus, chloroform should be at once withdrawn. The time usually selected for its administration is in the second stage of labour, when the pains are increasing to their highest degree of intensity.

In operative midwifery, a more powerful dose is requisite. In such cases the writer prefers the inhaler of Dr Snow, by which the quantity of chloroform given can be accurately measured. When used by Dr Simpson, or those who are perfectly conversant with the effects of chloroform, the handkerchief is certainly safe; besides, it is convenient and most efficient; but in the hands of the profession at large, it appears to the writer to be the most hazardous method of administration that can be employed. As a proof of this, it is sufficient to say, that in all the fatal cases reported, with two exceptions, chloroform was given on a handkerchief or cloth. Dr Snow's inhaler being applied to the mouth and nose, inhalation is continued until sensation and consciousness are lost. If the patient become restless or incoherent, the dose must be increased, and a very few inspirations will be sufficient to produce sopor. The moment that the desired effect is obtained, inhalation should be discontinued, and not again renewed until there is some evidence that the influence of chloroform is disappearing. It should then be repeated only at intervals, according as it is necessary to maintain the effect. The quantity of chloroform consumed in these cases is greater than in natural labours, but the time of inhalation is much shorter, and in this respect more resembles surgical operations, with this difference, that in obstetric operations the degree of anæsthesia need not be so great. The perfect stillness essential to the success of an operation where the scalpel is cutting its way through the very springs of life, is not required for midwifery practice.

The importance of using pure chloroform is alluded to by every writer who has paid any attention to the subject. Pure chloroform should contain no oxygen, and the more it approaches to chloric ether, and to the properties of alcohol, the more exciting it becomes. Hence it is probable, that in those cases where most excitement was manifested, the effect might be attributed to the impurity of the chloroform employed. On the other hand, it should be remembered, that perfectly pure chloroform is most prompt in producing its sedative effect, and therefore it should not be recklessly administered. When the quantity of chloroform inhaled is sufficient for the purpose, it may be known by observing the pulse, the respiration, the voluntary muscles, and the eye. The pulse—that may have been increased—becomes slower and *fuller*. The respiration, also, is slower and deeper, as in sleep. The voluntary muscles have lost their power, —the arm drops. The eyes are inclined upwards; the pupil is sometimes dilated, but always contracts sluggishly. If the quantity is more than enough, the pulse may not only be slower, but *feebler;* a symptom which should never be passed over. The respiration becomes stertorous. In surgical operations this degree of narcotism is

generally required, but it is not so in obstetric practice. Spasmodic contractions of the voluntary muscles may occur. As soon as the respiration becomes slow, deep, and regular, chloroform may be withdrawn, because its action will continue to increase for sometime afterwards. If it should not do so, it can be renewed. By allowing sufficient time for chloroform to develop itself, and by ordinary attention to its effects, the practitioner can scarcely err in its administration.

The writer having given the subject of anæsthesia his best attention, he feels perfectly assured that chloroform neither impairs the contractile power of the uterus, nor injures in any way either the mother or the child; and that if ordinary caution be used, and if the administrator is conversant with the properties and the effects of the agent he is employing, there is no risk whatever in the administration of chloroform. But at the same time it must be understood that these conditions are absolutely essential. The closest attention should be paid to the manner in which the influence of chloroform is developing itself, and equal care should be given to the quality of the chloroform that is used.

Having satisfied himself that it can be given with safety, he feels that he is not only justified but bound to afford his patient that relief from the anguish of her suffering, which, in no other way, can be afforded her.

It is incorrect to consider the intensity of suffering—the agony that is often endured by the parturient woman—as a merely physiological or normal pain. The writer believes it to be, in the strictest sense of the term, a pathological or abnormal pain, the result of a higher degree of nervous irritability than exists in a perfectly healthy constitution. Further than this, the pains are more prolonged and intense than normal pains would be, because there are so many causes in operation, the result of our civilised and therefore *unnatural* habits, that render parturition much more severe than a perfectly natural labour would be. The diminution of the intense suffering of the parturient woman accomplishes more, much more, than merely saving her the agony of enduring it. It prevents the depression and exhaustion that is the consequence of intense and long-continued pains, and thus enables the constitution to recover itself much more rapidly than otherwise it would do. In fact, by bringing the amount of suffering nearer to what might be considered the true normal pains of labour, the recovery of the patient is proportionably favourable.

In conclusion, when he is satisfied that such benefits may be obtained through the medium of chloroform, the writer can feel no regret or self-reproach in employing it, although it might so happen that one in a thousand cases may manifest its disordered action.

ARTICLE II.—*On Typhoid and Typhus Fevers,—An Attempt to Determine the Question of their Identity or Non-Identity, by an Analysis of the Symptoms, and of the Appearances found after Death in Sixty-six Fatal Cases of Continued Fever, observed at the London Fever Hospital from January* 1847 *to February* 1849. By W. JENNER, M.D., Lond., Licentiate of the Royal College of Physicians, Professor of Pathological Anatomy in University College, London.—(*Continued from p.* 1112.)

LARGE INTESTINES.

Typhoid Fever.—The large intestines were not examined in three cases; four proved fatal after the 35th day of the disease, so that so far as the colour and consistence of the mucous membrane are concerned, sixteen cases only are available for analysis.

The colour and consistence of the mucous membrane in ten of the sixteen were normal, or nearly so. In two of the ten there were ulcers on the mucous membrane of both cæcum and colon.

In two cases the mucous membrane of the large intestines was softened throughout; in one of the two it was pale throughout; in the other the ascending colon was deeply injected, the remainder of the gut being pale.

In one of the sixteen cases the mucous membrane of the cæcum was pale grey, and normal in consistence and thickness; the colon in the same subject being bright red, mottled with patches of grey; in another the lining membrane of the cæcum was slightly softened; the ascending colon, here and there, dull red, and slightly softened, the transverse and descending colon being normal in consistence and thickness; the cæcum was pale in one case in which the ascending colon, just above the cæcum, was highly vascular, but from that part to its termination, pale. Its consistence in this case was normal. In the three last cases there were ulcers in either the cæcum or colon, or in both. In one case there was unequivocal evidence of inflammation of the colon. The subject was a man æt. twenty-eight, who died on the 27th day of the disease; the mucous membrane of the colon immediately above the cæcum,—which was pale and healthy in consistence and thickness—was minutely injected, and scattered over the same part were patches of a bright red colour. The mucous membrane at these deeply red parts looked roughened, the minute elevation being of a deeper hue than the floor on which they were seated; on the deepest coloured spots were shreds of lymph-like matter, of a yellowish colour. These patches of shreds had a transverse direction—*i. e.*, ran round the intestine; these shreds were capable of being removed without injury to the subjacent membrane. The mucous membrane itself was neither thickened nor softened, but was capable of being removed in larger and longer strips than usual, probably from softening of the submucous cellular tissue. At this

part of the colon the solitary glands were not visible, but farther on they were found without difficulty. They did not appear diseased in any way. There was no ulceration of the large intestines in this case.

Ulceration.—Twenty of the twenty-three cases may be considered with reference to this point—*i.e.*, all those in which the large intestines were examined. In seven of the twenty, ulcers existed in the cæcum or colon, or in both. Generally round and small, they were occasionally oval, and of considerable size; in the latter case their long axis lay in the direction of the circular fibres of the intestine. Sometimes their floors were formed of submucous cellular tissue, at others of muscular fibres, and in one instance by peritoneum only. The following descriptions will show that their first development resembles, in all particulars, the origin of the ulcers observed in the small intestines in the cases here examined. A male, æt. 23, died on the 25th day of the disease; the colon was bright red, mottled with grey; scattered over its whole surface were numerous round spots about a line in diameter, elevated, some of them considerably, above the level of the surrounding membrane. Their centres were white and opaque, their bases grey. The white opaque central spot had, in some instances, separated at its circumference from the surrounding membrane, still, however, adhering to the submucous tissue; the line of separation was deep red. Again, at other places this line had widened so as to form a complete furrow, the opaque white mass still adhering more or less firmly to the submucous cellular tissue; while in a third set of cases the opaque white mass had disappeared, and a small ulcer had been formed. The elevation of these spots was caused by an opaque white deposit in the submucous tissue. In the descending colon the deposit causing the elevation was greater in amount, and the round spots were larger and stood higher above the surrounding level, than in the parts of the colon near to the cæcum. In another male, æt. 28, who died on the 31st day of the disease, there were about twenty ulcers seated on the four inches of colon immediately adjacent to the cæcum; they were oval, and varied from one-fourth to one-and-a-half inch in length, the largest being one-half inch broad, their long diameter corresponded to the transverse fibres of the intestine. They were situated between the transverse folds of the gut. Their edges were undermined, grey and rounded. Their floors formed of transverse muscular fibres; but in a few places there was little more than peritoneum to prevent the escape of the fœcal matter. There was no appearance of solitary glands in this case. In a third case, which proved fatal on the 46th day, the cæcum and ascending colon were thickly studded with ulcers, varying in diameter from one to four lines, the edges of which were scarcely elevated, their floors being formed by submucous cellular tissue, or transverse muscular fibres. In addition to these ulcers, there were interspersed among them numerous opaque white, smooth, shining spots, one or two lines in

diameter, the borders of which were grey, the mucous membrane around being slate-coloured, and slightly puckered toward them.

It admits of question whether the ulceration in these cases had its origin in the solitary glands, and although, in the first case, the general appearance of the elevations studding the mucous membrane, closely resembled enlarged solitary glands ; yet I by no means feel confident that they were such in reality. The analogies of the plaques molles and plaques dures are clearly seen in the above cases, and the last case exhibits what was seen to be true with respect to ulcers in the small intestines,—viz., that while after the fever had run its course, some of the ulcers might heal, yet that others might pass into the state of simple ulcers, and prolong the illness to an indefinite period.

Typhus Fever.—The large intestines were examined in thirty-seven cases before the termination of the 4th week of the disease. In twenty-eight of the thirty-seven the mucous membrane lining them was normal, or nearly so, in colour and consistence.

In one case the cæcum, in one the cæcum and colon generally, and in one the cæcum and ascending colon, were congested. The congestion, though marked, was confined to the larger vessels, and was ramiform in character. In one case the cæcum and colon were pale, but slightly softened; and in one the softening was limited to the transverse arch.

In four cases only were unequivocal traces of inflammation of the mucous membrane of the large intestines present. A female, aged twenty-nine, died toward the end of the third or during the fourth week of the disease. The appearances found after death in the intestine have already been described. In another of the four cases, the patient, a woman, aged forty-seven, died on the 12th day of the disease. In the colon, just above the cæcum, were four large patches running transversely, of a deep red colour; the mucous membrane at this part was thickened, and decidedly softer than that around. Although it was in this case rather softer throughout the colon than usual, still it was not more so generally than might be considered within the range of health. With the exception of the patches, just described, the mucous membrane of the large intestine was pale. The third case also was that of a woman, æt. forty-three, who died on the 16th day of the disease. Scattered over the eighteen inches of colon next the cæcum, there were about twenty patches, varying from one-quarter to half-inch in diameter, irregular in shape, of a dull yellow colour, considerably elevated. The mucous membrane which immediately surrounded them was of a bright red colour, gradually fading as it receded into the natural hue of the intestinal mucous membrane. The yellow matter itself was readily removable, friable, and finally granular. It appeared to be a deposit of lymph in and on the mucous membrane, a portion of the latter being carried away when the former was removed. In the transverse and descending colon were a few linear patches, resembling the yellow spots

above described. Some of these ran transversely, others longitudinally; the latter appeared in some measure to correspond in course and situation to the longitudinal bands. The mucous membrane of the colon generally was rather soft, of normal thickness, and pale. The fourth case in which there were signs of inflammation of the mucous membrane of the colon was that of a male, æt. thirty. In the sigmoid flexure was a patch, about four inches in diameter, of vivid redness; the mucous membrane at the same part was decidedly softened. With these four exceptions, there was no trace of inflammation of the mucous membrane of the large intestines.

The frequency of dysentery as a complication of typhus fever, when patients are placed in unfavourable circumstances, ought to make us particularly careful in admitting such evidence (excepting M. Landouzy's[1]) as Gaultier de Claubrey has adduced in support of the opinion, that the typhus fever of camps and jails of old writers, and typhoid fever, are identical. For we see, even by the examples here brought forward, that lesions of a very serious nature may affect the intestinal mucous membrane in typhus fever; but, at the same time, we see also that these lesions are of a very different character from those occurring in typhoid fever. Therefore, to show that petechiæ, diarrhœa, and delirium, were present during life in a number of cases, and that some lesion of the intestine was found after death in the same cases, is by no means to prove that these persons died of typhoid fever. The cases here analyzed, like those recorded

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[1] I may here briefly observe, that M. Landouzy appears to have had, in the prison at Rheims, cases both of typhus and of typhoid fevers. The presence of petechiæ by no means proves that a case is not typhoid fever. Petechiæ may occur in scarlet fever, small-pox, and in many other diseases (as all who have witnessed much of these affections will readily allow), when the patients are placed in unfavourable circumstances, and occasionally even under other conditions. There is no proof that the mulberry or true typhus rash was present in M. Landouzy's cases, or that true rose spots ever passed into petechiæ; nay, it is even within the range of probability that some of the so-called petechiæ were really flea-bites. Similar circumstances favour the spread of typhus and typhoid fevers. The poison of both appears subject to the same laws of development, therefore where one exists there the other is likely to exist. The apparent discrepancies in the observations of the older writers on camp and jail fevers may probably be explained thus:—Their descriptions of the disease varied, because they had at least two diseases to describe, each capable of assuming a mild or a severe form. Now, the varied proportions in which these two diseases prevailed at different times, the presence of one disease only in the camp or jail, and variations in the severity of type, must have necessarily caused confusion in the accounts of camp and jail fever. The same confusion, from the same cause, exists in the descriptions of typhus fever by the writers of our own age, who have not drawn the distinction, which nature has, between typhus and typhoid fevers. The attempt to settle the question of the identity or non-identity of these diseases by a reference to old writers, appears as absurd as it would be for astronomers of the present day with eyes in their heads, telescopes in their hands, and the heavens above them, to found their opinions respecting the movements of certain celestial bodies on the dicta of Ptolemy, or the observations of Copernicus.

by previous writers, prove that neither typhoid nor typhus fever is gastro-enteritis.

MESENTERIC GLANDS.

Typhoid Fever.—These organs were more or less extensively and unequivocally diseased in all of the twenty-three cases I am considering. In every case they were larger than natural, and their volume increased as they approached nearer to the ileo-cæcal valve.

Their size varied from a pea to a pigeon's egg; their colour from pale rose to dark grey; their consistence from firm to soft and flabby, and soft and friable. The deep grey of these organs, with one exception, was limited to those cases which proved fatal after the 30th day. The four cases, also, in which they were recorded to be flabby, all proved fatal after the same date of the disease.

In three cases one or more glands were in a state of suppuration.[1] In two of the three the purulent-looking fluid, collected in a mass, was separated from the abdominal cavity only by a thin layer of peritoneum. In the third the purulent fluid occupied distinct points in the glands. Two of these three cases proved fatal respectively on the 20th and 30th day, the third at least five weeks after the commencement of the disease.

In a fourth case, in which probably the suppurative process took place, the patient recovered so far as the fever was concerned, and the glands were reduced at the time of death to a state in which they could no longer have exercised any injurious effect. The patient died of erysipelas, on the 86th day of illness.[2]

In four cases there was a deposit of opaque, pale yellowish friable matter in the substance of the mesenteric glands. This appeared to be exactly the same material as that seated in the submucous cellular tissue, corresponding to Peyer's patches; and microscopic examination demonstrated the identity of the minute structure of the two deposits. Three of the four cases in which this deposit was found proved fatal respectively on the 12th, 25th, and 46th days; the fourth at least five weeks after the commencement of illness, and three weeks after the admission of the patient into the hospital.

That the before-referred to pseudo-suppuration sometimes occurs *around* this deposit, the annexed description of the appearances found after death in the mesentery of a man, aged thirty-three, who died on the 20th day of the disease, will, it appears to me, afford sufficient proof. The mesenteric glands were considerably enlarged. They varied in size from a pea to a walnut; just behind the cæcum was a

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[1] Though resembling in external characters true pus, this fluid differs considerably when seen by the aid of the microscope.

[2] For particulars of this very interesting case, I must refer the reader to the Medical Times for the present month. The observation is too long to introduce here.

mass, the size of a large pigeon's egg. Externally they were of a deep slate grey. On dissection, a considerable quantity of reddish purulent-looking fluid escaped from them. In the centre, and quite detached and loose, and bathed on all sides by this purulent-looking fluid, was a solid mass, in one of the largest glands about the size of a small walnut, of a delicate pink colour, finely granular externally, and on section firm and friable. In some of the glands the disorganising process appeared to have proceeded further. The central solid pink mass being replaced by a semi-purulent clot. Microscopic examination demonstrated this mass to be made up chiefly of typhous matter, as it has been called.

The mesocolic glands were noted to be enlarged in five cases, in one the enlargement was limited to the vicinity of the colon adjacent to the cæcum, and these glands, which were dark-red and softened, contained some purulent-looking fluid.

In the case in which there was extensive ulceration at the lower part of the œsophagus, there was considerable enlargement of the corresponding *œsophageal lymphatic glands*.

In two cases the *lumbar glands* were enlarged.

Typhus Fever.—In one case, that of a child, æt. 10, in whose lungs and agminated glands numerous tubercles were found, the mesenteric glands varied in size from a pea to a small bean, the largest corresponded to the middle of the small intestine. These glands contained some tubercular matter.[1] In the same case the mesocolic glands were dark grey and firm, and some of them were as large as peas. In another case, that of a child in whose lungs was found some tubercular deposit, the mesenteric glands were large and pale.

With these two exceptions, no deviation from a healthy structure was observed in the mesenteric or other lymphatic glands, in either of the forty-three subjects dead from typhus fever. So that patients labouring under typhus fever are exempt from acute disease of the lymphatic glands, at least to any appreciable extent. How much they differ in this respect from those affected with typhoid fever I need not here point out.

PERITONEUM.

Typhoid Fever.—In five of the twenty-three cases there were signs of peritonitis, *i.e.* increased vascularity, with effusion of turbid serosity and lymph, or recent adhesions. In four of the five cases the inflammation had extended over nearly the entire surface of the peritoneum. In three cases perforation of the coats of the gut appeared to have been the cause of the inflammation. In a fourth case, the co-existence of perforation was a matter of doubt, as I was not permitted to complete the examination. In the fifth case, the peritonitis depended

[1] See page 1111.

for its origin on perforation of the peritoneum covering a gland in the process of recovery from a state of suppuration.[1]

Typhus Fever.—In no case was there any trace of recent peritonitis.

SPLEEN.

Typhoid Fever.—Weight.—Excluding the cases of the subjects under fifteen years of age, those which survived the 35th day of disease, and those the spleens of which were not weighed, there remain only eleven cases for analysis. The average weight of the spleens in these eleven cases was 10 oz. 3 drms. avoirdupois; neither of them weighed less than 6 oz., two exactly 6 oz. each, and one as much as 14 oz.; the patients from whom the two former were obtained, died respectively on the 21st and 31st day of the disease, the last on the 27th.

The size of the organ was nearly in proportion to its weight; its vertical measurement varied from four and a-half, to seven inches.

Consistence.[2]—The condition of the spleen with reference to its consistence, was recorded in fourteen cases that proved fatal before the 35th day. In four of these fourteen cases, it was decidedly softened, in one of the four pulpy; these four cases terminated respectively on the 23rd, 25th, 27th, and 28th day of disease. In the remaining ten cases the spleen was of normal consistence; these ten cases terminated respectively from the 12th to the 34th day inclusive.

It is worthy of remark, that seven of the fourteen cases terminated fatally between the 20th and 31st days; and that it was in this group that the four cases of softening of the spleen were found. Now this is the period of the disease in which M. Louis found the organ the most frequently softened.

Relation between Enlargement and Softening of the Spleen.—The four spleens which were decidedly softened, weighed respectively 11 oz., 13 oz., 13 oz. 4 drms., 14 oz.

Two of those which were as firm as in health, 11 oz., and 12 oz. 4 drms., the patients from whom these two were removed, died on the 12th and 20th days of disease. The others less than 10 oz.; two of them only 6 oz., these two died respectively on the 21st and 31st day of disease.

Typhus Fever.—The spleen was weighed in thirty-four of the forty-three subjects aged more than fifteen years, who died before the termination of the fourth week of disease.

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[1] This case has been before referred to, and is detailed in the "Medical Times" for the present month.

[2] In examining the consistence of an organ, there is no certain standard of comparison; so that what one observer terms normal in consistence, another may call softer than natural. It is evident that the term of expression will vary according to each man's own idea of what the normal consistence of any given organ is. I have endeavoured to restrict my application of the term softening, to those cases regarding which there could be no difference of opinion.

Its average weight was 7 oz. 5 drms.

In nine, or rather more than one-fourth of these thirty-four cases, it weighed less than 6 oz. In one case only 2 oz. 6 drms.

In seven, or rather more than one-fifth of the cases, its weight was greater than the average weight of the same organ in the case of typhoid fever. In two cases it weighed as much as 14 oz.

Its size was usually in proportion to its weight. Its vertical measurement varied from four to seven inches.

Attempt to determine the co-existing conditions which favoured enlargement of the spleen.—Sex.—Of the above thirty-four cases, seventeen were males, and seventeen were females.

The average weight of the spleen in the seventeen males, was 7 oz. 2 drms. Four of the seventeen spleens removed from the male subjects, weighed less than 6 oz., two more than the average weight of the typhoid spleens, one as much as 14 oz., one as little as 2 oz. 6 drms.

If the male cases which proved fatal before the 21st day only be taken, the average weight of the spleen in them was 8 oz. 2 drms., one only weighing less than 6 oz., and two more than the average of the typhoid spleens.

The average age of these seventeen males, was forty-six years eight months, the eldest was sixty-five, the youngest twenty-nine; eight of them were under fifty years of age.

The average weight of the spleen in the seventeen females, was 8 oz. 4 drms., five of these seventeen spleens weighed less than 6 oz., and five more than the average weight of the typhoid spleens. The heaviest weighed 14 oz. 4 drms., the lightest 3 oz. 4 drms.

If the cases (female) which proved fatal before the 21st day only be taken into the calculation, then the average weight of the organ was 7 oz. 2 drms., five of the spleens weighing less than 6 oz., and three more than the average weight of the typhoid spleens.

The average age of these seventeen females was 43 years 9 months—the age of the eldest was 70, of the youngest 22 years—ten of the seventeen being under 50 years of age.

Thus it is evident that sex exerted no influence over the size and weight of the spleen in these thirty-four cases.

Age.—The spleens of thirteen subjects between the ages of 16 and 40 inclusive, were examined before the termination of the 4th week from the commencement of the typhus fever. The average weight of the organ in these thirteen cases was 9 oz. 1 drm.—two of them weighing less than 6 oz., six or nearly one half more than 10 oz. 3 drms, *i. e.*, the average weight of the spleen in those dead from typhoid fever.

The spleen was weighed in nineteen cases, the ages of which varied from 41 to 70 years inclusive. In these nineteen the average weight of the organ was only 6 oz. 6 drms., 6 or nearly one-third weighed

less than 6 oz., one only weighed more than the average of the typhoid spleens.

Thus the age of the patients appears to have exerted an appreciable influence over the size attained by the spleen in the cases of typhus fever.

If instead of taking the average of the spleen in these thirty-four cases, the weight of the organ in the subjects the ages of which corresponded with the ages of the cases of typhoid fever here analyzed, *i. e.*, were under 40 years, be considered, and these cases be still further limited to those which proved fatal before the termination of the 21st day of disease, then there will be nine cases for examination, and the weights of the spleens in these nine will nearly correspond with the weights of the same organ in the cases of typhoid fever,—thus the average weight of the organ in these nine cases was 10 oz. 4 drms., none weighing less than 6 oz., and 6 over the average of the weight of the same organ in the cases of typhoid fever.

But if the cases of the patients between 40 and 50 years of age, who died by the 21st day of the disease, only be considered—there were eight of them—the average weight of the spleen in these eight cases was 6 oz. 4 drms., and while four of the spleens weighed less than 6 ozs., only one exceeded in weight the typhoid spleens.

The spleens of eight patients, whose ages exceeded 50 years and who died before the 21st day of disease, were weighed. The average weight of the organ in these eight cases was 7 ozs. 1 drm., one only weighing less than 6 oz., and none more than 9 oz.

Consistence.—The consistence of the spleen was notably diminished in thirteen cases. In these cases its consistence was characterised by the terms pulpy, very soft—soft—rather soft.

In eighteen cases it retained its normal consistence. In these cases it was stated in my notes to be firm, healthy in consistence, or moderately, or tolerably firm, or not decidedly softened.

In the remaining twelve cases, there was either old disease of the organ present, or the patients survived beyond the 4th week of the disease, or the consistence of the organ was not noted.

Influence of sex—season—local complications—cadaveric change—condition of the blood—age—and duration of the disease in determining the presence or absence of softening of the spleen.

Sex.—Nine of the eighteen subjects in which the spleen was of good consistence were males; 9 females.

Six of the thirteen in which it was softened were males; seven females.

The proportion of males to females, in both instances, was, therefore, as nearly equal as was possible, so that sex exerted no influence in producing softening of the spleen.

Season.—Seven of the eighteen subjects, the spleens of which were of normal consistence, were examined during the 6 winter months, eleven during the 6 summer months.

Six of the thirteen subjects, in which the same organ was softened,

were opened during the 6 winter months, seven during the 6 summer months. The latter, therefore, are in as nearly equal proportion as is possible, seeing the total is an odd number. Thus season, like sex, had no part in causing the change in the consistence of the spleen.

Local Complications of an Inflammatory Nature.—These appeared to exert little influence on the consistence or size of the spleen. Five of the eighteen cases in which that organ was firm, were uncomplicated, and four of the thirteen in which it was softened.

Was the Softening the effect of a Cadaveric Change?—Three of the cases in which the spleen was very soft, or soft, were examined respectively, 8, 9, and 11 hours after death, and only three of the thirteen cases more than 30 hours after death, while eight of seventeen cases, in which the spleen was of normal, or nearly normal consistence, were examined more than 30 hours after death.

It appears highly probable that this change, then, was pathological and not cadaveric; for although the subjects, the spleens of which were so decidedly softened 8, 9, and 11 hours after the fatal termination, were examined in the months of July and September, yet the temperature was noted to be cool at the time the two former lay dead.

Relation between Softening of the Spleen and the Condition of the Blood.—Of six cases in which the blood was fluid, or in which there was merely a little soft black clot in the right side of the heart, two had the spleen soft, and four firm, and as there were eighteen of the latter to thirteen of the former, this condition of the blood could have exerted little influence in producing the variations in the consistence of the spleen.

Age.—Of the thirteen subjects in which the spleen was noted to be of normal consistence or firm, there were—

Aged 30 years and under,	5	*i. e.* $\frac{5}{13}$,	or in the proportion of		38·46 per cent.
... between 31 & 40 incl.,	3	... $\frac{3}{13}$,	...	...	23·07 per cent.
... ... 41 & 50 ...	4	... $\frac{4}{13}$,	...	...	30·77 per cent.
... ... 51 & 62 ...	1	... $\frac{1}{13}$,	...	...	7·68 per cent.

If to these be added the five subjects in which the organ was noted to be of tolerably firm, &c., consistence, there will be—

Aged 30 years and under,	6	*i. e.* $\frac{6}{18}$,	or in the proportion of		33·33 per cent.
... between 31 & 40 incl.,	4	... $\frac{4}{18}$,	...	...	22·22 per cent.
... ... 41 & 50 ...	5	... $\frac{5}{18}$,	...	...	27·77 per cent.
... ... 50 & 65 ...	3	... $\frac{3}{18}$,	...	...	16·66 per cent.

Of the thirteen subjects, the spleens of which were *decidedly* softened, there were—

Aged 30 years and under,	1	*i. e.* $\frac{1}{13}$,	or in the proportion of		7·68 per cent.
... between 31 & 40 incl.,	2	... $\frac{2}{13}$,	...	...	15·38 per cent.
... ... 41 & 50 ...	4	... $\frac{4}{13}$,	...	...	30·77 per cent.
... ... 51 & 62 ...	6	... $\frac{6}{13}$,	...	...	46·15 per cent.

So that, of the spleens undoubtedly softened, rather more than three-fourths belonged to subjects more than forty years of age, and nearly one-half to subjects more than fifty years of age; while of the spleens, the consistence of which was as firm as in health, decidedly less than half were removed from subjects more than forty years of age, and one only from a subject more than fifty years of age; and if to the latter group be added the five in which the consistence was so slightly diminished as not to be considered abnormally so, still less than half were removed from subjects the ages of which exceeded forty, and three only from subjects more than 50 years of age.

Duration of the Disease.—Fifteen spleens of normal or nearly normal consistence, were removed from subjects, the duration of the disease in which was known. Of these fifteen spleens there were,—

Belonging to subjects who had been ill a period not exceeding 14 days, 5
... ... between 14 and 29 days, 10

Of ten spleens noted to be decidedly softer than natural, there were,—

Belonging to subjects who had been ill a period not exceeding 14 days, 7
... ... between 14 and 17 days, 3

and not one of the four subjects in which this organ was noted to be *very* soft, had survived the 14th day of the disease.

Relation between the weight and consistence of the Spleen.—Twelve of the softened spleens were weighed, of these,—

3 or one-fourth weighed less than 6 oz.
11 less than ... ... 9 oz.
1 ... ... 11 oz. 4 drms.

Sixteen of the spleens of normal consistence were weighed, of these,—

3 or rather less than one-fifth weighed less than 6 oz.
8 or one-half more than ... ... 9 oz.
2 as much as ... ... 14 oz.

So that the smaller spleens appear to have been the softer.

From this rather lengthened analysis it is evident,

1.—That the spleen was considerably enlarged in the cases of Typhoid and Typhus fevers here analyzed.

2.—That it is generally larger in the subjects dead from Typhoid, than in those dead from Typhus fever.

3.—That if the cases of Typhus fever be limited to those, the ages of which do not exceed forty years, then the relative difference in the weight of the spleen in the cases of Typhoid and Typhus fevers, which prove fatal respectively before the 35th, and the 28th day of the disease is considerably diminished.

4.—That the average weight of the organ is exactly equal in subjects dead from Typhoid and Typhus fevers, if the cases of the latter disease only be considered, which prove fatal before the 21st day of

the disease, and which do not exceed forty years of age, *i. e.*, the age after which no case of Typhoid fever (here analyzed), proved fatal.

5.—That after the age of forty, the spleen is much less enlarged in Typhus fever than it is before that age. How far this holds true with regard to Typhoid fever, the cases here considered afford no means of determining.

6.—That softening of the spleen in cases of Typhus fever is much more frequent after the age of fifty, than before that period of life.

7.—That softening of the spleen both in Typhoid and Typhus fevers, is much more frequent in those cases that terminate unfavourably at an early period of either disease, than in those which prove fatal at a later period.

8.—That in no case of Typhoid fever (here analyzed), was the spleen found softened, after the 28th day of the disease.

9.—That softening of the spleen is much more frequent in cases of Typhus fever before, than after the 14th day of the disease.

We may therefore conclude that the age of the patient, and the duration of the disease are the two circumstances which exert the greatest influence in determining the presence of softening and of enlargement of the spleen in Typhus and Typhoid fevers.

(*To be continued.*)

ARTICLE III.—*On Anormal Nutrition in Articular Cartilages.* By P. REDFERN, M.D., London, Lecturer on Anatomy and Physiology, and on Histology, at the University and King's College, Aberdeen. (*Continued from p.* 1127.)

Before proceeding with the subject of disease in articular cartilages, it is necessary to notice their position amongst the tissues, and their mechanical and vital properties in the healthy state. They are formed of a texture, which, in the purest state in which we see it, consists of a mass of nucleated cells, similar in every respect to those of the cellular textures in plants. Other forms of cartilage have their cells spread out in a granular or fibrous mass, called the hyaline or inter-cellular substance of cartilage. The cells, in every form, are capable of producing others, so that, whilst the old cells are constantly disappearing, others are growing to supply their places; and thus the tissue is continually undergoing important nutritive changes, by which its functions are preserved to the latest periods of existence. Its powers of absorption are very great, and there can be little doubt that, by these, articular cartilages receive a sufficient amount of nutritive material from the blood which circulates in the vessels of the bone or synovial membrane.

In the Philosophical Transactions for 1841, Mr Toynbee pointed out that the blood-vessels of the bone, near a cartilaginous surface,

present certain convolutions, dilatations, plexuses, and other peculiarities, well adapted for the reception of blood, from which the liquor sanguinis may be separated by exudation, for the purpose of nourishing the cartilage. He showed, also, that these vessels are separated from the cartilage by "a lamella of bone, composed of two sets of osseous layers; the one, dense and thick, is continuous with the vertical fibres of the cancelli; the other, delicate and thin, principally composed of osseous corpuscles, is situated at right angles to the latter, and fills up the interspaces of the vertical fibres." Mr T. believes that articular cartilage is chiefly nourished by exudations from the above-named blood-vessels, the arrangement of which, with the absence of any other means of nutrition, have led him to this conclusion. He points to the existence of a number of osseous lacunæ, without other openings or canals, in the articular lamella of the bones; and to a series of minute canals, situated in the deeper parts of the cartilage, and leading towards the free surface, as means whereby the passage of nutritious fluid from the blood-vessels of the bone into the substance of the cartilage may be facilitated. In the fibro-cartilages, Mr T. states, that the blood-vessels uniformly terminate within the boundaries of the fibrous tissue, without passing into the part formed of cartilage. He regards the fibrous portion of fibro-cartilage as formed from the cells of the cartilaginous part, having found that, in the fœtus of three and seven months, the external part of the inter-vertebral substance contains cells elongating into fibres, whilst up to adult age, the changes in it consist in the encroachment of the fibrous portion upon the cartilaginous. It may be noticed here, that Mr T. also maintains that, during the whole of life, articular cartilages gradually become thinner by being converted into bone.

Dr Leidy[1] has different views regarding the structure and mode of nutrition of articular cartilages. He describes the cartilage corpuscles as solid nucleolo-nucleated cells lying in groups. The contents of the cells are a translucent, homogeneous, or minutely granular substance, with a central mass of distinctly coarser and darker granules, in which a transparent, round, or oval nucleus can be seen, coloured brown by the application of tincture of iodine. He has described and figured the cartilage matrix, as a substance made up of filaments $\frac{1}{25000}$th of an inch in diameter, composed of a single row of granules. These filaments are arranged horizontally at the free surface, but vertically in the deeper parts of the tissue. To this arrangement of the filaments, he attributes the horizontal and vertical arrangement of the cells, which, he says, in being developed amongst fibres, will necessarily extend in the direction in which there is least resistance, in the line of direction of the filaments,—and also the direction taken by fractures in different parts of the thickness of the cartilage. He regards the intra-cellular substance as originating in the exercise

[1] American Journal of Medical Sciences, April 1849.

of the vital activity of the cells, which accumulate and fix around and within themselves certain matters which they derive from the surrounding nutritive fluids, as happens in the case of many zooid plants,—the cells themselves multiplying by division into masses which ultimately become cells like the former. Dr L. believes articular cartilage to be nourished by fluids poured out from the blood on its attached surface, and by the *circulus vasculosus* at its circumference, but especially by the synovial fluid, which is rich in albumen, the substance from which he regards it as certain that the tissue of cartilage is formed. Henle noticed the probability of the occurrence of this third mode of nutrition many years ago, but it is difficult to conceive that the synovial fluid should serve the purpose of nourishing the tissue, the free surface of which, there can be no doubt, it is intended to lubricate. Again, the synovial fluid is deficient in potash, magnesia, and sulphuric acid, which are found in cartilages; and, moreover, the costal and many other cartilages are certainly not nourished by it. The filamentary arrangement of the molecules of the healthy inter-cellular substance is important, as it is shown, by the preceding observations, that, in the most frequent changes to which articular cartilages are liable under the influence of increased or anormal nutrition, the hyaline substance on the surface splits into bands and fibres parallel to it; whilst, in the deeper parts, these are always arranged more or less vertically.[1]

It was noticed by Mandl that the nuclei of fibro-cartilage more frequently contain fatty matter than those of true cartilages; and Henle remarks that whenever fibres become developed in the inter-cellular substance of certain cartilages, the nuclei of their cells become converted, in great part, into fat, as if the two processes were connected with each other, whilst the cytoblasts never contain fat in those cartilages which do not become fibrous. The latter author also adds, that whilst fibres are frequently found in advanced life, in those cartilages which have a tendency to ossify, he has never seen a trace of fibre in those which do not ossify, as the articular cartilages and those of the nose.

So far as I am aware, the actual conversion of the hyaline substance of articular cartilage into fibres, in disease, has not been previously demonstrated, though the external appearances of such cartilages are sufficient to lead to a suspicion of its occurrence. In many of the states of disease which have been described in the preceding observations, the fusion of the walls of old cells with the inter-cellular substance, and the division of the gelatinous cell-contents

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[1] I am indebted to my friend, Dr W. T. Gairdner, for the opportunity of perusing Dr Leidy's paper after the first part of these observations was in the hands of the Editors. The splitting of the hyaline substance, so manifest in the first observation, was the subject of a paper read before the Medico-Chirurgical Society of Aberdeen, on the 4th of January last.

into as many pieces as there are corpuscles or nuclei in the interior, were seen to take place much more evidently than they ever are in the healthy state, proving that this is one very important mode in which new cells are formed and the old ones removed from the tissue.

The most important uses of articular cartilages are mechanical. They constitute a beautifully smooth and highly elastic covering for the ends of the bones, thus facilitating motion, and decomposing a large portion of the forces, which would have produced repeated fractures, had they been applied directly to the bones. But, as these textures are subjected to great friction, they would wear away gradually, but for the constancy of their nutritive changes, which are sufficient to prevent this, as is shown in the fact that the cartilages are not thinner in individuals, or in particular limbs that are almost unceasingly employed, than in others; and that, though the cartilages are a little thinner in old than in young persons, the difference has never been shown to result from friction, nor to exceed in amount similar changes known to take place in other textures as age advances. Again, the elasticity of most, if not of all substances, is impaired by constant use, in which we see an additional necessity for the constantly recurring changes in articular cartilages. The fact being certain, that important nutritive changes do constantly take place in these textures, it is surely inconsistent with true pathology to conclude that they are not likewise subject to anormal nutritive changes, similar to those from which all other textures are known to suffer. And yet, the argument can have no weight without the support of numerous and well-ascertained facts.

Mr Birkett,[1] following out Richet's view, has endeavoured to show that bone and cartilage in their relations to each other are similar to true skin and cuticle. To prove this, however, appears to me to require some explanation—why the cells of cartilage should so readily produce others, when no single epithelial cell can perform a similar function;—why articular cartilage is never reproduced, whilst cuticle and epithelium are replaced more readily than any other tissues in the body;—why, when there is a very obvious chemical difference in the deep and superficial layers of cuticle, no such difference can be shown in the parts of articular cartilage;—and why the deep cells of cartilage are arranged in perpendicular columns, and the superficial ones horizontally to the surface, if they merely constitute a simple epithelial layer.

It is important to notice that no nerves enter into cartilages, and that nothing is known of their existence in fibro-cartilages, in which we might have expected to have found them, in connection with the fibres. Such structures are not sensitive, as has been shown by numerous observations and experiments, and yet it is generally believed that, in disease, they cause such acute pain as exhausts the patient.

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[1] Guy's Hospital Reports, October, 1848.

I shall proceed to notice the affections to which articular cartilages are liable under the heads of Morbid Anatomy, Chemistry, Pathology, Diagnosis, and Treatment.

Morbid Anatomy.—Hypertrophy, or an actual increase in the substance of the cartilage, occurs occasionally, without any obvious change in the texture, though it is generally accompanied with a soft, spongy, and decidedly diseased condition. The elevation described as occurring in Case No. II., on the external articular surface of the left tibia, took place without any very material change in structure; whilst the thickening of the cartilage of the patella in the same joint evidently arose from softening and positive disease.

Softening is generally very evident to the naked eye. The texture of the part so diseased is pulpy, and resembles a portion of soft fibro-cartilage. For microscopic examination, sections are difficult to be obtained, and the part requires slight separation with needles, —the hyaline substance is dark and indistinctly granular or fibrous, —the cells are enlarged, and contain a number of other cells or nuclei;—in many parts, the dark fibrous mass, contains corpuscles or small cells, which appear to have been set free by the bursting of larger ones.

Atrophy is seen in old people, in whom it is found affecting a whole surface or particular parts. If affecting particular parts, there are generally traces of the former existence of other disease; but when it is thinner than usual over the whole surface, its structure appears healthy.

Ulceration is found involving whole surfaces, the cartilage being soft, velvetty and abraded;—more frequently, such changes are found in the central parts of the cartilages alone, the circumferential part being white, dense, and generally split by numerous small fissures;—or a small spot of the size of a split pea may be the only part affected, and may present an irregularly rounded and excavated spot, the margins of which are fringed by a multitude of projecting bands and fibres, the deepest part being also covered with similar processes. Again, limited depressions occasionally exist, and appear as smooth as if portions of cartilage had been scooped out:—in more active forms of disease, the cartilage is removed very rapidly, and, on examination, a few patches here and there may be the only traces of its former existence. Such patches are generally reddened; —their surface, though not perfectly smooth, may yet be glistening, and their general appearance is often such as might lead to the impression, that they are portions of tolerably healthy cartilage, which have been left after the absorption of the other parts. The whole surfaces entering into the formation of a joint, or particular parts of the same, are not unfrequently found covered with fibrous and projecting fringes in advanced life. These are best seen on immersing the surface in water, when the fibrous processes float out,

and may be carefully examined. They are of all sizes and lengths, attached at one end to the surface of the cartilage, and free at the other. They are found most frequently on the cartilages of the patella and trochlear surface of the femur, the substance of these cartilages, at the same time, being apparently healthy, as far as can be ascertained by the naked eye.

The process thus spoken of as ulceration may commence at any part of the cartilage,—on either of its surfaces, or in its very substance, though its favourite seat is the free surface, from which it gradually extends to the deeper parts, destroying them as it meets with them, and, at last, leaving the bone completely bare.

Whatever external appearance the disease presents, the structural changes are found to be similar, in all cases, on a microscopic examination. These consist of changes in the structure and arrangement of the cells, and of others in the hyaline or inter-cellular substance. In very active forms of disease, the changes in the inter-cellular substance are little observed, owing to its rapid disintegration, or to its absorption into the cells; whilst, in chronic cases, the splitting of the hyaline substance gives to the surface a character so peculiar and well-marked, that, after repeated microscopic examinations, it is possible to state, on a simple inspection, what changes the structure will be found to have undergone, when more minutely examined.

The first changes which can be noticed in the cells are their enlargement and irregular arrangement in the tissue. Some of the cells, in the foregoing observations, were found to measure $\frac{1}{100}$th of an inch in their long diameter, and others were five or six times their usual size, over large portions of many diseased surfaces. They become rounded, oval, or oblong, and very generally isolated and irregularly distributed, instead of being elongated and found in groups which form columns in the deeper parts. If only slightly enlarged, their nuclei remain in small number, and are irregular;—if considerably enlarged, their contents differ entirely from those of healthy cells, and consist of a mass of corpuscles, which are irregularly rounded, oblong or triangular, glistening on the surface, and varying in diameter from $\frac{1}{2000}$th to $\frac{1}{5000}$th of an inch, the larger ones being finely granular, or containing a small nucleolus—(Fig. I.) On tracing sections, containing such cells, towards the surface or diseased part, the cell-walls become indistinct,—patches of corpuscles are met with without cell-walls enclosing them, and the corpuscles are found at length freely mixed with the tissue—(Fig. XV.) These changes are best marked in the deep cells, though they occur also in the superficial ones. The cells of the free surface enlarge and become much further separated from each other, losing their regularity of arrangement. Their nuclei generally disappear, and the whole cell often becomes finely granular, and is reduced to a mere shred of membrane, which is gradually lost—(Figs. XI., XII., and XXV.) In some places the diseased cells enclose others, of about $\frac{1}{1400}$th of an inch in diameter, which are shortly released by the

bursting of the containing cells. These smaller cells are full of dark and irregular granules, $\frac{1}{15000}$th to $\frac{1}{3000}$th of an inch in diameter —(Figs. XIII. and XIV.) They are often found mixed with fibrous tissue, to which they give a dark and indistinct character, as they become gradually lost amongst its fibres—(Fig. VII.) The diseased cells, nuclei, or corpuscles, contain granules under many other circumstances. Indeed, the granules appear of all intermediate sizes up to $\frac{1}{2000}$th of an inch, when they are evidently formed of drops of oil, into which the nuclei have probably been converted.

When the disease advances with great rapidity, scarcely any changes can be seen but those in the cells. These become rapidly distended,—thin nuclei are converted into granules, and the cells afterwards form a number of cavities in the tissue, the walls of which appear finely mottled—(Figs. XIII. and XIV.)—or the cells burst upon the surface, causing it to present a series of cavities; and, having discharged their contents, these enter into the formation of a fibro-nucleated membrane, with nipple-like processes which fill up the opened cells—(Figs. XVII., XVIII., and XXXV.)

The alterations in the hyaline substance consist of a splitting into bands and fibres of all sizes, the smallest fibres being indistinguishable from those of white fibrous tissue. The bands constitute the processes projecting into the joint, loose at one end, and attached to the cartilage at the other. This change was noticed in all the preceding observations. It is not so often seen in acute as in chronic cases, probably because, in the former, the rapid enlargement of the cells destroys the hyaline substance before it has time to break up into bands and fibres; whilst, in the chronic disease, the structure is not always wholly removed, but converted into the fibrous membrane which covers the bone when ulcers have healed. Wherever the surface presents the velvetty appearance, this is found to be due to the splitting of the hyaline substance into fibres and bands. Amongst these, rounded gelatinous masses are at times found, and they are occasionally stuck upon the striated bands, and interrupt their continuity. They have every appearance of being the changed contents of cartilage cells—(Fig. X). The smaller bands are very transparent, and contain no traces of nuclei or cells,—the larger ones have, within them, all the forms of cells and nuclei, which are found deeper in the texture, the most advanced stages of disease in the cells being always found at the part split into fibres. In the parts wholly converted into small fibres, traces of the nuclei cells can generally be seen—(Figs. IX., XIX., XX., and XXVIII.), or there are small granular cells, as in Fig. VII. Where the bands are connected with the cartilage, enlarged cells or cavities are occasionally found (Fig. XXIV.) in a position which indicates that they are concerned in causing the splitting; and it is to be noticed, that when the hyaline substance of the original surface of the cartilage splits, its bands and fibres lie horizontally, but when that of the deeper parts undergoes a like change, the bands and fibres are always ver-

tical to the surface, the difference being evidently owing to the normal arrangement of the cells of these parts, whether the cells are or are not the immediate cause of the splitting.

The membrane covering the bone at the bottom of old ulcerations which have healed, and that which covers the free surface of cartilages in a chronic state of disease, are of the same character, the latter being only of more recent formation. They consist of dark and very small fibres running horizontally, no nuclei being seen in the parts of old formation until after the action of acetic acid—(Figs. XXI. and XXII.); whilst in other and more recently formed parts, the nuclei are very distinct—(Fig. XXIII.), and in still more recent ones, there is an indistinctly granular and fibrous mass, full of the corpuscles, discharged by the bursting of cartilage cells, and in various stages of elongation—(Figs. XVII. and XVIII.)

As long as the cartilage is the only tissue affected, no other structures are found than those which have been named, and no pus is formed. But, whenever the bone or synovial membrane becomes diseased, these parts throw out the usual exudation, in which new structures grow. Upon the synovial membrane, the exudation becomes organised into lymph or converted into pus, or it may be mixed with tubercular granular matter if it be in a scrofulous individual; in the tissue of the bone, plastic and fusiform cells and primitive filaments are formed in the exuded matters. In all such cases, the new elements just-named appear, and have a few changed cartilage cells mixed amongst them—(Fig. XXIX.)

Disease affecting the articular fibro-cartilages is of a character precisely similar to that before-named, whether the changes in the cells or in the fibrous mass be considered—(See Figs. XXVI., XXVII., and XXVIII.)

Chemistry.—During the occurrence of the above-named morphological changes, there are doubtless many important chemical ones. Thus, the fibres (Figs. VI. and IX.) formed from the hyaline substance, are at first rendered transparent, and then almost wholly dissolved, by the action of acetic acid, as is the case with white fibrous tissue, which is known to consist of gelatin, but cartilages contain chondrin. Now, when the complete conversion into fibres has not taken place, and granular cells are mixed with indistinct fibres, acetic acid has less effect in producing transparency and solution, the actual chemical composition of the part probably changing in a direct ratio with the changes of structure. Similar chemical and morphological changes are found to advance side by side in the conversion of fibrin into pyin, and this into gelatin, during the formation of white fibrous tissue from coagulated blood-plasma. The gelatinous masses, stuck upon the bands in some places, and loose in others, are also the changed contents of cartilage cells. In many places, the nuclei of cells appear to be resolved into fat; and it is curious to notice, that, in the greater number of places where granular cells are found, these are of the size of the compound gra-

nular or exudation cells found in diseased states of other tissues; and yet they are not newly formed cells, but those of cartilage changed in character. Another well-marked chemical action is shown in Observation IV., in the deposition of a number of masses of carbonate of lime in the very substance of the cartilage; and, should it prove that the crystals of oxalate of lime, found on the surface in this case, and in Observation II., were formed during life, the mode of production of the oxalic acid will constitute a very interesting and important object of research.

Pathology.—The essential nature of all the known diseases of articular cartilages, is shown by the preceding observations to be a change in the nutrition of their texture. That any one of these is produced mechanically cannot for a moment be maintained, after a careful examination of the texture of the diseased part. Even the fringed and flocculent appearance of cartilages in chronic disease, is always associated with marked changes in the cells and hyaline substance. And yet it is possible, that, in Observation IV., the greater amount of pressure thrown on one lower extremity than the other, was connected with the more extensive disease in the right knee-joint. That the disease depends on actions going on in the cartilage, and proper to it, is proved by its frequent occurrence without a vestige of disease in the synovial membrane or bone; and, that all the changes go on independently of any peculiar action in the blood-vessels, is shown by the appearance of the disease on the surface, in by far the greater number of cases, and at parts far removed from the vessels of the bone, or of the synovial or other membrane. Many authors get rid of the "fibrous degeneration of the cartilage," by assigning to it a nature altogether different from that of other processes in the same texture. That it differs thus, is the opinion of Mr Key, who was the first to maintain that ulceration of articular cartilages results from the absorbing action of a membrane produced on its surface: yet Mr K. does not hesitate to state boldly, that after inflammation caused by wounds, the cartilage is often found to be extensively destroyed and the bone laid bare, without any appearance of a membrane for the purpose of absorption. Yes, it is certain, that in many, if not in the majority of cases of ulceration of cartilages, the process commences without the presence of any membrane whatever; whilst, on the other hand, there can be little doubt that cartilages are very frequently covered by a thickened synovial, or a newly-formed vascular membrane, without any ulceration resulting.

That these processes do not *depend* on the conversion of the nuclei and hyaline substance into fat, is shown by such changes being only occasional ones, the whole substance of the cartilage being very frequently destroyed, without the production of fatty granules in a single cell. They can only be referred to an anormal nutrition of the texture, by which the cells become inordinately and imperfectly developed, and the hyaline substance is split into bands and fibres.

It will be seen that these statements require to be received as ultimate facts, and that, however inexplicable the nature of the process may appear beyond this, an anormal state of the nutrition of the part, gives us as perfect an explanation of the formation of the diseased textures before-named, as healthy nutrition does of the manner in which the texture is preserved in its normal state.

The causes of disease in articular cartilages, may be either local or general. At times, the cartilages of a great number of joints are found affected in the same individual; whilst, in others, a particular part of one cartilage in a single joint may be the only diseased spot. Injuries, as a blow or fall, may be the exciting causes, as well as a constitutional disposition to disease, the latter being the cause where a great number of joints are affected at the same time. Again, in many cases, the disease is secondary, and is the result of acute or chronic inflammation of the synovial membrane or bone, by which the circulation in these parts is accelerated, and their nutritive processes disordered. Should an anormal exudation take place, as in scrofulous inflammations and tubercular deposits, its ingredients become mixed with the products of the disease in the cartilages.

The disease may be acute or chronic. When acute, the changes in the cells are at times the only ones which can be observed, and the whole cartilages of a joint may be removed with extreme rapidity. In two cases, Mr Mayo saw the articular cartilages of the ankle completely removed within a month after an injury; and after the lapse of two months, in another case, he found that the cartilages had disappeared, and that the bones were joined by semi-transparent and organised lymph from $\frac{1}{6}$th to $\frac{1}{4}$th of an inch in thickness. Mr Lawrence,[1] relates the case of a patient, who died of phlebitis after blood-letting, in which he believes that complete destruction of the articular cartilages of the femur and tibia took place in four days. When the disease is chronic, it may last for years without producing any symptoms whatever; or, it may be the cause of the crackling and grating sometimes noticed in the joints of old people. In Observations I., II. and IV., the patients died of other affections without knowing that disease existed in the joints; and, in the last-named one, the nature of the changes was such, as to render it very probable that disease had existed for years, and had been cured, in many places, by the formation of the thin and translucent membrane covering the bone.

Now, if it be true, that disease having the same essential structural characters in all cases, at times goes on to the complete destruction of the texture, without the patient or his attendant being aware of its existence; and in others, causes such violent pain and hectic irritation in the system as to require amputation, the fact is of extreme importance, as, so far as I know, no similar occurrence takes place in any other texture whatever.

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[1] Lectures on Surgery; *Lancet*, 1829 and 1830, Vol. ii.

I shall not presume to deny, that uncomplicated disease of articular cartilages produces excessive pain, since this is stated very positively on the high authority of Sir B. Brodie, and by most surgical writers. And yet, on looking over the numerous cases which have been related by Sir B. Brodie and other authors, I find but few which will aid in the determination of the question. Uncomplicated disease of cartilage is of very rare occurrence, if we exclude the cases of fibrous degeneration; and carefully collected reports of numerous cases, with histological examinations, are necessary to establish a fact so decidedly at variance with everything we know of disease and its characters in other textures. This will appear more evident from the following considerations :—

1st. Nerves have never been found in cartilages, and there is not the slightest reason for believing that they contain any, for they give no evidence of sensibility in man or in animals on being exposed or sliced away. The cuticle, enamel, and the substance of the brain, are in a similar position. They contain no nerves, and give no indications of sensibility, when parts are removed, or otherwise destroyed. Moreover, it is well known to every pathologist, that an immense portion of the whole bulk of the cerebrum may be utterly destroyed by disease, without a single recognised symptom or other mark of the existence of the lesion. Hence the value of pain.

2nd. It is certain that disease of cartilages may go on for years, and even be cured by the efforts of nature, without the knowledge of the patient, and that the essential characters of such disease are similar to those of other diseases of the same tissue, which are supposed to be the cause of excessive and exhausting pain.

3rd. The symptoms supposed to indicate disease in the cartilages are so closely allied to those produced by affections of the bones, that a positive diagnosis is very difficult to make until the bone becomes exposed, and then the symptoms are, at least, as easily attributable to the disease in the bone as to that in the cartilage. Indeed, M. Richet, in the work before-named, when entirely ignorant of the real nature of the changes which take place in diseased articular cartilages, gives several good anatomical and other reasons why the symptoms supposed to arise from disease in them should be referred to the bones.

4th. Pus has never been shown to be formed from the substance of diseased articular cartilages, nor from effusions into their texture. At the time when pus is formed in joints, the pain becomes very severe, though there may have been but little previously, the very formation of pus indicating that other textures than the cartilage are affected, and that the pain proceeds from the disease in them.

After ulceration has gone on for a considerable time, the part may be healed without any evident exudation. The structure which is formed, is precisely of the same character as the fibro-nucleated membrane found on the surface of cartilages in a chronic state of disease, and it is probably formed in a similar way to the

latter, by such a new arrangement of the particles of the cartilage as converts it into fibrous tissue.

Diagnosis.—Little remains to be said on this head. It has been shown, that in cases of chronic disease affecting every cartilage in both knee-joints, there may not be a single indication of disease. It is generally believed, that ulceration of cartilages is indicated by pains, which are at first slight, and afterwards become excessively severe,—which affect a particular spot, deep in a joint, as well as remote parts of the limb, becoming more severe during the night, and always unattended by swelling for four or five weeks, or even for many months. These pains are always increased by motion of the joint and pressure of the articulating surfaces against each other. At last, the suffering becomes most excruciating, and is accompanied by painful spasms and startings of the limb during sleep; the patient's health and spirits fail, and amputation alone can save his life. Sir B. Brodie lays great stress on the character of the pains, and the exhaustion produced by them; on their gradual increase in intensity, and on the occurrence of pains in remote parts of the limb;—on the absence of swelling for a long time, and then on its peculiar character. But the symptoms of disease extending in the bone are so nearly similar, that it is doubtful if all the characters taken together will enable an accomplished surgeon to distinguish the two. It is not at all surprising that, when ulceration has laid the bone bare at a particular part, the symptoms should be so much aggravated, and pus formed, for this is just what we should expect from disease in the bone.

Treatment.—Without a correct diagnosis, the treatment must be empirical, to a certain extent; and directed to the relief of particular symptoms. All the diseases of cartilages result from a local or general disorder of the nutritive processes:—in the former case, there may be no indications of disease; and, in the latter, the bone and synovial membrane are either implicated from the first, or become so subsequently, and the treatment must vary accordingly. Sir B. Brodie places great reliance on caustic issues, and states, that they are of greater service in cases where the cartilages are ulcerated than in other diseases of the joints. When the disease is of a scrofulous or tubercular character, there can be no doubt that more benefit is to be expected from constitutional than from local treatment. For all such cases, the Ol. Jecoris Aselli is especially indicated, and local counter-irritation should be used at the same time, at a greater or less distance from the joint, according to the activity of the disease. It is very obvious that, whenever the cartilages of a joint have lost their smooth surfaces, perfect rest is essential for a cure; and it should be secured by bandaging, according to Mr Scott's plan, or by splints, &c. This is equally essential when anchylosis is expected; for nothing can be conceived so likely to produce destructive irritation as the moving of two roughened and highly sensitive surfaces of bone upon each other; nor is anything so likely to prevent the organiza-

tion of the lymph, which may be expected to be poured out in this stage of the disease.

Finally, I may state the following as the conclusions, which appear to be warrantable from the foregoing considerations :—

1*st*, That the whole of the known forms of disease in articular cartilages are connected with changes in the texture, which are essentially similar to each other.

2*nd*, That during the progress of these changes, the cells of the cartilage become enlarged, rounded, and filled with corpuscles, in lieu of healthy cells; bursting subsequently, and discharging their contents into the texture on the surface; whilst the hyaline substance splits into bands and fibres, the changed hyaline substance and the discharged corpuscles of the cells, afterwards forming, in many cases, a fibro-nucleated membrane on the surface of the diseased cartilage.

3*rd*, That these changes are alone referable to an anormal nutrition as their immediate cause, and, in no case, to mechanical or chemical actions, such as attrition or digestion in a diseased secretion.

4*th*, That most extensive disease may be going on in many joints at the same time, and may proceed to destroy the whole thickness of the cartilage in particular parts, without the patient's knowledge, and whilst he is engaged in an active occupation.

5*th*, That the disease commences most frequently upon the free surface; but it may proceed from the bone to affect the attached surface, or it may take place in the middle of the thickness of the cartilage.

6*th*, That it is, at least, very doubtful if the symptoms which are believed to indicate the existence of ulceration of articular cartilages, are not really dependent on a morbid change in the bone.

7*th*, That disease of the whole thickness of an articular cartilage, at particular parts, admits of a natural cure, by the formation of a fibro-nucleated membrane from the substance of the cartilage, without the occurrence of any new exudation.

ARTICLE IV.—*Practical Remarks on the Hooping-Cough, with the suggestion of a New Method of treating that Disease.* By EBEN WATSON, A.M., M.D., Member of the Faculty of Physicians and Surgeons of Glasgow.

(*Read before the Medical Society of Glasgow, on* 16*th October* 1849.)

FEW diseases of equally frequent occurrence have been consigned so long and so universally as hooping-cough to a routine and empirical treatment. Hence it is little to be wondered at that, seeing physicians could offer but slight opposition to its onward progress, the disease has come to be considered beyond the power of medicine,

and is generally allowed to go on without much interference, unless some grave affection of the head, the chest, or the abdomen, threatens to complicate the original malady, and bring the patient's life into imminent danger. Nor has this custom arisen from any paucity of vaunted remedies for the hooping-cough: witness the long list of external applications and internal specifics for the disease in daily use both among regular and irregular practitioners. It would, doubtless, be very improper to class all the ordinary modes of treating the hooping-cough in one and the same category; for, while some are actually pernicious, and others utterly destitute of efficacy, a few, and only a few, out of the many, have a beneficial tendency, when judiciously conducted. Yet even these can only moderate symptoms or remove complications, and thus preserve the patient in a state favourable to recovery, after the disease has run its natural course.

Perhaps the best method of treating the hooping-cough, in common use, is that of administering frequent small doses of some emetic medicine, such as ipecacuan, or the tartarized antimony, in order that the patient may be ready to vomit, whenever the paroxysm commences. Under this treatment, in most cases, the fits of coughing and hooping are not only shortened, but the system is kept so low that inflammatory complications are much less likely to occur. Nevertheless, these results, though good in themselves, do not amount even to an attempt at *curing* the hooping-cough, or checking its progress; there is, in fact, no attack made at the seat of the malady.

This remark is equally true of the employment of embrocations over the chest, so fashionable in this disease. They are only directed against the bronchitis or pneumonia, which frequently complicate the hooping-cough; and, in so far as they may assist in removing these co-existing pectoral affections, they are serviceable, but they can have little influence on the primary disease.

The various expectorants, too, and sedatives, in common use for the hooping-cough, can have no curative influence on it; while some of them are highly dangerous, especially in the case of young children, unless when administered with much caution. I allude to the prussic acid, belladonna, tobacco, opium, digitalis, nux vomica, &c., all of which, and many more, have been proclaimed *cures* for the hooping-cough. One is, I think, often at a loss to discover what originally suggested the use of such medicines in cases of this disease. Surely it was not a study of its pathology; for they are but ill calculated to prevent or remove any of the lesions usually found after death. There is, indeed, very little unanimity in the profession regarding the proximate cause or causes of the hooping-cough; but I do not think that its treatment has improved or kept pace with even such advance as has been made in our knowledge of its pathology. Nay, I can hardly believe that any of the ordinary modes of treating the disease has been founded on a theory of its

nature at all, or has aimed at removing the morbid state produced by its peculiar poison.

There are four different views of the nature of the hooping-cough, each of which has had its advocates. According to one, it depends on inflammation of the brain and its membranes, from which the inflammation extends to the respiratory surfaces; while another class of pathologists maintain exactly the opposite—viz., that the inflammation originates in the bronchial membrane, and extends rapidly to the brain and medulla oblongata. A third theory is, that, while the brain and medulla may be unaffected, the essence of the malady lies in a peculiar irritation of the trunks of the great nerves supplying the larynx, diaphragm, and stomach—viz., the pneumogastric and phrenic—without any bronchial inflammation necessarily existing at all. And the fourth theory is, that the poison of the hooping-cough first causes inflammation of the respiratory surfaces, and very speedily affects the nerves, so as to render the glottis peculiarly irritable.

This last-mentioned theory is substantially the one which I adopt, as being simplest, and nearest to the truth. I believe that in a case of uncomplicated hooping-cough the morbid poison first inflames the mucous membrane lining the pharynx and upper part of the larynx; that there the extreme branches of the sensory nerve, the superior laryngeal, become affected in some peculiar way, which I shall not attempt to explain; and that finally the motor nerve, the recurrent laryngeal, is excited into reflex action. Hence the ordinary progress of the symptoms, which first resemble those of a common cold, or slight bronchitis—differing, however, from the latter in the absence of any constitutional or stethoscopic indications of inflamed bronchi, as well as in the periodic character of the cough. Along with this symptom may also be ranked certain peculiar pains of the throat and neck, which are often complained of, and seem to indicate the effect of the poison on the sensory nerves of these parts. Then follows the distinctive symptom of the affection—viz., the hoop, or drawback—which is produced by the excitement of the inferior laryngeal nerve, and consequent spasm of the muscles of the glottis. Such are the symptoms which, in my opinion, are alone essential to the hooping-cough. Few cases of that disease, however, proceed far on their course without the occurrence of another symptom—viz., vomiting—during or towards the end of the paroxysms of coughing; which is doubtless caused by an extension of the morbid influence to the branches of the pneumogastric supplying the stomach.

I do not mean to extend my remarks at present to the complications of hooping-cough farther than to state it as my firm conviction, that all of them, whether affecting the head, the chest, or the abdomen, are consequences of the disease, and by no means part of it. Dr J. C. Williams has well remarked, that "any one who has witnessed the severe paroxysms of hooping-cough can scarcely wonder that it may produce, in the head, in the lungs, and in the abdo-

men, serious congestions, which previous tendencies, or additional exciting causes, may readily convert into inflammation and its consequences, hydrocephalus, pneumonia, and intestinal disease."

Viewing the hooping-cough, then, as essentially an inflammation of the pharyngo-laryngeal mucous membrane, with a peculiar nervous irritability of the glottis, I was naturally led to the opinion, that, if I could reduce either or both of these states, I should mitigate or even cure the disease; and I shall now, in a few words, explain the way by which I came to try, for that purpose, the topical application which I am about to recommend to the society.

Soon after the publication of Dr Horace Green's work on "Diseases of the Air-passages," I had several opportunities of putting to the test of experience his method of treating chronic laryngeal affections, viz., by touching the lining of the larynx with a solution of nitrate of silver. My trials fully confirmed his statement of the efficacy of the treatment referred to; and I soon found that I could with advantage carry out a similar practice in many other diseases, such as in ordinary acute bronchitis, in the intervals of asthma, and even with relief of the tickling cough in early phthisis. Having thus established to my satisfaction the efficacy of a topical application of caustic solution, in cases, not only of chronic disease of the larynx, but in all cases of inflammatory irritation of the glottis, I came to the conclusion that it might operate beneficially in the hooping-cough; and, after a pretty extensive trial, I have not been disappointed.

I had applied the solution to the interior of the adult larynx in very many cases; but, as hooping-cough generally occurs to young children, my first thought was, whether it were equally practicable to introduce the sponge into the glottis in their case, as in that of older persons. It is, however, a pretty well ascertained fact that the aperture of the glottis attains about two-thirds of its ultimate dimensions before puberty, and I found, on trial, that this was quite enough to admit with ease a small sponge, of the size of a horse's bean. I have in my possession two prepared larynges; one that of a female infant, ten months old, who died of hooping-cough, complicated with pneumonia, in which the aperture of the extended glottis measures 0·451 of an inch; the other that of a boy, seven years of age, who died of dysentery, and in it the aperture of the glottis, when at rest, measured 0·476 of an inch, but could be stretched to 0·552 of an inch. Whereas numerous measurements of the adult larynx, given in Müller's work on Physiology, and some made by myself, yield the following mean results :—

	Male Larynx.	Female Larynx.
Length of aperture in state of repose, ...	0·765 in.	0·555 in.
Ditto ditto in state of tension, ...	1·013 in.	0·696 in.

It will be observed that these measurements corroborate the accuracy of the general statement formerly made, that the aperture of

the glottis attains nearly two-thirds of its ultimate dimensions even in infancy. It is also worthy of remark, that the glottis of the infant was nearly as large as that of the boy at seven years of age, and that in fact it was larger, when considered in relation to its ultimate dimensions, and the sex of the child. The evident inference was, that the youth of the patient need in no case be an objection to the employment of the treatment in question.

The occurrence of the following accident afforded experimental proof of this conclusion:—A child, not a year old, was affected with scarlatina. The inflammation of the throat was pretty severe, and had apparently extended into the larynx, for the child had a constant tickling cough—the chest being free of disease. On one occasion, when touching this child's tonsils and pharynx with a solution of nitrate of silver, by means of a small sponge, attached to a bent whalebone—which, let me observe, is a much easier and quicker mode of applying it than by a hair pencil—the child struggled, and the sponge, without my intending it, slipped fairly down into the larynx. Symptoms of a sense of suffocation ensued, but passed away in a few seconds, and the patient was never afterwards harassed by the cough as it formerly had been. Encouraged by this somewhat unexpected result, I have applied a similar solution in a good many cases of children affected with the hooping-cough, and the following have been the results of the practice.

The first case, in which I attempted it, was that of a weakly boy, about eight years old, in whom the disease was already at its height. He had a severe paroxysm of coughing regularly every quarter of an hour; he was already much debilitated, and in constant danger of having some serious lesion produced in his delicate lungs. From the first time that I applied the solution to the glottis, the severity of the cough was mitigated, and after repeating the application every second day for about a week, he was found not to hoop at all. The boy then made a speedy and complete recovery of his ordinary strength under the use of the cod-liver oil.

The sister of this patient was also seriously affected by the same disease, and the application of a solution of caustic was likewise used in her case. But its effects were not so striking as in the other case, owing to the presence of lobular pneumonia of the posterior part of the left lung. Nevertheless, the paroxysms of the hooping-cough diminished in frequency and in violence under its use; and after the pneumonia had been subdued by pretty active measures, the child ultimately made a good recovery.

My third case was that of a boy, about six years of age, in whom the disease had nearly passed without much treatment of any kind having been employed; but he still was harassed with frequent and pretty violent fits of coughing; and when excited or frightened, he still had that mode of drawing in his breath, popularly known as "the drawback." Two or three applications of the solution to the glottis and larynx of this child were sufficient to accomplish a per-

fect cure of his symptoms; and a short sojourn in the country, with the use of a tonic, completed his restoration to health and vigour.

I have since treated, in a similar manner, several other cases of hooping-cough, with the details of which I need not occupy the time of the Society. The results are equally favourable with those already mentioned. None of my patients continued to hoop more than eight or ten days after the solution of caustic began to be applied to the glottis; and from the first of such applications, the mitigation of the symptoms was very marked. In some cases of children living in the same house with patients affected with hooping-cough, and who exhibited symptoms of the commencement of that disease, such as frequent fits of coughing, especially during the night, and when suddenly agitated, with absence of co-existing fever, and of the physical signs of bronchitis, &c., I have, by applying the solution of caustic to their larynges, succeeded in dissipating the symptoms alluded to, and restoring the children to health. I should be sorry to say positively that these symptoms, in all the cases referred to, would, if let alone, have issued in true hooping-cough; but it seems to me *probable* that in most of them they would; and it is certain that they existed in children who had not previously had hooping-cough, who were at the time exposed to its undoubted infection, and in whom the disease did not become formed. I am myself inclined to think that in these cases the disease was checked; but I can hardly expect that the reasoning, by which I have been led to this opinion, can be equally conclusive to others who have not had opportunities of judging of the instances referred to.

I have met with several cases, and have already given an example, in which the violence of the disease had been got over with the ordinary treatment, but the cough still continued. Such children are usually of nervous temperament, and in bad general health, and whenever they cry, a modified hoop is still heard. In all these cases the topical treatment is remarkably efficacious, as well as speedy, in its action, a few applications of the solution sufficing, in most instances, to effect a cure.

While thus stating my confidence in the topical plan of treating the hooping-cough, I ought to mention, that I invariably combine with it due attention to the patient's diet, and proper regulation of the functions of the alimentary canal. I also confine my patients strictly to one apartment, suitably heated and well ventilated, until the stage of hooping has been fairly overcome; and then, if necessary, I recommend change of air and the use of some tonic, generally the cod-liver oil. By pursuing such a mode of treatment, this disease, instead of extending with severity for from two to six months, need not last longer than as many weeks, and with no greater severity than that of a common catarrh.

The ordinary strength of the solution which I have used in the treatment of the hooping-cough, is gr. xv. of the nitrate of silver to an ounce of distilled water; but in some cases I have with advan-

tage increased it to ʒij. of the salt in the ounce of water. It should be applied every second day: at first to the pharynx only, and, as soon as the parts will bear it, to the glottis and interior of the larynx. In most children the sponge may be passed down to the glottis at the first application, and, at all events, this procedure need not be delayed beyond the next occasion.

The mode of making this application is sufficiently simple. With a bit of sponge sewed firmly on the end of a whalebone rod, shaped nearly like a male catheter, the operation may, in most cases, be performed with the greatest ease. The patient's head being steadied by an assistant, the tongue is held down by a teaspoon, and the sponge made to glide, on the surface of the epiglottis, down into, and through, the rima glottidis. A very slight and temporary sense of suffocation follows, and the child very soon forgets that anything has been done. The whalebone should be pretty strong, for it is apt to bend in the mouth, and, perhaps, from the tossing of the child, to pass into the œsophagus instead of into the larynx. It is right also to mention, that sometimes a slight amount of pressure is required on the glottis ere the sponge is allowed to pass down. This obviously happens from the valves closing, as in the act of swallowing, before the operator has interposed his sponge, and prevented their coming in contact. The difficulty generally occurs in those cases in which the glottis is placed unusually low in the larynx, and when this is known to be so, the tongue should be pulled well forward, and an opportunity being watched for, when the patient coughs or draws in his breath, by a quick movement of the operator's hand the sponge should be suddenly passed into the larynx.

By thus taking the parts by surprise, he will often accomplish his purpose with ease; though sometimes he will still find the glottis shut against him, and in these cases a little gentle pressure will generally overcome the obstacle. It does require some tact and experience to know what degree of pressure the glottis will bear without injury; but, that it will bear a certain amount of pressure with impunity, every one is aware who has tried to any extent this method of treating laryngeal disease. I must remark, however, that I have not found this practice of gentle pressure on the glottis by any means so frequently required in children as in adults. The former generally become frightened, gasp, and cry freely, so that, in them, the aperture is found fully open, and the operator may easily and safely pass his sponge into the larynx.

Some, indeed, not questioning the possibility of applying such solutions to the glottis and larynx, may deny the propriety of their use in the case of organs so irritable, and at the very time in a state, if not of acute inflammation, at least of one bordering very closely upon it. But every one admits the advantage of using similar solutions to no less delicate an organ than the eye when affected with the most acute inflammation; and the results of a few trials will convince any one, that, in irritable and inflammatory states of the

larynx, a similar treatment may be pursued with equal safety and success.

In conclusion, I have great pleasure in being able to adduce, in favour of the method of treating the hooping-cough, which I have suggested in this brief paper, the testimony of my father, who is well known to the members of this Society for caution and accuracy. He has pursued it, at my recommendation, in several cases, and authorises me to state, that he now prefers it to any other mode of treating the disease, and that he believes it capable of very greatly shortening, and probably of checking, the progress of the malady, while it almost at once diminishes its severity. I hope, then, that the members of the Society will give a fair trial to this topical treatment in cases of hooping-cough;—a treatment which can do no harm, and promises to do very much good.

Postscript.—After I had read the foregoing paper to the Society, I was agreeably surprised to hear Dr Hislop, of Govan, state that he had already made trial of my plan of treating the hooping-cough, and that the result was highly favourable to it. He had heard of my mode of treatment from one of my own patients, whose children, but a short time before, had strikingly illustrated its efficacy, and who had been temporarily removed from town to the neighbourhood of Govan, for the sake of the country air during their convalescence. Happening, soon afterwards, to call my father down to Govan in consultation, Dr Hislop inquired more particularly of him regarding the details of the method in question, and immediately thereafter, being about two months prior to the date of the following letter, written at my request, he commenced to treat his cases of hooping-cough according to my plan. His own letter will best tell the results of his experience. I make no apology for subjoining it, since it is not only highly interesting in itself, but it contains the testimony of, I believe, *the only person*, besides my father and myself, who has treated cases of hooping-cough to a conclusion in the manner described in my paper.

"GOVAN, 24th October 1849.

"MY DEAR SIR,—My experience of your new method of treating hooping-cough extends to some six or eight cases. I was first led to make inquiries about it by the prevalence of the disease in this locality during the past summer and autumn; and, although I was not without my misgivings, both as to the capability of applying the new remedy in the manner proposed, and as to its ultimate utility, yet *a trial of it* in these cases *has dispelled all dubiety*, and convinced me that you have added by far the most valuable agent to the therapeutics of uncomplicated pertussis.

"The first case in which I have tried the nitrate of silver to the larynx, was a very chronic one, and well calculated to test its efficacy:—A child, three years of age, seized early in the present year with pertussis, and, at this time, after a lapse of five months, still liable to a return of the cough on making the least exertion, or receiving any excitement. I continued the application of the solution (ten grains to the half ounce of water) for a succession of mornings,—at first to the back of the throat, but gradually wearing downwards to the glottis, until I managed, I believe, ultimately to completely insert the sponge. The cough gradually diminished in frequency, and *is now entirely removed.*

"In the second case, the patient had only been five weeks affected by the disease, was four years of age, robust, and with considerable mucous râles in the bronchi of both sides. The solution was applied *four times*. A vast diminution in the returns of the cough and the length of the back-draught took place, and the patient *is now rapidly getting quit* of the disease.

"In the remaining cases, the improvement that has followed the application of the caustic solution has been equally marked, particularly the diminishing the frequency, severity, and duration of the drawback; and in one of the cases, where the solution was more perseveringly applied, *a complete cure* has been *speedily* effected.

"In regard to the difficulties that have been foreshadowed in the way of *applying* the remedy, I conceive they have been much exaggerated, and that it will not be found more difficult than any similar operation on the neighbouring parts.—I am, &c.,

"P. H. Hislop.

"Dr E. Watson."

ARTICLE V.—*On the External Use of Acetic Acid in Preventing the Dissemination of Scarlatina.* By JOHN WEBSTER, M.D., F.R.S., Consulting Physician to St George and St James's Dispensary, &c.

SCARLATINA being universally acknowledged a contagious disease, it seems superfluous to allude particularly to the subject, or to bring forward any arguments in support of such a well-established proposition. Believing its infectious character, therefore, to be undoubted, I at once proceed to the chief object proposed in the present communication, which is simply to bring before the profession the efficacy of diluted acetic acid, when applied externally—in conjunction, of course, with ordinary treatment—as an efficacious remedy in preventing this very frequently serious eruptive malady being disseminated amongst other individuals. According to my experience, now of considerable extent, the antiseptic action of vinegar, thus used, is very decided; and, having employed it as an external application during many years, I can consequently speak with some confidence respecting its influence. Indeed, the instances which have come under my immediate observation, wherein the disease did not spread to other members of the same establishment, placed under apparently similar circumstances, and in which the remedy was applied, are so numerous and conclusive, that I hesitate not to recommend practitioners to employ the frequent sponging of their patient with tepid vinegar and water, more especially in the early stages of scarlatina, and whenever the skin is hot, or the pulse accelerated; because I confidently expect the practice, in most cases, will be then found eminently successful. I would, however, premise, that the treatment now recommended is by no means considered as novel; and the following four cases are merely reported to illustrate the doctrine

advanced; trusting that, in other hands, as in mine, the results will prove equally satisfactory.

CASE I.—Many years ago, I was requested to attend a young gentleman, then on a visit to a nobleman resident in London, who was attacked by scarlatina. The symptoms were unequivocal as to the nature of the disease; the eruption was copious, and the malady exhibited a decidedly inflammatory type; the patient having previously enjoyed good health. In this case the general treatment was strictly antiphlogistic—which it is, however, superfluous to detail—and in due time the patient recovered. From the first appearance of the eruption, tepid vinegar and water, copiously diluted, was frequently applied to the skin. Not only the face, temples, hands, and arms, were thus very often moistened, but the entire surface of the body was repeatedly sponged with the same application. In fact, from the constant employment of this local remedy, the increased heat of the skin was moderated, and the greatly accelerated pulse diminished in frequency; whilst the patient may be almost said to have constantly respired an atmosphere loaded with the vapour of acetic acid, for several days consecutively. Every precaution was, of course, taken to prevent the disease being communicated to other members of the establishment, by keeping the patient always in the same apartment, until every trace of the malady had vanished; by not allowing any visitor to have access to his room, and by restricting all attendance to only two domestics, who, however, mixed with the other servants of the house—at that time numerous. Although much company visited the mansion of this noble peer—alike distinguished for his hospitality, as the liberal use he made of his large revenue—during the time scarlatina affected the inmate now alluded to, no other member of his Lordship's large establishment ever exhibited the slightest symptom of the malady in question; which was entirely confined to the individual whose case has been just briefly reported.

CASE II.—During the prevalence of epidemic scarlatina, some years ago, I professionally attended the daughter of a respectable tradesman in Regent Street, who then laboured under this complaint; the disease being at the same time common in the neighbourhood. There was no mistake regarding the nature of the patient's malady, all the symptoms being very distinct, and of a decidedly inflammatory diathesis. In this case, the rule I always advise respecting isolation, was strictly adopted; and the application of tepid diluted acetic acid to the surface was assiduously continued; the general treatment being at the same time regulated according to the symptoms and the stage of the disease. Notwithstanding the number of persons comprising this family, as also those engaged daily in the shop, and the workmen who pursued their ordinary avocations in the lower part of the premises, as previously—besides the fact, that the house was frequented by many customers during the day-time—no other case of scarlatina occurred; although the complaint, as already stated, had then become very common in the metropolis.

CASE III.—The subject of this case was the son of a tradesman, living in Holles Street, Cavendish Square. He was the eldest of the family, and had two younger sisters, then resident with their parents in the same house; besides which, there were also several domestics, as well as workmen, on the same premises; and, during the day-time, customers and others often visited the shop. Frequent sponging with diluted acetic acid, was employed in this example; the patient was kept always in the same apartment during the eruptive stages of the disease, and none but those absolutely required, were allowed to come in contact with him. In this case, the symptoms proved so severe, that one night I feared my patient would have sunk under the violence of his malady. However, from the treatment pursued, as well as from his good constitution, and

the anxious care of his parent and attendant, the lad fortunately struggled through, and became ultimately convalescent. In this instance, as in those previously related, the disease did not spread to any other individual connected with the family; the scarlatina having been confined exclusively to the single example above described.

Case IV.—Whilst scarlatina was unusually prevalent during the current year, not only in the metropolis, but throughout England, I was requested to visit the young son of a gentleman resident in the neighbourhood of London, who was then labouring under a well-marked form of this virulent disease. The eruption came out most copiously; indeed, it was more severe than I had often observed hitherto, whilst the other symptoms were distinct and unequivocal. Isolation, frequent sponging with tepid diluted acetic acid, and the rules usually recommended in similar cases, were rigidly followed in this individual; whilst the general treatment pursued was regulated by the nature of the symptoms, and the diathesis of the disease. Considering it superfluous to detail the curative measures employed in this, as in the other cases reported, I have purposely avoided describing the general treatment; the sole object proposed at present being to bring forward examples of scarlatina, which did not spread to any other person placed under similar circumstances, and where diluted acetic acid was had recourse to as an external application; at the same time, that strict isolation had been enforced as much as possible. I must, however, remark, that in this instance, as in all the others now given, the patient ultimately got well, without the disease having shown itself in any other person belonging to the household, although it was numerous, and comprised children, and in-door as well as out-door servants.

Many similar examples, illustrating the beneficial effects of the practice here recommended, might be mentioned, were it necessary; but the cases already reported will suffice to show the benefits supervening upon such a mode of treatment. I might even say, after considerable experience respecting the efficacy of this local remedy in scarlatina, that it seldom or ever fails to operate upon the system, not only as an efficient curative measure, but will prevent the diffusion of this common and frequently very dangerous epidemic malady. Knowing, however, the difficulty of acquiring sound practical knowledge, and how liable we sometimes are to draw general conclusions from a limited number of facts, which subsequent experience, and more extensive observation, especially on medical questions, may lead us ultimately to modify, I do not expect, from what has now been stated, that the reader will unhesitatingly coincide with me in opinion, that tepid diluted acetic acid, applied externally, will invariably prevent scarlatina from spreading; although I entertain little doubt respecting its efficacy, when used in the manner described. Nevertheless, should other practitioners think differently, as the remedy is easily employed, cannot act otherwise, in most cases, than beneficially, even as a curative agent, and also can never prove injurious, I would strenuously recommend the plan advocated to be tried, especially during the early stages of this epidemic disease, in order thereby to judge of its results fairly and impartially.

Part Second.

REVIEW.

On Stricture of the Urethra, and Fistula in Perineo. By JAMES SYME, F.R.S.E., &c. 8vo, pp. 72. Edinburgh, 1849.

ANY new proposal for the remedy of stricture of the urethra, supported by undoubted evidence of the efficacy of the treatment recommended, cannot fail to attract the attentive consideration of the practical surgeon. No one who has had much experience in the methods usually employed for the cure of the disease can have failed occasionally to have met with much annoyance and disappointment from the unsatisfactory results of treatment.

The variety of methods employed at different times for the remedy of stricture point to the difficulties connected with it. The means employed, even at the present day, are sufficiently numerous:—1st, Dilatation by bougies; 2d, dilatation by means of catheters retained in the bladder; 3d, the escharotic effects of caustic; 4th, internal incision, by sheathed blades passed through the stricture; 5th, incision of the perineum, in search of the urethra, deemed impermeable.

The obstinacy and fatal effects of the complaint are sufficiently evinced by the fact, that the existence of stricture of the urethra now frequently forms an insuperable barrier to effecting a policy of insurance on the lives of those who labour under the disease.

The method now proposed is not entirely new. It was communicated to the profession, by Mr Syme, in the pages of this Journal,[1] five years ago, and reprinted in his valuable work of last year.[2] The practice, however, notwithstanding the strong evidence adduced in its favour, seems not to have been followed by the profession—a fact which is probably to be attributed to the fear which generally exists of a permanently fistulous opening following incision of the urethra anterior to its membranous portion. We cannot doubt, however, that the publication of the present treatise, with the additional evidence which it contains of the safety and efficacy of the treatment, and of the permanency of the cure obtained by means of the external incision, will lead to the general adoption of the practice proposed by Mr Syme.

It is especially in the most inveterate and obstinate form of stricture that the treatment by external incision is demanded—those cases in which, independently of the mere tightness of the constriction, there exists a tendency to contraction, in spite of re-

[1] Number for October, 1844.
[2] Contributions to the Pathology and Practice of Surgery.

peated and successful attempts at dilatation by the bougie. Mr Syme, however, does not limit his recommendation of the external incision entirely to the treatment of the most severe forms of the complaint. The experience of a considerable number of cases has given him such confidence in the safety and efficacy of the practice, that he is inclined to extend its adoption to the treatment of cases which do not speedily yield to dilatation by bougies, or which still show a tendency to contraction after repeated dilatation.

The frequency of such cases, and the unsatisfactory results of the more ordinary methods of treatment, may be judged of from the following passage in Sir B. Brodie's lectures on diseases of the urinary organs :—

"In a few cases of incipient stricture, and in some of those in which a stricture is merely spasmodic, after a bougie has been used for a certain length of time, the use of it may be dispensed with, and there will be no recurrence of the disease. But these cases are rare exceptions to the general rule, which is, that there is danger of a relapse, and that a patient who is desirous of continuing well must submit to the occasional use of the bougie ever afterwards."

The following description of the operation is given by Mr Syme. The patient, if he wishes it, having been placed deeply under the influence of chloroform,

"He should then be brought to the edge of his bed, and have his limbs supported by two assistants, one of them standing on each side. A grooved director, slightly curved, and small enough to pass readily through the stricture, is next introduced, and confided to one of the assistants. The surgeon, sitting or kneeling on one knee, now makes an incision in the middle line of the perineum or penis, wherever the stricture is seated. It should be about an inch or inch and a-half in length, and extend through the integuments, together with the subjacent textures exterior to the urethra. The operator then, taking the handle of the director in his left, and the knife, which should be a small straight bistoury, in his right hand, feels, with his forefinger guarding the blade, for the director, and pushes the point into the groove behind, or on the bladder side of the stricture—runs the knife forwards, so as to divide the whole of the thickened texture at the contracted part of the canal, and withdraws the director. Finally, a No. 7 or 8 silver catheter is introduced into the bladder, and retained by a suitable arrangement of tapes, with a plug, to prevent trouble from the discharge of urine.

"The process having been thus completed, which it may be in less time than is required for reading its description, the patient has merely to remain quietly in bed for forty-eight hours, when the catheter should be withdrawn, and all restraint removed. The urine sometimes maintains it proper course from the first, but more frequently passes in part through the wound for some hours, or it may be a few days. No attention or interference is required on this account; but, at the end of eight or ten days, a moderate-sized bougie should be passed, and repeated once a week or fortnight for two months. In most cases the cure may then be deemed complete and lasting. But if the tendency to contraction should have been extreme, or if the patient's way of life should be such as to favour the reproduction of stricture, it will be a prudent precaution to have the bougie passed four or five times in the course of a year, in order to avoid all risk of future trouble."

The history of eleven cases, treated in this manner, are given by Mr Syme, to which more might be added, showing equally satisfac-

tory results. The prominent advantages of the operation, as deduced from the history of these cases, seem to be—1st, The speedy good effects resulting from the operation, and the apparent immunity from the rigors and succeeding severe constitutional disturbance from which those treated by the bougie so frequently suffer. 2d, The safety with which the incision may apparently be practised. Of all the cases operated on, one only had been followed by any unpleasant result. From an attack of erysipelas, consequent on the operation, the cicatrisation of the wound had been slow and imperfect, so that the urethra had a very thin covering at the seat of the aperture, which, therefore, was apt to open from time to time, and discharge a little urine. Lastly, the permanency of the cure. On this, the most important point, it may be said that sufficient time has not elapsed to prove the lasting benefit of the operation. In Case I., however, six years have passed, and the patient has remained perfectly well. In the other cases related, the time which has elapsed since the performance of the operation ranges from three months to between two and three years, and in no instance has there been any return of the complaint. We have watched some of the cases with attention, and have satisfied ourselves as to the distant results of the operation being all that could be desired. In one of the cases, which we have had under observation, the patient, though much exposed to vicissitudes of weather, and habitually intemperate, has not had the slightest return of his urinary complaints, from which he had suffered severely for several years previously.

To the description of the above method of treating stricture of the urethra are appended a few practical remarks on the pathology and treatment of *Fistula in Perineo*. The chief object of these observations is to impress on the profession correct views as to the formation and method of treating urinary abscess, on which considerable misconception seems to prevail. Attention is particularly directed to the contracted state of the urethra, on which the perineal fistula usually depends; and the applicability of the perineal incision to those cases which resist the ordinary treatment by dilatation, is pointed out and enforced, by the history of one or two cases, which have at once yielded on division of the stricture by external incision. The practical remarks bearing on the subject of perineal fistula would alone render this short treatise well worthy of perusal.

As to the treatment of stricture by incision, we regard its introduction as a valuable contribution to practical surgery. From Mr Syme's repeated assurance of the safety and efficacy of the practice, as well as from the admirable results which have followed the performance of the operation, both in private cases and in hospital, we cannot doubt that its general adoption by the profession will not be much longer delayed. Assured ourselves of the practical benefit to be derived from the treatment advocated by our author,

we strongly recommend his treatise to the attentive consideration of the practical surgeon.

Human nature is not a little indebted to our excellent professor of Clinical Surgery; he has bettered its lot. The removal of diseased ankle-joints, without unnecessary sacrifices of life or limb; the treatment of fungus of the testicle; the excision of joints; the radical and speedy cure of bursal swellings of the wrist; the management of *gangræna senilis*;—these are a few of the points in which he has made permanent additions to his art, besides, as we well know, simplifying the principles of his science. We have now brought before our readers his mature views as to the treatment of stricture, in which he seems to us to have made a signal improvement. To have done so, is in itself an unspeakable benefit, as every one who has seen much of this afflicting malady can testify. But when to this are added the many other claims of Mr Syme to the gratitude of our age and country, we need not hesitate to rank him among its most distinguished benefactors, as he has long been considered one of its most accomplished surgeons.

Part Third.

MEDICAL NEWS.

THE PROPOSED ALTERATIONS IN THE EDINBURGH INFIRMARY.

[We publish the following Communication from a Correspondent, without making ourselves responsible for the views expressed. It is difficult to see how the demand of the Faculty can be conceded in its full extent, without compromising seriously the interests of the other medical officers, who cannot and ought not to be neglected by the Managers. At the same time, the discretionary power claimed by the Faculty in allotting and disposing of the beds committed to their charge, is a privilege which, if granted, would lead to most important improvements in Clinical instruction.]

For some time past, the Medical Faculty has been anxious to increase the advantages hitherto enjoyed by the student in that important department of his education which is obtained in the wards of an hospital. It is also desirous of securing to such of its members as wish to do so, the right of practising in a public institution, and thereby obtaining that kind of knowledge without which the most elementary and theoretical branches of medicine cannot be properly taught. Indeed, what has long distinguished the British medical schools is their eminently practical character: a circumstance which has resulted from the fact, that even the professors of theoretical branches have generally in this country been able practitioners. Surely it cannot be maintained that the Monroes, the Hunters, and the Bells, were bad anatomists and physiologists, because they were in the habit of treating disease, or that, as a general rule, an acquaintance with medicine as a science renders a man incapable of prosecuting it as an art. We maintain the very opposite of these propositions. We believe that theory and practice mutually illustrate each other; that he is most scientific, who, while able to generalise facts, can put principles into operation; and that he is most practical, who, being well acquainted with the state of theory, knows where to stop in the application of it.

Edinburgh, however, possesses only one hospital, and the best method of carrying out the improvements intended by the Faculty, would undoubtedly be the establishment of another. If a sum of six or seven thousand pounds could be collected for the purpose of erecting a plain substantial building, capable of holding 150 beds, these, in conjunction with out-patients, would be amply sufficient. Or, if the sum left in trust to the Faculty of Advocates by Mr Chalmers, for endowing an hospital for the sick and hurt (amounting to about L.40,000), could be made available, the two faculties might build and maintain a very handsome structure. When it is considered how the city of Edinburgh has increased in extent and population since the present infirmary was built—that a modern hospital is much required—that such an institution would secure able physicians and surgeons in the members of the Medical Faculty, and that it would greatly add to the efficiency of the university, there is every reason for supposing that such subscriptions as might be necessary, could be obtained without any great difficulty. Indeed, we feel satisfied that such an undertaking, headed by the University, and supported by the annual contributions of the students' fees (amounting to L.1500 or L.2000 per annum), would meet with very cordial support, if the pressing reasons which demand it were publicly made known. On the other hand, it has been imagined, that if the managers of the infirmary would consent to certain modifications in their present system, the wishes of the Medical Faculty would be accomplished more speedily, and with less trouble and cost. In this case the students' fees, as heretofore, would remain at the disposal of the infirmary managers.

In accordance with this last supposition, the Medical Faculty of the university have proposed to the managers of the infirmary, that 150 beds shall, under the general approval of the latter, be set aside, so as to constitute a Faculty department of that institution. It is considered that this number, in conjunction with out-patients, would furnish sufficient illustration for carrying on three cliniques, Medical, Surgical, and Obstetrical, besides providing separate wards for the treatment of cutaneous, syphilitic, ophthalmic, and children's diseases. These beds are to be arranged, as the faculty may deem expedient, and entrusted to such of its members as in its opinion are able and willing to undertake the charge.

This plan has the merit of in noway interfering with the other medical officers in the house, and will enable the faculty to give a few beds to the Professors of anatomy and surgery ; the first of whom has been, and the latter soon will be, shut out from hospital practice. It must be acknowledged that it is of great importance to the interests of medical education, that these Professors should have this advantage. It is enjoyed by the teachers of anatomy and surgery in other large schools, and what reasonable objection can be urged to their possessing it in Edinburgh ? We understand, however, that the assistant-surgeons of the Infirmary believe that this arrangement will, in someway or other, interfere with their prospects. We are satisfied that such notions are altogether futile, and that both parties could obtain their wishes. At all events, it is clear that the Professors of anatomy and surgery ought not to be excluded from Hospital practice ; and it is equally apparent that, if this point be conceded, minor difficulties can easily be overcome.

The plan proposed by the Faculty, is, as a whole, comprehensive, and well calculated to benefit the students of Edinburgh, advance the interests of the medical school generally, and support the reputation of the infirmary. If consented to, it will unite parties that have long been opposed, reunite conflicting interests, allay certain irritations that have been excited, and enable the members of the university, and the managers of the infirmary, cordially to unite in supporting both institutions. It interferes in a very trifling degree, if at all, with pre-existing arrangements, and with the rights of the other corporate medical bodies. It is therefore to be desired that the managers will regard the plan as a whole, and consider the principles it involves, uninfluenced by individual interests on the one hand, or apparent difficulties on the other.

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